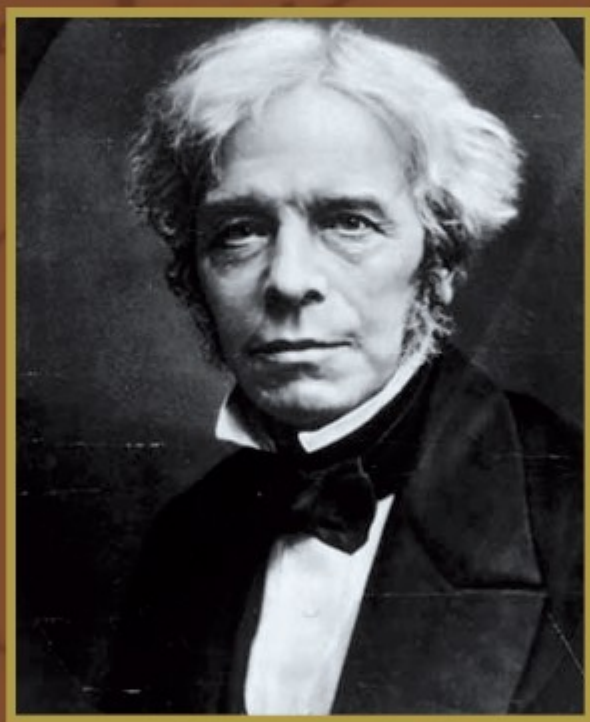


The Correspondence of MICHAEL FARADAY

Volume 5, 1855 – 1860



Edited by
Frank A J L James

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MICHAEL FARADAY**

Volume 5



Plate 1. Michael Faraday. Photograph by Maull and Polyblank, published in October 1857.

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November 1855–October 1860

Letters 3033–3873

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To Geoffrey Cantor and David Gooding:
two fellow Faraday enthusiasts.

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Editorial Procedures and Abbreviations

All letters to and from Faraday that have been located in either manuscript or in printed form have been included in chronological order of writing. The term letter has been broadly construed to include not only extracts from letters where only these have survived but also reports on various matters which Faraday submitted to institutions or individuals. What has not been included are scientific papers written in the form of a letter, although letters which were deemed worthy of publication, *subsequent* to their writing, are included, as are letters to journals, newspapers and so on. Letters that exist only in printed paraphrase form have not been included. Letters between members of Faraday's family, of which there are relatively few, are included as a matter of course. Of letters between other third parties, only those which had a direct effect on Faraday's career or life are included; the large number of letters which simply say what an excellent lecturer, chemist, philosopher, man etc. Faraday was, or the letters that are critical of him, are not included.

The aim has been to reproduce, as accurately as the conventions of typesetting will allow, the text of the letters as they were written. The only exceptions are that continuation words from one page to the next have not been transcribed and, as it proved impossible to render into consistent typeset form the various contractions with which Faraday and his correspondents tended to terminate their letters, all the endings of letters are spelt out in full irrespective of whether they were contracted or not. Crossings out have not been transcribed, although major alterations are given in the notes.

It should be stressed that the reliability of the texts of letters found only in printed form leaves a great deal to be desired, as a comparison of any letter in Bence Jones (1870a, b) with the original manuscript, where it has been found, will reveal. The punctuation and spelling of letters derived from printed sources has been retained.

Like Volume 4, this volume contains a number of letters between Faraday and John Tyndall. Most of these letters now only exist in the form of typescripts prepared by Tyndall's widow, Louise (1845–1940), as part of her project to write a life of her husband, which was never completed. Only

a very few original manuscripts of these letters have been found. Obvious minor typographical errors (for instance 'thw' for 'the') have been silently corrected. Otherwise the same editorial policy has been adopted for these typescripts as for the rest of the correspondence.

The only calendar difference in this volume reflects the fact that by the middle of the nineteenth century all of Europe apart from the Russian Empire had adopted the new-style calendar. In this volume only letter 3103 is dated in the old-style calendar (by this time twelve days adrift from the new style) in use in Russia until 1917.

Virtually all of the letters that Faraday received from the Board of Trade have been found only in the form of press copies retained by the Board which are now in The National Archives. The original letters would have included a printed heading (giving the address of the Board), salutation and a phrase for the opening sentence. Judging by other letters found in the Board's papers, the opening sentence would have begun with something to the effect "I am commanded by my Lords of the Board of Trade to". Since there was some variation in the printed texts of these letters, no attempt has been made to suggest what the precise wording for any of the letters published here might have been.

Each letter commences with a heading that gives the letter number, followed by the name of the writer and recipient, the date of the letter and its source. There is, occasionally, a fifth line in the heading in which is given the number that Faraday allotted to a letter. He only numbered letters to provide himself with a reminder of the order of a particular series of letters which usually referred to some matter of controversy; however, in this volume Faraday only numbers those letters dealing with the offer by Queen Victoria and his acceptance of the Grace and Favour house at Hampton Court. The postmark is only given when it is used to date a letter or to establish that the location of the writer was different from that of the letterhead.

The following symbols are used in the text of the letters:

[some text]	indicates that text has been interpolated
[word illegible]	indicates that it has not been possible to read a particular word (or words where indicated)
[MS torn]	indicates where part of the manuscript no longer exists (usually due to the seal of the letter being placed there) and that it has not been possible to reconstruct the text
<some text>	reconstructs the text where the manuscript has been torn
[sic]	indicates that the peculiar spelling or grammar in the text has been transcribed as it is in the manuscript. The use of this has been restricted as much as possible to rare cases. Hence, for example, the spelling of 'untill' is not followed by [sic].
[blank in MS]	indicates where part of the text was deliberately left blank

The following abbreviations are used in the texts of the letters:

BA	Batchelor of Arts
BM	British Museum
BS	Batchelor of Science
CB	Companion of the Order of the Bath
CE	Civil Engineer
DCL	Doctor of Civil Law [also given as LLD occasionally]
DS	Doctor of Science
DV	Deo volente [God willing]
FE	Friday Evening
FGS	Fellow of the Geological Society
FRS	Fellow of the Royal Society
HC	Hampton Court
HM	Her Majesty or Majesty's
MP	Member of Parliament
MS	Manuscript
NS	Nova Scotia
OB	Old Buckenham
OM	On Main
PRA	President of the Royal Academy
RA	Royal Artillery
RE	Royal Engineers
RI	Royal Institution
RM	Royal Military [Academy]
RN	Royal Navy
RS	Royal Society
TH	Trinity House
to	turn over

During 1857 postcodes using compass points such as W, SW as well as EC (East Central) and so on, started to be introduced in London by the Post Office.

Britain did not decimalise its currency until 1971 and is still half-heartedly trying to metricate its weights and measures, although in scientific and technical writings this latter has been largely completed. During the nineteenth century the main unit of currency was the pound (£) which was divided into twenty shillings (s) of twelve pennies (d) each. The penny was further subdivided into a half and a quarter (called a farthing). A sum such as, for example, one pound, three shillings and sixpence could be written as 1-3-6 with or without the symbols for the currency values. Likewise two shillings and six pence could be written as 2/6; this particular coin could be called half a crown. There was one additional unit of currency, the guinea, which was normally defined as twenty-one shillings. There is no agreed figure by which

the value of money in the nineteenth century can be multiplied to provide an indication of what its value would be now and, as this is one of the more contentious areas of economic history, no attempt will be made here to provide such a figure.

The following give conversion values for the units used in the correspondence as well as their value in modern units. For mass, only the Avoirdupois system is given, as that was most commonly used. But it is important to remember that both the Apothecaries' and Troy systems were also used to measure mass and that units in all these systems shared some of the same names, but different values. For conversion figures for these latter (and also for other units not given here) see Darton and Clark (1994).

Temperature

To convert degrees Fahrenheit (°F) to degrees Celsius (°C), subtract 32 and then multiply by 5/9.

Length

1 inch (in or ")			=	2.54 cm
1 hand (equestrian)	=	4 inches	=	10.16 cm
1 foot (ft or ')	=	12 inches	=	30.48 cm
1 yard (yd)	=	3 feet	=	91.44 cm
1 mile	=	1760 yards	=	1.6 km

Volume

1 cubic inch (ci)			=	16.38 cc
1 pint	=	4 gills	=	0.568 litres
1 gallon	=	8 pints	=	4.54 litres
1 bushel	=	8 gallons	=	36.3 litres

Mass

1 grain (gr)			=	0.065 g
1 ounce (oz)			=	28.3 g
1 pound (lb)	=	7000 grains		
	=	16 ounces	=	0.453 kg
1 stone	=	14 pounds	=	6.3 kg
1 hundredweight (cwt)	=	112 pounds	=	50.8 kg
1 ton	=	20 cwt	=	1.02 tonne

The Notes

The notes aim to identify, as far as has been possible, individuals, papers and books that are mentioned in the letters, and to explicate events to which reference is made (where this is not evident from the letters). In correspondence, writers when discussing individuals with titles used those titles, but as British biographical dictionaries use the family name this is given, where necessary, in the notes.

The biographical register identifies all those individuals who are mentioned in three or more letters (in either text or notes). The register provides a brief biographical description of these individuals and an indication of where further information may be found. No further identification of these individuals is given in the notes. Those who are mentioned in one or two letters are identified in the notes. While information contained in the genealogies of various Sandemanian families has been invaluable, this information has been checked against that available in the General Register Office (GRO) and Scottish Register Office (SRO). In these cases, and others, where the GRO or SRO is cited, the year of death is given followed by the age at death. If this agrees with information derived from other sources, then the year of birth is given in preference to the age.

References in the notes in the form of Faraday (1856a) refer to the bibliography, while the following abbreviations are used to cite other sources of information:

AC	Alumini Cantabrigienses
ACAB	Appletons' Cyclopedia of American Biography
ADB	Allgemeine Deutsche Biographie
AIKL	Allgemeines Künstler-Lexikon
ANB	American National Biography
ANF	Annuaire de la Noblesse de France
AO	Alumni Oxonienses
AsKL	Aschehougs Konversasjons Leksikon
AuDB	Australian Dictionary of Biography
Bx	Boase Modern English Biography, volume x
BJDN	Biographisches Jahrbuch und Deutscher Nekrolog
BNB	Biographie Nationale de Belgique
BWN	Biographisch Woordenboek der Nederlanden
CP	Complete Peerage
DAB	Dictionary of American Biography
DBE	Deutsche Biographische Enzyklopädie
DBF	Dictionnaire de Biographie Française
DHBS	Dictionnaire Historique et Biographique de la Suisse
DHS	Dictionnaire Historique de la Suisse
DSB	Dictionary of Scientific Biography
EB	Encyclopaedia Britannica, 11th edition
EI	Enciclopedia Italiana
ES	Entsiklopedicheskii Slovar
GDMM	Grove Dictionary of Music and Musicians
GRO	General Register Office
LGE	La Grande Encyclopédie
LUI	Lessico Universale Italiano
MN	Magyar Nagylexikon

NBL	Norsk Biografisk Leksikon
NBU	Nouvelle Biographie Universelle
NDB	Neue Deutsche Biographie
NNBW	Nieuw Nederlandsch Biografisch Woordenboek
NRAx	National Register of Archives report number x
OBL	Oesterreichisches Biographisches Lexikon
ODB	Oxford Dictionary of Byzantium
ODNB	Oxford Dictionary of National Biography
Px	Poggendorff Biographisch-Literarisches Handwörterbuch, volume x
POD	Post Office Directory (see below)
PRFDHC	Principal Registry of the Family Division of the High Court
RI MM	Greenaway <i>et al.</i> (1971–6). This is followed by date of meeting, volume and page number
RNL	Révai Nagy Lexikona
SRO	Scottish Register Office
WWWx	Who Was Who, volume x

Reports of lectures in the daily and weekly newspaper press and references to plays, poems and pieces of music are given only in the notes. From 1851 the Royal Institution published accounts of the Friday Evening Discourses in its *Proceedings*. These reports are listed in the bibliography and when cited in notes, indication is made that these were discourses. References to the *Gentlemen's Magazine* and the *Annual Register* (which has no volume number after 1862) are likewise given only in the notes. The following directories are cited in the notes:

Almanach Impérial
 Medical Directory
 Royal Kalendar
 Post Office Directory (POD)

Citations to these directories, unless otherwise indicated, refer to the edition of the year of the letter where the note occurs. These directories universally make the claim to contain up-to-date and complete information. This was frequently far from the case and this explains apparent discrepancies that occur.

Faraday's Diary. Being the various philosophical notes of experimental investigation made by Michael Faraday, DCL, FRS, during the years 1820–1862 and bequeathed by him to the Royal Institution of Great Britain. Now, by order of the Managers, printed and published for the first time, under the editorial supervision of Thomas Martin, 7 volumes and index, London, 1932–6 is cited as *Faraday, Diary*, date of entry, volume number and paragraph numbers, unless otherwise indicated.

Faraday's Experimental Researches in Electricity are cited in the normal way to the bibliography, but in this case the reference is followed by 'ERE'

and the series and paragraph numbers, unless otherwise indicated. Two of Faraday's papers (1852d, 1855b) were published in the *Philosophical Magazine*, as he regarded them as 'of a speculative and hypothetical nature' (1852d, p. 401), but in them he continued the paragraph numbering of his 'Experimental Researches' published in the *Philosophical Transactions*. To help locate references within these papers, I have allocated them the series numbers 29a and 29b respectively and they are thus cited in the notes in square brackets.

John Tyndall's manuscript diary in the archives of the Royal Institution (RI MS JT/2) is cited as Tyndall, *Diary*, followed by date, volume and page numbers. (Note: because at time of writing the manuscript collection of the Royal Institution was stored off site, references to Tyndall's diary in the introduction are given with the date only.)

The diary of Herbert McLeod, published as James (1987), is cited as McLeod, *Diary*, followed by the date of entry.

The diary of George Eliot, published as Harris and Johnston (1998), is cited as Eliot, *Diary*, followed by the date of entry and page number.

Manuscript abbreviations

The following are used to cite manuscript sources where the primary abbreviation is used twice or more. (NB Reference to material in private possession is always spelt out in full.) These abbreviations are used in both the letter headings and the notes:

APS	American Philosophical Society
AS MS	Académie des Sciences manuscript
BL add MS	British Library additional manuscript
BM CA	British Museum Central archives
Bod MS	Bodleian Library manuscript
BPRE MS	Biblioteca Panizzi, Reggio Emilia
BPUG MS	Bibliothèque Publique et Universitaire de Genève manuscript
BrUL MS	Bristol University manuscript
BS MS	Bayerische Staatsbibliothek MS
BUL	Boston University Library
DUA	Dundee University Archives
FACLM	Francis A. Countway Library of Medicine
FSL	Folger Shakespeare Library
GL MS	Guildhall Library manuscript
GS MS	Geological Society manuscript
IC MS	Imperial College manuscript
HP	Huxley papers
LP	Lyon Playfair papers
ICE MS	Institution of Civil Engineers manuscript

IET MS	Institution of Engineering and Technology (formerly the Institution of Electrical Engineers) manuscript
SC	Special collection
2	David James Blaikley collection
CPB	Common Place Book
3	S.P. Thompson collection
LU	Lehigh University
NG MS	National Gallery Manuscript
NLS	National Library of Scotland
MS	Manuscript
JMA	Archives of John Murray Ltd
NRCC ISTI	National Research Council Canada Institute for Scientific and Technology Information
NUL	Newcastle University Library
QUB MS	Queen's University Belfast manuscript
RAW	Royal Archives, Windsor
RCP	Royal College of Physicians
RGO	Royal Greenwich Observatory
6	Airy papers
RI MS	Royal Institution manuscript
F1	Faraday collection
A, D, E, G	Letters from Faraday
H, I	Faraday's portrait albums
L, N	Miscellaneous letters to and from Faraday
F2	Faraday's experimental notebooks
F3	Bound offprints of Faraday's papers
F4	Faraday's notes of lectures
F5	Apparatus books
G	Papers of W.R. Grove
GB	Guard Book
GM	Minutes of General Meeting
JT	Papers of John Tyndall
TS	Typescript volumes
Le	Lecture records
RI CG	Correspondence, general
RLSA	Redbridge Local Studies and Archives
RS MS	Royal Society manuscript
241	Faraday's diploma book
AP	Archived papers
Cert	Certificate of Fellow elected
CM	Council Minutes (printed)
CMB 90d	Minutes of Committee of Papers
MA	Meteorological archives
MC	Miscellaneous correspondence

PT	Manuscript of <i>Phil. Trans.</i> Papers
RR	Referee reports
RSC MS	Royal Society of Chemistry manuscript
SAU MS	St Andrews University manuscript
JDF	Papers of J.D. Forbes
SI	Smithsonian Institution Library
D MS	Dibner collection
SM MS	Science Museum Library manuscript
SuRO	Surrey Record Office
TCC MS	Trinity College Cambridge manuscript
TNA	The National Archives, Kew (formerly the Public Record Office)
ED28	Department of Science and Art records
HO107	1851 census
LC6	Records of levees
MT	Marine Department of the Board of Trade records
PRO30/69	Ramsay MacDonald papers
RG9	1861 census
RG10	1871 census
RG11	1881 census
RG12	1891 census
WORK19	Office of Works records relating to Royal Palaces
UB MS NS	Universität Basle Manuscript Nachlass Schoenbein
UCLA UL	University of California, Los Angeles, University Library
ULC	University Library Cambridge
Add MS 7342	Thomson papers
Add MS 7655	Maxwell papers
Add MS 7656	Stokes papers
UMO MS	University Museum Oxford manuscript
US MS	University of Southampton manuscript
UU	Uppsala University Handskriftsavdelningen
EW	Erik Waller's collection of autographs
WIHM MS	Wellcome Institute for the History of Medicine manuscript
FALF	Faraday autograph letter file
YUL	Yale University Library

Introduction

This volume publishes the correspondence of Michael Faraday during the five years from November 1855 to October 1860. Earlier volumes of Faraday's correspondence have illustrated that while he undertook his scientific research and work for the Royal Institution, he had another, concurrent, career, almost as time-consuming, in providing scientific advice for the state and its agencies. The 841 letters in this volume, of which just over 70 per cent are published for the first time, track a major shift in the balance of Faraday's work from a more or less equitable division between these three activities during the period from 1830 to 1855, to the subsequent dominance of his role as an adviser. This is perhaps best indicated by the fact that of the letters published in this volume just over a quarter relate to his work on lighthouses undertaken for Trinity House.

Despite such commitments, during this period, when Faraday was aged from 64 and 69, he nevertheless continued to maintain contact with many of his long-term Continental European friends, including Jean-Baptiste-André Dumas, Justus Liebig, August De La Rive, Julius Plücker and Christian Schoenbein. Likewise he continued to correspond with such British figures as Henry Bence Jones, John Barlow, Angela Burdett Coutts, William Snow Harris, James David Forbes, William Thomson, George Airy and George Stokes, while adding James Clerk Maxwell and Henry Roscoe to his circle of correspondents. However, the effects of age began to tell and are apparent in a marked reduction of letters with some of his long-standing correspondents, such as Charles Babbage, John Herschel¹ and William Whewell. Furthermore, death removed a number of individuals with whom Faraday had been on close terms including Jane Marcet (for whose obituary De La Rive asked Faraday how her work had influenced him²), William Buckland³, John Paris, Alexander von Humboldt, William Scoresby⁴, Charles Barry, William Somerville, Maria Herries, Isambard Brunel (though not before he had shown him over the SS *Great Eastern*⁵) and Emilie Schoenbein (discussed below). Among his fellow Sandemanians, the deaths that particularly touched Faraday were those of William Fisher⁶, Margaret Reid⁷, John Crichton⁸ and William Reid⁹, who tragically died from drowning. The loss of Crichton, Barry

and Somerville in quick succession in 1860 prompted a short meditation by Faraday on the subject of death¹⁰.

Faraday's primary concern remained the Royal Institution. The Duke of Northumberland was President and John Barlow was Secretary during the entire period covered by this volume. Both Northumberland and Barlow had been in office since the early 1840s and by early 1858 both were considering retiring from their posts. This prospect alarmed Faraday so much that he wrote to Northumberland saying that he thought it 'a very serious thing for the *President* & the *Secretary* to resign at the same time'¹¹. Evidently the subsequent negotiations were successful, from Faraday's point of view at least, since Barlow, with whom Faraday had a good relationship, remained Secretary for nearly three more years (succeeded by Bence Jones in December 1860) while Northumberland served as President until 1865.

In the five seasons of lectures covered by this volume Faraday delivered two Friday Evening Discourses each year (thus continuing the norm he had established during the early 1850s). Furthermore, he delivered all five series of Christmas lectures, again continuing the pattern of the first half of the decade. The last series, 1859–60, 'On the Various Forces of Matter' was published¹². This was the first time that Faraday had agreed to his lectures being published and represented a change of mind since he had refused permission the previous year¹³. These discourses and Christmas lectures were the only lectures that he delivered in these years¹⁴. The administration of the discourses was now largely undertaken by Barlow. Very occasionally, Faraday invited a member of the elite scientific community to deliver a discourse: for example, Thomson¹⁵, who accepted, and Airy, who declined¹⁶. Under Barlow's secretaryship, it had become usual for him to hold a dinner at his nearby house, 5 Berkeley Street, for the speaker before a discourse¹⁷. Faraday did not attend these dinners, but he then hosted the discourse at the Royal Institution unless he was absent owing to illness¹⁸.

In addition to his duties associated with the Royal Institution's lecture programme, Faraday, as Resident and Fullerian Professor of Chemistry remained, with Barlow, the key figure, responsible to the Managers, for running the Royal Institution¹⁹. For instance in 1856 he represented the request of the new Fullerian Professor of Physiology, Thomas Huxley, to the Managers for a natural history museum to be created in the Royal Institution. It is not clear what Faraday thought of this proposal, but the Managers rejected it out of hand²⁰). Furthermore, as superintendent of the house of the Royal Institution, Faraday remained responsible for overseeing necessary repairs²¹, including the drains²². In executing such duties he was hampered in 1859 by a strike of building workers²³.

The key professional relationship that centred on the Royal Institution was that between Faraday and John Tyndall, the Professor of Natural Philosophy since 1853. Although Faraday sometimes found Tyndall

exasperating²⁴, he supported Tyndall's ambitions within the Royal Institution; for instance, in his application to the Managers for a reduction in the number of afternoon lectures he delivered each year²⁵. On the other hand Faraday proceeded tactfully when dealing with the various scientific controversies that Tyndall became embroiled in. For example, he sought to make peace between Tyndall and Julius Plücker (a long-standing correspondent of Faraday's) in their argument over the magnecrystallic effect, in which Faraday actually disagreed with Tyndall's interpretation of the experimental results²⁶.

But their relationship went beyond the professional and was markedly personal. Thus on one occasion, in December 1855, while on their way to visit William Robert Grove, Faraday took a slight detour and showed Tyndall where he had been a bookbinder's apprentice in Blandford Street early in the century²⁷. Tyndall, who had boundless admiration for Faraday (indeed dedicated his first book, on glaciers, to him²⁸), was, however, careful not to appear too familiar and always addressed him as Mr. Faraday in his letters while Faraday, nearly 30 years his senior, addressed the younger man with the more familiar 'Tyndall'²⁹. Indeed Tyndall saw Faraday as a father figure and Sarah Faraday as a mother figure: 'They treated me like a son,' he wrote in his diary after visiting them one day at Highgate³⁰. His diary also frequently recorded visiting the Faradays' second-floor flat in the Royal Institution for a cup of tea³¹ or for dinner³². Tyndall also found the Faradays' joint niece and member of the Sandemanian church from 1857, Jane Barnard, who lived in the Royal Institution, 'very pretty'³³.

The closeness of Tyndall to the Faraday family is evinced by his long letters to them detailing his adventures and observations on his frequent visits to the Alps³⁴. But these visits were not just to admire the scenery and climb mountains (though Tyndall did both), but to study the nature of glaciers which Tyndall sought to understand in terms of molecular action. Here Faraday's work was again crucial to him since in 1850 Faraday had shown that two pieces of ice at freezing point placed in contact with each other would fuse together because the film of moisture between them would freeze³⁵. Tyndall seems to have coined the term 'regelation' to denote this phenomenon or at any rate its first published use was by Tyndall in a Friday Evening Discourse in 1857³⁶. He deployed the word to explain the motion of glaciers in terms of his reductionist molecular view of the world. His views brought him into conflict with another of Faraday's long-standing correspondents, James David Forbes³⁷. This interest in the phenomena prompted Faraday to include in his *Experimental Researches in Chemistry and Physics*, the fourth and final volume of his collected papers, published in the early spring of 1859³⁸, his original account first published in the *Athenaeum*³⁹. By this he acknowledged his authorship of the article, which had not been signed; furthermore, he included an entirely new, theoretical, piece on the subject dated September 1858⁴⁰.

Aside from his renewed interest in regelation and his continuing concern with electrical induction, Faraday's experimental research during the second half of the 1850s was dominated by three substantial projects: the transmission of light through gold, the electric discharge and the relationship of gravity with other forces.

Faraday possessed a long-standing interest in the relationship between light and matter⁴¹. In his afternoon lectures on the non-metallic elements, delivered in the spring of 1852, he noted that very thin gold leaf appeared green when white light was transmitted through it⁴². In the first of his Christmas lectures in 1855–6, on common metals, he again referred to this property of gold⁴³. What particularly interested him was how gold leaf, approximately 1/280,000 of an inch thick, could affect the passage of a ray of light where each vibration was between 1/59,570 (violet) and 1/37,640 (red) of an inch. Just over a month later he began an extensive series of experiments on the transmission of light through gold films and solutions. In total between 2 February 1856 and 20 December 1856, when he wrote his last entry, Faraday devoted 1161 paragraphs to this work which cover 244 pages of the printed diary⁴⁴.

From the start of this project Faraday was assisted by the wealthy amateur chemist and astronomer Warren De La Rue, who appears to have owned better microscopes than those at the Royal Institution⁴⁵. Some of Faraday's experiments were undertaken at De La Rue's house in Bunhill Row⁴⁶ and he also provided Faraday with information on gold beaters⁴⁷. It is possible that Faraday commenced work on gold as a result of a request by Anthony Panizzi to determine the best way of applying gold leaf to the ceiling of the British Museum's new Round Reading Room⁴⁸. The continuation of this link is confirmed by a letter from Sydney Smirke – the architect of the British Museum – who supplied Faraday with at least one sample of gold leaf⁴⁹ and Faraday informing Smirke on one occasion that he could not answer a question of his until he had seen De La Rue⁵⁰. Another connection with Faraday's practical concerns was that gold glass was red and therefore useful for lighthouse purposes, but there was always the problem that the absorption of light needed to produce a red colour seriously reduced the brightness of the light. Faraday referred obliquely to this connection at an early stage of his work⁵¹, and later compared red glass with some of the gold solutions that he had made⁵².

Faraday devoted part of a discourse half way through the project to 'some Observations on divided Gold'⁵³. By this time it was apparent that he was not making much progress with the original purpose of the investigation of examining the relationship of light and matter. However, he was undertaking some very interesting work on making thin films and gold solutions and in examining their properties, including of course their optical ones in which he speculated about the role of an aether⁵⁴. All this work resulted in his fifth and final Bakerian Lecture to the Royal Society which, published in the *Philosophical Transactions*⁵⁵, was his last paper to appear there⁵⁶.

Faraday's collaboration with De La Rue suggests that the material resources of the Royal Institution were beginning to prove inadequate to deal with new types of scientific research requiring specific instrumentation. This is further confirmed by his close collaboration between January and March 1858 with another wealthy amateur, John Peter Gassiot. Much of the research on the discharge of electricity through vacua seems to have been undertaken at Gassiot's house on Clapham Common. Faraday's notes on this topic, to which he accorded a separate sequence of paragraph numbers, cover 50 pages of his printed laboratory notebook⁵⁷. No paper or lecture by Faraday stemmed from this work, although Grove did deliver a discourse on it⁵⁸ and Gassiot gratefully acknowledged Faraday's aid⁵⁹.

The Managers of the Royal Institution began to address the apparent problems of the material resources of the laboratory in 1859 when they established the Holland Fund. This was named after Henry Holland who had offered to donate £40 a year for the remainder of his life towards the fund for the purchase of apparatus⁶⁰. However, this fund was merely the precursor to much more drastic changes in the direction of the Royal Institution which occurred in the early 1860s and will be covered in the next volume.

The third major piece of work that Faraday undertook in this period was his critique of gravitational theory and his attempt to link gravitation with other forms of force. As well as light, Faraday had a long-standing interest in gravity going back at least to the mid-1830s⁶¹. In 1849 and 1850 he tackled the problem experimentally and his negative results were published in the *Philosophical Transactions*⁶². Nevertheless, his belief in a direct link between electricity and gravity remained unshaken. In a Friday Evening Discourse entitled 'Conservation of Force', delivered on 27 February 1857⁶³, Faraday argued that the theory of gravitation as generally understood was incompatible with the law of the conservation of force. This lecture provoked considerable controversy and was reported and critiqued in places where natural philosophy was not usually discussed, for instance in *The Civil Engineer and Architect's Journal*⁶⁴. Herbert Spencer wrote a long, rather rambling letter criticising it⁶⁵, while Augustus De Morgan gave it a less than favourable review in the *Athenaeum*⁶⁶. But others, such as Rudolf Virchow⁶⁷ and, more interestingly, members of the Cambridge–North British school such as Forbes⁶⁸ and James Clerk Maxwell⁶⁹, a new correspondent, treated Faraday's views seriously and sympathetically.

Such support doubtless encouraged him to continue his experimental work and in 1859 he undertook another sustained series of experiments, mostly at the Shot Tower in Lambeth, seeking evidence of a direct relationship between gravity and electricity or heat. These experiments were recorded in 214 paragraphs, covering 47 printed pages of his laboratory notebook⁷⁰. In May or June 1860 he sent the resulting paper to the Royal Society. George Stokes, one of the society's secretaries, seems not to have treated this as a formal submission of a paper, but commented adversely on it to Faraday

who accepted his judgement and withdrew the paper⁷¹. It is worth noting that following this work and its rejection, he did not deliver a discourse on the subject which would have been his usual practice⁷².

Throughout Faraday's career there was a close, perhaps at times even symbiotic, relationship between his research and his deployment of science for practical purposes. In the period covered by this volume, this relationship underpinned Faraday's consultancy work, which can be divided into two parts. First, his provision of scientific advice relating to the conservation of artefacts and buildings; and second, his extensive work on lighthouses, which included the application of one of his fundamental scientific discoveries.

Faraday had a long-standing interest and involvement with conservation issues. For example in 1838 he had advised against moving the Raphael cartoons from Hampton Court Palace to central London⁷³ and had provided evidence on the conservation of paintings to the Select Committee on the National Gallery in 1853⁷⁴. As with so many other areas where scientific advice was needed in mid-Victorian Britain, Faraday was seen as possessing the necessary scientific expertise. Thus not only did he advise on the gold leaf of the Round Reading Room, the Trustees of the British Museum also asked him, a bit late in the day in his view, to advise on the general safety of the new building⁷⁵.

Faraday's earlier involvement with the issues surrounding the National Gallery doubtless contributed to his appointment, in early 1856, to the Royal Commission charged to consider the gallery's future⁷⁶. The main issue was whether it should remain in Trafalgar Square or be moved in part or in whole to other sites, especially South Kensington or the British Museum. The Royal Commission was chaired by Lord Broughton and its other members included Henry Milman and George Richmond while Henry Butler was Secretary. Under Henry Cole the South Kensington Museum, established following the success of the Great Exhibition in 1851, was especially keen to take over and relocate the National Gallery there⁷⁷. A key issue was that by continuing to locate the gallery in central London, its collections would be more accessible to the public, but more prone to damage by the atmosphere. Moving the collections, or part of them, to somewhere like South Kensington would, so the argument ran, reduce accessibility but also lessen damage. Hence Faraday's crucial role as a member of the Royal Commission was to assess the impact that the polluted atmosphere of central London had on works of art⁷⁸. He was a very active in questioning the witnesses who appeared before the Royal Commission and on 6 April 1857 had a short but sharp exchange with John Ruskin⁷⁹, during his evidence, over conservation and access issues⁸⁰. Although Ruskin had been an admirer of Faraday's and sought to be to art what he believed Faraday was to chemistry⁸¹, it would appear that Ruskin never forgave Faraday for the exchange, later subjecting him to some quite severe criticism⁸². On behalf of the Royal Commission⁸³, Faraday also examined the state of the Elgin Marbles in the British Museum,

which he had first inspected 20 years previously⁸⁴. His visit in 1857 resulted in a pretty damning report on their condition, written in a letter to Milman⁸⁵. The outcome of the Royal Commission was the recommendation that the National Gallery should remain in Trafalgar Square. Faraday abstained from voting for or against this proposal on the grounds that in his view the choice between preservation of the pictures and public access to them was too finely balanced for him to come to a conclusion⁸⁶. This was despite the commission having been presented with strong statistical evidence showing that location did not have an effect on attendance⁸⁷.

Faraday's involvement with the National Gallery continued in early 1858 when its Director, Charles Eastlake, asked him to investigate the effects of gaslight on paintings⁸⁸. This formed part of a long-running issue within the National Gallery and the following year it became part of the argument between the gallery and South Kensington. On 6 June 1859 the Department of Science and Art, which ran South Kensington, formed a commission, which included Faraday, Tyndall and August Hofmann, to examine the effects of gas lighting⁸⁹. This commission was established in the context of the need to move the Vernon and Turner collections of the National Gallery from their temporary location in Marlborough House because of the decision to establish the household of the Prince of Wales there⁹⁰. Because of shortage of space in the gallery's Trafalgar Square site, it had been agreed to display them in South Kensington⁹¹. The Department of Science and Art wanted to illuminate them by gas in order to extend opening hours and accessibility⁹². This proposal and the formation of the commission prompted a somewhat anxious letter from Eastlake to Faraday in mid-June asking about the effects of gas lighting⁹³. Faraday, evidently aware of the politics, replied cautiously⁹⁴ and sent Eastlake a copy of the report when it was published a month later⁹⁵. Although the report stated that it was safe to use gas to illuminate pictures, nevertheless the National Gallery did not install such lighting⁹⁶.

Faraday's other major involvement in conservation issues related to decay of the limestone of the relatively new Houses of Parliament. This decay, which was due to the sulphur content of the London atmosphere, was the cause of such significant embarrassment that the First Commissioners of the Board of Works (Henry Fitzroy and then, following a change of government, William Cowper) were personally involved. A number of contractors, including Nicholas Szerelmey, Frederick Ransome and John Daines, had proposed various treatments to the stone in order to consolidate and stabilise it. Beginning in about 1856 tests had been carried out to determine the effectiveness of these treatments, and in mid-1859, the Board of Works asked Faraday to adjudicate between these treatments. Faraday unambiguously reported that Szerelmey's method was by far the best⁹⁷. However, the board also asked Faraday to confirm that Ransome's process was inferior to Szerelmey's⁹⁸, which Faraday did but in terms that left some room for interpretation⁹⁹. There matters were left for some months while the government changed and it

was not until March 1860 that the new First Commissioner, Cowper, asked Faraday the same question¹⁰⁰. In his reply Faraday referred to his original answer, and described some observations that he and Roderick Murchison had subsequently made of the state of the stone. He concluded, evidently realising the potential pitfalls, by asking that his opinion should not be conveyed to either Szerelmey or Ransome because it was a trade matter¹⁰¹. Faraday was right to be cautious, since Ransome wrote to the board raising doubts about Szerelmey's process. Faraday responded by reaffirming his belief that Szerelmey's process was the best, but emphasised, in typical Faraday fashion, that the final outcome would only be known after many years of practice¹⁰².

In the arguments over the merits of Szerelmey's and Ransome's methods, Daines's work had been overlooked and Faraday was asked in May to comment on it, which he did, suggesting that it was not especially effective¹⁰³. Following publication of this, and other letters, in *Parliamentary Papers*, Daines visited Faraday who mistook him for someone else. When Daines threatened Faraday with legal action he ended the meeting¹⁰⁴, but no further steps appear to have been taken.

Such was the concern with the issues that Cowper wrote personally to Faraday for his views which, once again, he repeated¹⁰⁵. This whole experience, and especially the threat of legal action, led Faraday to write to Cowper that his willingness to undertake work for the government was cooling¹⁰⁶ and this may explain why Faraday was not called on for advice when there was a major enquiry the following year¹⁰⁷. The basic problem was that the stone had been badly quarried in the first place and no amount of remedial conservation would have been successful in arresting the decay; in the 1930s much of the original stone was replaced¹⁰⁸.

The amount of time and effort that Faraday devoted to the British Museum, National Gallery and the Houses of Parliament was small compared to his activities on behalf of the Corporation of the Elder Brethren of Trinity House, the English and Welsh lighthouse authority. Faraday had been appointed its scientific adviser in 1836¹⁰⁹ and letters published in earlier volumes have tracked his close involvement with the development of lighthouse technology, so crucial to the protection of shipping, lives and trade. Since 1836 Faraday had mainly dealt with the Secretary of Trinity House, Jacob Herbert, with whom he seems to have had a reasonable working relationship. In 1856 Herbert retired and was replaced by Peter Berthon, with whom Faraday also appeared to work well, although he did sometimes become irritated when responding to enquiries that he had already addressed.

Trinity House seems to have attributed three roles to Faraday. First, he was to undertake the mundane, but important, chemical work of analysis of fuel oil, lead paint and water. Second, he would act as a barrier between Trinity House and persistent inventors trying to foist their inventions on the lighthouse service. In this volume the main offender was William Fitzmaurice who had invented a form of limelight. His persistence in trying to persuade

Trinity House to use it irritated Faraday¹¹⁰ but it eventually paid off when Faraday agreed for it to be tested at the South Foreland lighthouse¹¹¹. Third, to actively provide advice on new developments in lighthouse technology, including the electric light and the improvement of lighthouse optical systems. Towards the end of the period covered by this volume Faraday was drawn into providing evidence for the Royal Commission established at the end of 1858 to examine the lighthouse service in Britain and the colonies. He had become directly involved in the latter following his appointment, early in 1856, at an annual salary of £100, as scientific adviser to the Board of Trade after it had taken over responsibility for the colonial lighthouse service¹¹² following the Merchant Shipping Act of 1854. In this guise Faraday advised on lighthouses as far afield as Vancouver¹¹³ and the Red Sea¹¹⁴.

In the early 1850s Faraday had examined John Watson's method of using electric light for lighthouse illumination. This method was based on passing an electric current from a battery through a carbon arc. Faraday ultimately rejected this proposal for a number of reasons and it was not even tested *in situ*¹¹⁵. He applied similar reasoning when Albert Way proposed his electric lamp in 1859¹¹⁶. In 1857 Frederick Holmes proposed another method of lighting lighthouses electrically. Instead of using batteries to power the carbon arc (which was the only method available to produce electric light), he proposed to use a magneto-electric generator based on the principles of electro-magnetic induction that Faraday had discovered in 1831.

Faraday's first report on Holmes's light, in April 1857, was fairly positive, particularly when he compared it with the light produced by batteries¹¹⁷. He followed this up with a series of questions which Holmes answered sufficiently satisfactorily that Faraday recommended that his method should be properly tested and to this Trinity House agreed to contribute towards the costs¹¹⁸. During 1858 Holmes installed his light in the South Foreland lighthouse. Owing to its relatively accessibility from London, this lighthouse was frequently used by Trinity House for experimental trials. Holmes's apparatus comprised a steam engine to drive the generator, which provided the electric power to the carbon arc in the lighthouse lantern. The constant distance separating the carbon poles of the arc (a crucial issue in this type of lighting) was maintained by a clockwork mechanism¹¹⁹. On 8 December 1858 this electric light first shone across the English Channel. Faraday was present and the following week attended a meeting at Trinity House where it was agreed to continue with the tests¹²⁰. Over the next few months various improvements were made to the system and on 28 March 1859 it was put into continuous use. Just under a month later Faraday visited the South Foreland for a couple of days. Sailing in the Trinity House yacht off the Kent coast, sometimes more than 20 miles from the lighthouse, he noted the behaviour of the light. He closely inspected the light and analysed data which had been collected by observers located at the Dungeness and Cap Grisnez lighthouses. Although Faraday discussed the merits and disadvantages of the light, he concluded

that, in his 'opinion, Professor Holmes has practically established the fitness and sufficiency of the magneto-electric light for lighthouse purposes'¹²¹.

The trial of the light continued until 7 May 1859 and was resumed between 22 August and 23 February 1860 when it was discontinued¹²². The reason for discontinuing the trial was that the lighthouse was required to test Fitzmaurice's experimental limelight later in the year¹²³. Just before the end of the trial Faraday tried to visit the South Foreland. On the first occasion, on 13 February 1860, he was snowed up and could not reach the lighthouse, but at the second attempt four days later, 'by climbing over hedges, walls, and fields' he was able to visit the lighthouse and subsequently produced another favourable report¹²⁴. After nearly a year of continuous service of the electric light, Faraday attended a meeting at Trinity House to discuss the results. There it was agreed that he would provide a written report recommending continuing the use of the magneto-electric light¹²⁵ and that it should now be tested in a revolving light¹²⁶. Such was Faraday's confidence in the electric light that the following month he delivered a discourse on the subject which was attended by nearly 800 people and attracted considerable interest¹²⁷.

The 1854 Merchant Shipping Act had theoretically placed the lighthouse service of Trinity House under the control of the Board of Trade and likewise with the Northern Lighthouse Board (covering Scotland) and the Dublin Ballast Board (Ireland). Although the corporation now reported to the Board of Trade, it is clear that in practice Trinity House retained considerable freedom of action. The establishment of the Royal Commission in early 1859 to enquire into lighthouses and other navigational aids can be read as a further attempt by the Board of Trade to increase its control over the lighthouse service, since this was one of the specific questions the commission was asked to consider¹²⁸. Its two-volume report, published in the spring of 1861 and running to nearly 1000 pages, provided a highly detailed snapshot of the British lighthouse system and those of other nations.

The Royal Commission spent most of its first two years gathering evidence by questionnaires and visiting lighthouses round the coast as well as in France and Spain. Indeed, it was not until November 1860, that it began to take formal oral evidence. In reviewing the use of electric light the commission received Faraday's report on the South Foreland trials¹²⁹. But the key issue for Trinity House, so far as Faraday's relationship with the Royal Commission was concerned, was the quality, or otherwise, of the apparatus used by Trinity House in its lighthouses, especially when compared with the French lighthouse service. To prepare themselves for the questions that were bound to be raised, Trinity House gathered its own evidence at the same time as the Royal Commission did. Thus in October 1859 Faraday joined some of the Elder Brethren in visiting a number of lighthouses in Northern France¹³⁰. On this occasion Faraday was violently seasick which led to some unpleasant external bleeding¹³¹.

Trinity House was right to be concerned about the approach the commission would take. They had secured the services of George Airy, the Astronomer Royal and thus a government employee, to provide them with scientific advice. In June 1860 he visited the Whitby lighthouses where 'he discovered a fault of a singular kind which materially diminished their power'¹³² and shortly afterwards he reported to the commission faults at other lights including that at North Foreland¹³³.

The commission sought to show the inadequacy of the corporation's provision of lighting apparatus and thus provide a further justification for the Board of Trade taking over its duties. That the Brethren of Trinity House were aware of this strategy is clear from a note that a highly indignant Berthon wrote to Faraday¹³⁴. Prior to visiting the North Foreland and Whitby lighthouses, a preliminary meeting was held between Trinity House, Faraday and the Royal Commission on 30 July 1860 at which Airy outlined the defects he had found¹³⁵. On 2 August these individuals, together with representatives from a wide range of lighthouse interests¹³⁶, visited North Foreland¹³⁷ and the following week Whitby was visited but without Airy¹³⁸. Highly technical issues of lighthouse optics, such as light sources, focal points and divergence were discussed, and, judging from Faraday's report of the visits¹³⁹ and from the Royal Commission report, Airy's criticisms were at least partially justified.

The optics of the Whitby light had been made by the Birmingham glass maker James Chance and he attended the visits to both the North Foreland and Whitby lighthouses. Faraday had had some dealings with Chance before and indeed had even visited his glassworks¹⁴⁰. However, following the site visits to North Foreland and Whitby, their correspondence increased as they sought to rectify the problems and thus defend both Chance's reputation for the construction of lighthouse optics and Trinity House's competence. A meeting, with Faraday present, was held at Trinity House at the end of August¹⁴¹ to discuss how to proceed and the following week an unminuted meeting at Trinity House led Faraday to write, on behalf of the corporation, a letter strongly supporting Chance¹⁴². An outcome of this was that Faraday visited Birmingham in mid-September to advise Chance and to learn about his progress¹⁴³. It was agreed that Chance would make alterations to the southern light at Whitby, but not to the northern one so that comparisons could be made. The alterations made by Chance at Whitby led to a second site visit by Trinity House, Faraday and the Royal Commission which lasted nearly a week from 9 to 15 October 1860. Chance's alterations, as the commission report acknowledged, had resulted in an improvement to the southern light which was now superior to the northern one, which had previously been the stronger of the two¹⁴⁴. Chance's work had been vindicated¹⁴⁵ and the commission's report acknowledged that defects in other lighthouses would be remedied¹⁴⁶. Although the Royal Commission did recommend the establishment of a central authority

for lighthouses¹⁴⁷, this was for administrative reasons rather than reasons of technical competence.

It can be argued that many aspects of Faraday's work, whether research or for the state, were underpinned by religious motivations. The purpose of bringing together knowledge of the world with power over that world was, in Faraday's words at a discourse in 1858 on the electric telegraph, to convey the 'gifts of God to man'¹⁴⁸. Related to Faraday's overt theological meaning was also a social meaning since, as he was well aware, the interaction of science and technology with society was a highly intricate one. This he made clear in the opening passage of his discourse on the electrification of lighthouses:

The use of light to guide the mariner as he approaches land, or passes through intricate channels, has, with the advance of society and its ever increasing interests, caused such a necessity for means more and more perfect, as to tax to the utmost the powers of both the philosopher and the practical man, in the development of the principles concerned, and their efficient application¹⁴⁹.

In this passage, Faraday encapsulated his belief of the moral responsibility that the practitioners of science and technology should have towards the demands of society and thus of the state and its agencies.

Yet, as with earlier volumes of the correspondence, Faraday's religious practices and beliefs are rarely made explicit¹⁵⁰. He would have attended church every Sunday as well as Wednesday afternoons and evenings¹⁵¹. His enforced absence led Faraday on one occasion to write a slightly stressed letter to an Elder, Benjamin Vincent, explaining that he had been detained in Whitby while visiting the lighthouses there¹⁵². It was probably the more stressed because Faraday was in line to take up, for the second time, the office of Elder which he had left following his exclusion from the church in 1844. However, on 21 October 1860 he did become an Elder of the church¹⁵³. As a member of the church, he visited Sandemanian communities round the country such as those at Old Buckenham near Norwich¹⁵⁴, Chesterfield¹⁵⁵ and Newcastle¹⁵⁶. In 1859 he visited the small community in Glasgow¹⁵⁷ and the large one in Dundee¹⁵⁸ on the way to attend the meeting of the British Association in Aberdeen. The Sandemanian church also entered his correspondence when the deaths of various members occurred, many of whom were also related to the Faradays¹⁵⁹.

Aside from his research and practical work, Faraday's religious beliefs and practices had a number of consequences which are illustrated in this volume. These included his continuing support of the London Orphan Asylum¹⁶⁰, his successful campaign to obtain a place for three-year-old Minnie Leighton in the Infant Orphan Asylum¹⁶¹, his distaste for table turners, and perhaps most revealingly of all, his attitude to the death of Emilie Schoenbein.

Although the controversy raised by Faraday's attack on table turning and spirit rappings had largely died down by the late 1850s, the telegraph

engineer Josiah Latimer Clark revived it and invited Faraday to witness the phenomena in late April 1857¹⁶². Faraday was clearly unwilling to accept the invitation and sent Tyndall instead¹⁶³. This worried Sarah Faraday who was vastly relieved when Tyndall told her over lunch the following day that he had not been persuaded by the demonstration as she had feared he might¹⁶⁴. Nevertheless, Clark continued attempts to attract Faraday's interest, but only drew a very sharp response from him¹⁶⁵.

More serious was Faraday's attitude to the death of Christian Schoenbein's eldest daughter Emilie. In a letter written at the end of November 1855, Schoenbein first raised the possibility that his 19-year-old daughter might make an extended visit to England in order to improve her English¹⁶⁶. After a delay of nearly a year Faraday suggested that she might be placed, as a governess, with Jemima Hornblower, a Sandemanian who kept a school in Stamford Hill¹⁶⁷. A further year passed before the arrangements were completed and Emilie Schoenbein arrived in London in September 1857¹⁶⁸. The Faradays saw her occasionally and she visited the Royal Institution. She intended coming to hear Faraday deliver a discourse on 25 February 1859 on her father's work on ozone¹⁶⁹ and on the 8th asked Faraday for an extra ticket¹⁷⁰. On the 10th there was clearly something wrong with her as Faraday was asked for details of Bence Jones's consulting hours¹⁷¹. At about 7.30 am on Sunday the 13th a messenger arrived at the Royal Institution with a note for Faraday reporting the seriousness of Emilie Schoenbein's condition. Faraday went immediately to fetch Bence Jones but had to leave a note asking him to visit her at Stamford Hill. Already suspecting from the notes sent by Hornblower, and with a passing allusion to a pessimistic Biblical passage, the worst¹⁷², Faraday then went to Stamford Hill himself. There he found that Emilie Schoenbein had died about the time he had received the note at the Royal Institution¹⁷³. Bence Jones performed the post mortem and concluded that she had died from a perforated stomach ulcer¹⁷⁴. Schoenbein was unable to come to England, although Faraday was half expecting him, and it was arranged that the funeral would be on 18 February and would be conducted by an Anglican priest who was a friend of Schoenbein¹⁷⁵. Schoenbein asked Faraday if he would allow his name to be inscribed on her tombstone. Faraday declined on the grounds that he was a dissenter and added that he had not attended the funeral for the same reason¹⁷⁶. Although Faraday was evidently upset, he was clearly not as devastated as Schoenbein and it is not known whether Schoenbein was offended by Faraday's attitude and behaviour in the face of this disaster, though it may be significant it was more than four months before their correspondence resumed¹⁷⁷.

Despite Faraday's Sandemanianism distancing him from many of the social norms of mid-Victorian Britain, his scientific research, his lecturing and his work for the state made him, by the 1850s, one of the cultural icons of the nation and this was recognised in a number of ways, both in Britain and overseas. In institutional terms, universities and societies sought

to recognise Faraday. For example, writing on behalf of the University of Edinburgh, David Brewster tried to persuade him to accept the professorship of chemistry in 1858. Faraday declined as was reported in *The Times* and the *Literary Gazette*¹⁷⁸. Then, in early 1860, he was offered an honorary doctorate at Edinburgh if he was willing to attend the ceremony, but evidently he could not¹⁷⁹. Overseas, Faraday continued to be elected to learned societies located from Moscow to Venice to Batavia (modern Jakarta)¹⁸⁰.

But, perhaps his most significant recognition, outside of a strict academic or scientific context, occurred when the Emperor Napoleon III of France appointed him Commander of the Legion of Honour at the closing ceremony of the Universal Exhibition in Paris in November 1855. As with Faraday's election as one of the eight foreign associates of the Académie des Sciences¹⁸¹, his old friend Dumas played a significant role. Dumas, a member of the Senate of France from 1852, suggested to the Emperor that Faraday be awarded a Grand Medal of Honour of the Exhibition despite not being an exhibitor, or even visiting Paris during the exhibition¹⁸². Then Dumas suggested to the Emperor that Faraday should also be appointed a Commander of the Legion of Honour (the highest rank in the order) to which the Emperor agreed¹⁸³. Because the arrangements were completed in a rush, there were a number of problems with the wording of the announcements for which Dumas felt the need to apologise¹⁸⁴. Furthermore, because Faraday was in Brighton at the time, he was rather slow in responding, but he was happy to accept the honours from the Emperor in a way that he would not accept them from the British political establishment¹⁸⁵.

Faraday did not receive news of the awards until the time of the presentation ceremony¹⁸⁶ and the artist Eugène Delacroix received the insignia on Faraday's behalf. However, Delacroix took them away with him and it was not realised until late April 1856 that Faraday had not received the insignia, which caused Dumas further embarrassment¹⁸⁷. However, the award was subsequently recovered and sent to Faraday¹⁸⁸. Presumably because Faraday had not gone to Paris at the time of the awards, he felt obliged to visit Paris for a week in late July and early August 1856. Whilst there, among other things, he attended a meeting of the Académie, visited Victor Regnault's laboratory and the catacombs¹⁸⁹. The latter he saw with Prince Gorchakov who was with the Russian delegation to the Paris peace conference to settle the Anglo-French war against Russia. This is one of the few indications that Faraday had some cognisance of the major political events that affected many parts of the world. Although he continued his interest in Italian unification¹⁹⁰, his correspondence does not even contain a hint of awareness of the Indian mutiny of 1857–1858 which led to a fundamental reordering of British rule in India with the abolition of the East India Company.

Perhaps the most significant recognition of his place in Victorian science occurred in 1858 when he was offered, for the second time, the nomination

for the presidency of the Royal Society. By that time Lord Wrottesley had served as President for four years and although, following the reforms of the late 1840s, there was as yet no fixed limit to the period an incumbent should serve, Wrottesley decided in May not to seek re-election the following November. Whether any on the Royal Society Council knew of the invitation for Faraday to become the first post-reform President in 1848¹⁹¹ is not known. But immediately following its meeting on 20 May 1858 the council sent a delegation comprising Wrottesley, Gassiot and Grove to Faraday with an invitation to accept nomination¹⁹², depicted in Plate 9. He asked for time to consider the offer. Sarah Faraday expressed her opposition¹⁹³ but whether that was the crucial factor is a moot point. Despite the pleas of another council member, John Percy¹⁹⁴, and also Tyndall, Faraday declined¹⁹⁵. Doubtless with his personal knowledge of the disaster of Humphry Davy's presidency in mind, Faraday told Tyndall if he 'accepted the honour which the Royal Society desires to confer upon me, I would not answer for the integrity of my intellect for a single year'¹⁹⁶. Benjamin Brodie was elected President instead.

Faraday continued to cultivate his good relations with Prince Albert (Prince Consort from 25 July 1857). The prince chaired four discourses during this period, three delivered by Faraday and one by Tyndall¹⁹⁷. More importantly, perhaps, the prince ensured that his two elder sons, the Prince of Wales and Prince Alfred, attended the Christmas lectures. He and they were present on 27 December 1855¹⁹⁸, a scene famously depicted by the artist Alexander Blaikley (Plate 3). Only the young princes attended the rest of the series¹⁹⁹, for which they wrote polite thank-you letters²⁰⁰). In the following two seasons the Prince of Wales attended some of Faraday's Christmas lectures²⁰¹. Faraday was invited to, and indeed is listed as attending, a *levée* each year²⁰². He was also invited to the marriage of the Princess Royal in the Chapel of St James's Palace on 25 January 1858²⁰³, but would have declined for the same reasons that he absented himself from Emilie Schoenbein's funeral.

Prince Albert evidently had a high opinion of Faraday. For example, on 18 March 1858 Faraday was present when the new Chelsea Bridge was opened by the prince, and the Prince of Wales and accompanied them across the bridge²⁰⁴. He must have been in the prince's mind when, just a month later, following the death of Lady Wheatley on 1 April, Faraday was offered, by Queen Victoria at the suggestion of the prince, Wheatley's former Grace and Favour house. This was a Queen Anne building located opposite an *ait* on the River Thames near Hampton Court Palace. As earlier in the decade, Faraday occasionally rented a house on the outskirts of London and commuted to the Royal Institution in order to avoid the bustle of London. In 1856 he stayed in Norwood (spring) and Hornsey (late summer) while in the late summer of 1857 he took a house in Highgate. Thus it was very convenient for Faraday's needs that he should be offered, on a permanent basis, a house at a reasonable

distance from London. Whether this consideration entered Prince Albert's thinking is not known, but Bence Jones was certain that he was responsible for the offer²⁰⁵. He seems to have been in a position to know since Margery Reid's account, written some years later, hinted that Bence Jones and the Prince's German secretary, Ernst Becker, were concerned in some preliminary discussions²⁰⁶.

The offer, which specifically referred to Faraday's lectures to the Prince of Wales, came from Charles Phipps, Prince Albert's private secretary²⁰⁷. However, Faraday's reply to Becker (confirming his involvement) expressed some hesitation over whether or not to accept the offer²⁰⁸. Faraday, Sarah Faraday and Margery Reid visited the house at some point in late April and it was clear, despite the chaos caused by preparations for a sale of the contents²⁰⁹, that a considerable amount of repairs and decoration needed to be carried out. Faraday was concerned about who would pay for the necessary repairs, but Phipps put Faraday's mind at rest on that issue²¹⁰. After informing Reid and Bence Jones²¹¹ (thus confirming Bence Jones's involvement), Faraday wrote to accept the offer²¹². By early July the repairs, estimated to cost £100²¹³, were well under way but, as usual in such cases, the work revealed more problems than anticipated²¹⁴ and had not been completed by the end of August²¹⁵. However, the Faradays were able to take possession during the first half of September and on the 13th he wrote his first surviving letter from his new address²¹⁶. Thereafter the Faradays divided their time between Hampton Court and the Royal Institution.

Growing fame both in the scientific community and in wider society involved his being requested to support several causes and individuals. He was invited to support various funds contributing, for example, to John Ralfs's annuity organised by Joseph Hooker²¹⁷ while he donated £5 to the Humboldt testimonial fund although not before criticising such exercises to John Phillips who had established it²¹⁸. In following his earlier practice he declined to provide either Lyon Playfair or Maxwell with testimonials for their respective applications for the chairs of chemistry and of natural philosophy at the University of Edinburgh²¹⁹. He did, however, seek to find employment for individuals connected with the Sandemanian Church such as Charles Vincent²²⁰ and William Deacon where Faraday used the contacts he had made through his lighthouse work to attempt to secure employment for him²²¹.

With all the significant indications of recognition of his contributions to science and to society that Faraday received, he started to become a subject of biographical interest, itself evidence of his growing fame. Although various short profiles of him had been published in the past, in the second half of the 1850s interest in Faraday's life began to grow, not only in England but elsewhere in Europe²²². Forbes, for example, wrote a long discussion of Faraday's work in his entry on the history of science for the new edition of the *Encyclopaedia Britannica*²²³ while in October 1857 Faraday appeared as

number 18 in the series of *Photographical Portraits of Living Celebrities* by Maull and Polyblank. This image depicted Faraday standing holding a bar magnet (Plate 1) and was published with a short, surprisingly accurate, biographical sketch by Edward Walford²²⁴. Such attention embarrassed Faraday, although presumably since he was gracious enough to supply material, refusal would have been a greater embarrassment²²⁵. He did, however, draw the line at providing autographs to casual enquirers²²⁶.

Now in his late sixties, and despite his usual complaints about poor health, poor memory and general weariness, Faraday remained remarkably active and bore up well under the pressures that were placed on him. Although his way of doing science was coming to be seen as rather old fashioned, as evinced by Stokes's reactions to his paper on gravity, nevertheless he remained a source of inspiration to the younger generation of mathematical natural philosophers such as Thomson and Maxwell. He was delighted with the way that Maxwell had proved the mathematical correctness of his lines of force²²⁷, although he wondered whether Maxwell's 'hieroglyphics' were necessary²²⁸. The perception of him as a conservative figure was confirmed by Charles Darwin, whose publisher, John Murray, sent Faraday a copy of *The Origin of Species* when it was published in late 1859²²⁹. Darwin was concerned that Faraday, because of his Sandemanianism, might prevent Huxley from delivering a Friday Evening Discourse on the subject²³⁰. However, Huxley did deliver it the following February²³¹. Although Faraday said he would read the book²³², it is not clear whether he did and his immediate reactions, if any, are not known. In the summer of 1860 he attended the meeting of the British Association at Oxford where, on the morning of 30 June, he was photographed at the top of Christ Church by Charles Dodgson²³³ (Plate 12). As Faraday felt very unwell²³⁴ he left afterwards and so missed the discussion that afternoon between Huxley, Hooker and Samuel Wilberforce on Darwin's work²³⁵. There is something emblematic about the changes happening to science at that time that Faraday, that epitome of the natural philosopher as Christian during the first half of the nineteenth century, should miss the event that by the end of the century had come to signify, for many people, an opposition between Christianity and science.

1. John Frederick William Herschel (1792–1871, ODNB). Man of science who worked on astronomy, chemistry and physics. Lived and worked at Collingwood near Hawkhurst from 1840. Master of the Mint, 1850–1855. (Those discussed only in the introduction are identified in the notes here. All other individuals are identified in the appropriate place in the Correspondence.)

2. Letters 3518 and 3519.

3. William Buckland (1784–1856, ODNB). Geologist. Dean of Westminster, 1845–1856.

4. William Scoresby (1789–1857, ODNB). Retired clergyman who lived in Torquay. Previously lived in Whitby and worked on magnetism especially in the Arctic.

5. Letters 3302, 3303, 3304, 3305, 3306 and 3309.

6. Letter 3315.

7. Letter 3375.

8. Letter 3798.

9. Letter 3184.

10. Letter 3798. See also letters 3325 and 3623.
11. Letter 3390.
12. Faraday (1860b). Letter 3777.
13. Letter 3541.
14. Jeffreys (1960), entry 439 mistakenly followed Bence Jones (1870a), 2: 393 in stating that Faraday delivered a course on electricity after Easter 1857.
15. Letter 3667.
16. Letter 3669.
17. See, for example, Pengelly, H. (1897), 97.
18. Letter 3138.
19. James (2002b).
20. Letters 3132, 3133 and 3135.
21. Letters 3183, 3474, 3511.
22. Letters 3323, 3324.
23. Letter 3622.
24. For example, Faraday to Tyndall, 6 October 1855, letter 3027, volume 4.
25. Letters 3272, 3397, 3398, 3399, 3404.
26. Letters 3116, 3258, 3317. For an account of this see Eve and Creasey (1945), 290–7.
27. Tyndall, *Diary*, 6 December 1855. See Tyndall (1868), 202 where he published the story.
28. Tyndall (1860).
29. See letters 3199 and 3200 for the tangle that Stokes got himself in over how to address Faraday.
30. Tyndall, *Diary*, 10 September 1857. For entries in a similar vein see also 27 February 1855, 7 May 1857 and 19 September 1857.
31. For example Tyndall, *Diary*, 2 November 1857.
32. For example Tyndall, *Diary*, 22 January 1858 and 5 February 1858.
33. Tyndall, *Diary*, 15 January 1858.
34. Letters 3185, 3186, 3320, 3327, 3495, 3514, 3699, 3700 and 3701.
35. *Athenaeum*, 15 June 1850, pp. 640–1, which contains an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'. It should be noted that Faraday had carried out his original experimental work in 1842 in the context of thinking about glaciers. Faraday, *Diary*, 8, 10, 26 September 1842, 4: 6314–67. See also Faraday, *Diary*, 16 April 1850, 5: 10844–52.
36. Tyndall (1857), Friday Evening Discourse of 23 January 1857, p. 322.
37. See Rowlinson (1971) and Hevly (1996).
38. Letter 3588.
39. Faraday (1859b), 372–4. See letter 3493.
40. 'On Regelation' in Faraday (1859b), 377–82. See letter 3529.
41. James (1985).
42. Faraday (1853), 69.
43. RI MS F4 J17, p. 1. Delivered on 27 December 1855.
44. Faraday, *Diary*, 2 February 1856 to 20 December 1856, 7: 14243–15403. For an analysis of the distribution of the entries see Tweney *et al.* (2002).
45. Faraday, *Diary*, 2 February 1856, 7: 14243. De La Rue in 1862 donated some of his apparatus to the Royal Institution. RI MS F5B, p. 119.
46. For example Faraday, *Diary*, 6 February 1856, 7: 14319–26.
47. Letter 3085.
48. Letter 3082.
49. Faraday, *Diary*, 22 October 1856, 7: 15183.
50. Letter 3168. See also letter 3164.
51. Faraday, *Diary*, 7 February 1856, 7: 14328. See also his reference to it in his Christmas lecture of 27 December 1855, RI MS F4 J17, p. 1.
52. Faraday, *Diary*, 4 June 1856, 7: 14774.
53. Faraday (1856b), Friday Evening Discourse of 13 June 1856, pp. 310–2.
54. James (1985), 154.
55. Faraday (1857c).

56. For detailed studies of Faraday's gold work see Tweney (2002, 2006) and Tweney *et al.* (2002, 2005).
57. Faraday, *Diary*, 23 January 1858 to 18 March 1858, 7, pp. 412–61.
58. Grove (1859), Friday Evening Discourse of 28 January 1859.
59. Gassiot (1858), 16.
60. RI MM, 4 April 1859, 11: 273–4. For early acquisitions from the fund see RI MS F5B, p. 128.
61. For example, Faraday to Whewell, 13 December 1836, letter 954, volume 2.
62. Faraday (1851a).
63. Faraday (1857a), Friday Evening Discourse of 27 February 1857. For discussions of this lecture see Lunteren (1991), 170–4 and Gooding (1980), especially pp. 26–7.
64. *Civ. Eng. Arch. J.*, 1857, 20: 39–40, 102–3.
65. Letter 3281.
66. *Athenaeum*, 28 March 1857, pp. 397–9. De Morgan's comments might be interpreted, in part, in terms of the long-standing institutional rivalry between the Royal Institution and University College, London.
67. Letter 3371.
68. Forbes to Maxwell, 31 March 1857, in Campbell and Garnett (1884), 182–3. Forbes was very critical of the (then unidentified) *Athenaeum* reviewer.
69. Letters 3371 and 3354.
70. Faraday, *Diary*, 10 February 1859 to 9 July 1859, 7: 15785–998.
71. Letters 3788 and 3790.
72. For discussions of this work see Cantor (1991), 245–56 and Lunteren (1991), 174–5.
73. Phillippus to Faraday, 4 July 1838 and Faraday to Phillippus, 19 July 1838, letters 1099 and 1101, volume 2.
74. Mure to Faraday, 7 June 1853 and Faraday to Mure, 7 June 1853, letters 2684 and 2685, volume 4.
75. Letters 3047, 3057 and 3058.
76. On the Royal Commission see Whitehead (2005), 158–62.
77. Conlin (2006), 81–2.
78. See letter 3246.
79. John Ruskin (1819–1900, ODNB). Art critic and social reformer.
80. *Parliamentary Papers*, 1857, 2nd session [2261] XXIV, questions 2419–21 and 2460–a.
81. Ruskin to Furnivall, 9 June 1854, in Wise (1897), 30–33, p. 31 and Ruskin (1856), vii.
82. Ruskin to Richmond, 6 March 1874 and March 1874; Ruskin to Brown, 25 June 1874, in Cook and Wedderburn (1909), 85–6, 117–8. Ruskin (1874), 109.
83. Letter 3262.
84. Faraday to Donaldson, 21 April 1837, letter 987, volume 2.
85. Letter 3278.
86. *Parliamentary Papers*, 1857, 2nd session [2261] XXIV, p. xvi.
87. Whitehead (2005), 162.
88. Letters 3378, 3381 and 3382.
89. TNA ED28/10, p. 54.
90. Robertson (1978), 197.
91. Whitehead (2005), 162–76.
92. Conlin (2006), 81–2.
93. Letter 3603.
94. Letter 3604.
95. Letter 3620. *Parliamentary Papers*, 1859 2nd session (106) XV. On this commission see Swinney (2003), 92–3.
96. Conlin (2006), 236–7, 398.
97. Letter 3610.
98. Letter 3613.
99. Letter 3614.
100. Letter 3737.
101. Letter 3741.
102. Letters 3763 and 3764.

103. Letters 3774 and 3778.
104. Bence Jones (1870a), 2: 435 and letter 3812.
105. Letters 3782 and 3785.
106. Letter 3812.
107. *Parliamentary Papers*, 1861 (504) XXXV.
108. Bradley and Pevsner (2003), 218.
109. Herbert to Faraday, 4 February 1836, letter 885, volume 2.
110. Letters 3837 and 3859.
111. Letter 3831.
112. Letters 3090, 3091 and 3096.
113. Letter 3703.
114. Letters 3841, 3855 and 3873.
115. James (1999a), 156–7.
116. Letter 3609.
117. Letter 3265.
118. Letter 3279.
119. The operation of this lamp is described in Holmes (1862).
120. Trinity House By Board, 14 December 1858, GL MS 30010/42, p. 41.
121. Letter 3590.
122. Holmes (1862), 1–2. McLeod, *Diary*, 2 August 1860.
123. Letter 3831. McLeod, *Diary*, 2 August 1860.
124. Letter 3728.
125. Letter 3733.
126. Trinity House By Board, 28 February 1860, GL MS 30010/42, pp. 552–3.
127. Faraday (1860a), Friday Evening Discourse of 9 March 1860.
128. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, p. 7.
129. Letter 3590.
130. Letter 3661.
131. Letter 3662.
132. Airy, W. (1896), 241. For his report see *Parliamentary Papers*, 1861 [2793] XXV, volume 1, pp. 79–80.
133. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, pp. 80–1.
134. Letter 3809.
135. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, p. 54.
136. Listed in letter 3828.
137. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, p. 55.
138. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, pp. 55–8.
139. Letter 3828.
140. Faraday to Herbert, 20 October 1855, letter 3029, volume 4.
141. Trinity House By Board, 28 August 1860, GL MS 30010/43, pp. 103–4.
142. Letter 3841.
143. Letter 3846.
144. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, p. 59. See also letter 3865.
145. For an account of the proceedings of the Royal Commission from Chance's perspective see Chance (1902), 7–28.
146. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, p. x.
147. *Parliamentary Papers*, 1861 [2793] XXV, volume 1, p. xxxix.
148. Faraday (1858c), Friday Evening Discourse of 11 June 1858, p. 560.
149. Faraday (1860a), Friday Evening Discourse of 9 March 1860, p. 220.
150. But see letters 3330, 3540 and 3757.
151. Letter 3453 and Cantor (1991), 65.
152. Letter 3862.
153. See Cantor (1991), 60.
154. Letters 3179 and 3315.
155. Letter 3324.
156. Letter 3166.

157. Letter 3643.
158. Letter 3639.
159. Letters 3184 and 3208.
160. Letters 3516 and 3583.
161. Letters 3793, 3794, 3808 and 3836.
162. Letter 3277.
163. Letter 3280.
164. Tyndall, *Diary*, 9 May 1857.
165. Letters 3313 and 3314.
166. Letter 3054.
167. Letters 3191 and 3197.
168. Letter 3337.
169. Faraday (1859a), Friday Evening Discourse of 25 February 1859.
170. Letter 3563.
171. Letter 3569.
172. Letter 3567.
173. Letter 3569.
174. Letter 3569 and death certificate (GRO).
175. Letter 3570.
176. Letter 3572.
177. Letter 3619.
178. Letter 3429; *The Times*, 20 May 1858, p. 12, col. d; *Lit. Gaz.*, 22 May 1858, p. 497.
179. Letter 3729.
180. Letters 3103, 3369 and 3111 respectively.
181. Dumas to Faraday, 23 December 1844, letter 1661, volume 3.
182. Letter 3038.
183. Letter 3042.
184. Letter 3042.
185. Compare letters 3048 and 3762. See also Cantor (1991), 101.
186. Letter 3044.
187. Letter 3125.
188. Letter 3150.
189. Letters 3172, 3177 and 3183 respectively.
190. Letter 3584.
191. Lyell to Bunbury, May 1848 in Lyell (1881), 2: 145–6.
192. White (1898), 117–8.
193. Tyndall (1868), 267.
194. Letter 3442.
195. Letter 3443.
196. Tyndall (1868), 267.
197. Faraday (1857a, 1858b, 1859a), Tyndall (1859), Friday Evening Discourses of 27 February 1857, 12 February 1858, 25 February 1859 and 10 June 1859.
198. *The Times*, 28 December 1855, p. 6, col. e.
199. *The Times*, 31 December 1855 p. 7, col. a; 5 January 1856, p. 6, col. f; 7 January 1856, p. 6, col. f; 9 January 1856, p. 9, col. a.
200. Letters 3071 and 3072. See also letter 3074.
201. *The Times*, 29 December 1856 p. 6, col. f; 1 January 1857, p. 6, col. e; 9 January 1857, p. 7, col. a. Tyndall, *Diary*, 29 and 31 December 1857, 7 January 1858.
202. *The Times*, 21 February 1856, p. 9, col. f; 27 February 1857, p. 7, col. e; 19 February 1858, p. 12, col. c; 3 March 1859, p. 5, col. f; 24 February 1860, p. 3, col. b. For this latter levée see also TNA LC6/16.
203. *The Times*, 26 January 1858, p. 9, col. a.
204. *The Times*, 19 March 1858, p. 9, col. e. This was not the only connection that Faraday had with new bridges being built over the Thames. On 16 April 1858 he took Tyndall over the works for the new Westminster Bridge, which was by the same engineer. Tyndall, *Diary*, 18 April 1858.

205. Bence Jones (1870a), 2: 398.
206. Margery Reid, *Diary*, 28 May 1865, RI MS F13B, p. 98.
207. Letter 3417.
208. Letter 3418.
209. Margery Reid, *Diary*, 28 May 1865, RI MS F13B, pp. 99–100.
210. Letter 3421.
211. Letters 3422 and 3423.
212. Letter 3424.
213. Estimate of 14 June 1858, TNA WORK19/74.
214. Letter 3474.
215. Letter 3510. The final cost was £179 12s 3d. Office of Works to Treasury, 6 May 1859, TNA WORK19/74.
216. Letter 3515.
217. Letter 3527.
218. Letter 3659.
219. Letters 3446 and 3688.
220. Letter 3547.
221. Letters 3825, 3826, 3829 and 3856.
222. Letter 3114.
223. Letters 3148, 3225 and 3485. Forbes (1857), 977–82.
224. Maull and Polyblank, *Photographical Portraits of Living Celebrities*, No. 18, October, 1857. Price 5s. Edward Walford (1823–1897, ODNB) was a journalist and writer.
225. Letter 3114.
226. Letters 3101 and 3336.
227. Letter 3364.
228. Letter 3357.
229. Letter 3689. Darwin (1859). This is not noted in the list of presentation copies in Burkhardt *et al.* (1985–2005), 7: 533–6.
230. Hooker to Darwin, 21 November 1859 and Darwin to Hooker, 22 November 1859 in Burkhardt *et al.* (1985–2005), 7: 383 and 387.
231. Huxley, T.H. (1860), Friday Evening Discourse of 10 February 1860.
232. Letter 3689.
233. See Taylor *et al.* (2002), 251.
234. Letter 3799.
235. James (2005).

Biographical Register

This provides information on those individuals who are mentioned in three or more letters in this volume.

AGASSIZ, Jean Louis Rodolphe (1807–1873, ANB): Swiss–American naturalist and geologist. Professor at Harvard from 1847.

AIRY, George Biddell (1801–1892, ODNB): Astronomer Royal at the Royal Greenwich Observatory, 1835–1881.

ALBERT EDWARD, Prince of Wales (1841–1910, ODNB): Eldest son of Queen Victoria and Prince Albert. Prince of Wales, 1841–1901.

ALBERT FRANCIS CHARLES AUGUSTUSEMANUEL, Prince (1819–1861, ODNB): Husband of Queen Victoria. Prince Consort from 1857.

ALFRED, Prince (1844–1900, ODNB): Second son of Queen Victoria and Prince Albert.

ANDERSON, Charles (d.1866, age 75, GRO): Originally a sergeant in the Royal Artillery. Joined Faraday on 3 December 1827 as assistant on the project to improve optical glass. Bence Jones (1870a), 1: 398. After the end of the glass project, in 1830, Faraday paid him out of his own pocket (Faraday to South, 3 February 1865, IET MS SC 3) before he was appointed Royal Institution laboratory assistant in 1832. RI MM, 5 November 1832, 8: 28.

ANDREWS, Thomas (1813–1885, ODNB): Professor of Chemistry at Queen's College, Belfast, 1845–1879.

ARAGO, Dominique François Jean (1786–1853, DSB): French physicist and astronomer. Professor of Descriptive Geometry at Ecole Polytechnique. Director of the Paris Observatory. Co-editor of *Ann. Chim.* Permanent Secretary of the Académie des Sciences from 1830.

AUSTIN, Alfred (1805–1884, B1): Secretary to the Office of Works, 1854–1868.

AYRTON, William Scrope (1804–1885, B1): Commissioner of the Court of Bankruptcy, Leeds.

BACHHOFFNER, George Henry (1810–1879, ODNB): Popular scientific lecturer.

BALMAT, Auguste (1808–1862, Clark, R. (1949), 187–8): Alpine guide.

BARLOW, Cecilia Anne, née Law (d.1868, age 72, GRO): Married John Barlow in 1824. See his entry in AC.

BARLOW, John (1798–1869, AC): Secretary of the Royal Institution, 1843–1860.

BARNARD, Frank (1828–1895, GRO): A nephew of Sarah and Michael Faraday who practised as an artist.

BARNARD, George (1807–1890, B4): Painter. Brother of Sarah Faraday.

BARNARD, Jane (1832–1911, GRO): A niece of Michael and Sarah Faraday. Lived with them at the Royal Institution from at least 1851. Census returns for 1851, TNA HO107/1476, f. 64.

BARNARD, John (1797–1880, GRO): Son of Edward and Mary Barnard. Brother of Sarah Faraday. Silversmith. See Grimwade (1982), 431.

BARNARD, Margaret, née Faraday (1802–1862, GRO): Faraday's younger sister. Married John Barnard, 14 March 1826.

BARNARD, Mary *see* DEACON, Mary.

BARNARD, William (1801–1848, GRO): A brother of Sarah Faraday. Silversmith. See Grimwade (1982), 431.

BARRY, Charles (1795–1860, ODNB): Architect of the Houses of Parliament.

BECKER, Ernst (1826–1888, Pangels (1996)): German secretary to Prince Albert.

BECKETT, William (1784–1863, B1): MP for Leeds, 1841–1852 and for Ripon, 1852–1857.

BECQUEREL, Alexandre-Edmond (1820–1891, DSB): Professor of Physics at the Conservatoire des Arts et Métiers from 1852.

BENCE JONES, Henry (1814–1873, ODNB under Jones): Physician at St George's Hospital, 1846–1862. A Visitor, 1851–1853, and, from 1853, a Manager of the Royal Institution.

BENCE JONES, Millicent, née Acheson (d.1887, age 78, GRO): Married Henry Bence Jones in 1842 (see under his ODNB entry). She was lady by virtue of being the daughter of an Earl.

BERTHON, Peter Henry (d.1890, age 91, GRO): Secretary of Trinity House, 1856–1867, Chaplin (1950), 183.

BIOT, Jean-Baptiste (1774–1862, DSB): French physicist.

BOWMAN, William (1816–1892, ODNB): Surgeon specialising in ophthalmology.

BRANDE, William Thomas (1788–1866, ODNB): Professor of Chemistry at the Royal Institution, 1812–1852. Superintendent of machinery, 1825–1852, and then of the Coining and Die Department, 1852–1866, at the Royal Mint.

BREWSTER, David (1781–1868, ODNB): Man of science. Worked chiefly on optics. Principal of St Andrews University, 1838–1859 and then of Edinburgh University, 1859–1868.

BRODIE, Benjamin Collins (1783–1862, ODNB): Surgeon at St George's Hospital. President of the Royal Society, 1858–1861.

BRODIE, Benjamin Collins (1817–1880, ODNB): Professor of Chemistry at University of Oxford, 1855–1872.

BROOKE, Charles (1804–1879, ODNB): Surgeon and inventor of self-registering instruments.

BROWN, Hannah, née Meredith (d.1878, age 70, GRO): Companion of Angela Georgina Burdett Coutts.

BRUNEL, Isambard Kingdom (1806–1859, ODNB): Civil engineer. Worked on the Great Western Railway, the *Great Eastern* ship and many other projects.

BUCHANAN, George (1834–1902, *Min. Proc. Inst. Civ. Eng.*, 1902, **150**: 425–7): Son of Charlotte Buchanan (a sister of Sarah Faraday’s) and George Buchanan. Civil engineer who worked for the Madras Irrigation and Canal Company, 1860 to 1863.

BUFFON, Georges-Louis Leclerc, Comte de (1707–1788, DSB): French natural historian.

BUNSEN, Robert Wilhelm Eberhard (1811–1899, DSB): Professor of Chemistry at the University of Heidelberg, 1852–1889.

BURDETT COUTTS, Angela Georgina (1814–1906, ODNB): Philanthropist and heiress who lived at 1 Stratton Street (near Albemarle Street).

CAMPBELL, John Francis (1822–1885, ODNB): Secretary of Royal Commission on Lighthouses.

CHANCE, Elizabeth, née Ferguson (d.1887, age 67, GRO): Married J.T. Chance in 1845. See his ODNB entry.

CHANCE, James Timmins (1814–1902, ODNB): Birmingham glass manufacturer.

CHANCE, Robert Lucas (1782–1865, B4): Birmingham glass manufacturer.

CHEVERTON, Benjamin (1794–1876, AIKL): Machine sculptor.

CLARK, James (1788–1870, ODNB): Court physician. Member of the Senate of the University of London, 1838–1865. Influential in establishing the Royal College of Chemistry.

CLARK, Josiah Latimer (1822–1898, ODNB): Assistant electrical engineer to the Electric and International Telegraph Company, 1850–1860.

CLOSE, Mark Currie (d.1884, age 79, GRO): An Elder Brother of Trinity House, 1856–1884, Chaplin (1950), 191.

COWPER, William Francis (1811–1888, ODNB): Whig MP for Hertford, 1835–1868 and First Commissioner of Works, 1860–1866.

CRAWFORD, William (d.1887, age 78, SRO): Glasgow wool merchant.

CRUM, Agnes, née Pollok (d.1887, age 81, SRO): Wife of James Crum.

CRUM, James (d.1861, age 55, SRO): Cotton merchant and uncle of Margaret Thomson.

DAINES, John Benjamin (d.1864, age 48, GRO): Inventor of a method of stone preservation.

DANIELL, John Frederic (1790–1845, ODNB): Professor of Chemistry at King's College, London, 1831–1845. Foreign Secretary of the Royal Society, 1839–1845.

DAVY, Humphry (1778–1829, ODNB): Professor of Chemistry at the Royal Institution, 1802–1812. Knighted 1812, created Baronet, 1818. Secretary of the Royal Society, 1807–1812, President, 1820–1827.

DEACON, Caroline, née Reid (1816–1890, Reid, C.L. (1914)): Niece of Sarah Faraday. Married Thomas John Fuller Deacon on 18 June 1846, GRO.

DEACON, Constance (1847–1924, GRO under Cree): Daughter of Caroline and Thomas John Fuller Deacon.

DEACON, Mary, née Barnard (1833–1910, GRO): Niece of Michael and Sarah Faraday. Married William Matheson Deacon in 1860.

DEACON, Thomas John Fuller (d.1901, age 78, GRO): Nephew-in-law of Sarah Faraday.

DEACON, William Matheson (1825–1891, Deacon (1899), 41): Merchant Navy captain. Married Mary Barnard in 1860.

DEGRAND, Georges-Mathilde-Ernest (1822–1892, DBF): French engineer.

DE LA RIVE, Arthur-Auguste (1801–1873, DSB): Professor of General Physics at Geneva from 1823 and, from 1825, of Experimental Physics. Swiss ambassador to London in 1850 and again in 1860.

DE LA RIVE, Louise-Victoire-Marie, olim Fatio née Fatio (1808–1874, Choisy (1947), 50): Married Arthur-Auguste De La Rive on 24 May 1855.

DE LA RUE, Warren (1815–1889, ODNB): Printer and amateur astronomer.

DE LA WARR, George John Sackville-West, 5th Earl (1791–1869, CP): Lord Chamberlain, 1858–1859.

DESPRETZ, César Mansuète (1792–1863, DBF): French chemist.

DUMAS, Herménie, née Brongniart: Married Dumas in 1826. See Crosland, M. (1992), 184.

DUMAS, Jean-Baptiste-André (1800–1884, DSB): Professor of Chemistry at the Sorbonne, 1841–1868. Member of the French Senate and Vice-President of the Imperial Council of Public Education.

DUNBAR, Duncan (1804–1862, B5): Very wealthy shipowner and member of Royal Commission on Lighthouses.

EASTLAKE, Charles Lock (1793–1865, ODNB): Director of the National Gallery, 1855–1865.

ELIOT, George (1819–1880, ODNB under Marian Evans): Novelist.

FARADAY, Sarah, née Barnard (1800–1879, GRO): Daughter of Edward and Mary Barnard. Married Faraday on 12 June 1821.

FARRER, Thomas Henry (1819–1899, ODNB): Assistant Secretary at the Board of Trade responsible for the Marine Department, 1853–1865.

FIELD, Frederick (1826–1885, ODNB): Former student at the Royal College of Chemistry, worked in Chile as a metallurgist before being appointed lecturer in chemistry at St Mary's Hospital in 1860.

FISHER, William (d.1857, age 52, GRO): Cordwainer and Sandemanian in Old Buckenham.

FITZMAURICE, William Edward (1805–1889, *The Times*, 29 June 1889, p. 6, col. d): Army officer and Conservative MP for Buckinghamshire, 1842–1847.

FITZROY, Henry (1807–1859, ODNB): Liberal MP for Lewes and First Commissioner of Board of Works, 1859–1860.

FORBES, James David (1809–1868, ODNB): Professor of Natural Philosophy at Edinburgh University, 1833–1860. Secretary of the Royal Society of Edinburgh, 1840–1860.

FRANCIS, William (1817–1904, Brock and Meadows (1984), 97–128): Publisher.

FRANKLAND, Edward (1825–1899, ODNB): Professor of Chemistry at Owen’s College, Manchester, 1851–1857 and then at St Bartholomew’s Hospital.

FRESNEL, Augustin Jean (1788–1827, DSB): French physicist.

GASSIOT, John Peter (1797–1877, ODNB): Wine merchant and electrician.

GEISSLER, Johann Heinrich Wilhelm (1815–1879, DSB): Technician at the University of Bonn from at least 1852.

GLADSTONE, John Hall (1827–1902, ODNB): Chemist and member of Royal Commission on Lighthouses.

GORDON, Alexander (1802–1868, B1): Lighthouse engineer.

GORDON, Robert (1796–1869, B5): Naval officer and Deputy Master of Trinity House, 1858–1861.

GRAHAM, Thomas (1805–1869, ODNB): Chemist and Master of the Mint, 1855–1869.

GROVE, William Robert (1811–1896, ODNB): Lawyer and man of science. Professor of Experimental Philosophy at the London Institution, 1841–1847.

HAMILTON, William Alexander Baillie (1803–1881, B1, O’Byrne (1849), 453): Second Secretary of the Admiralty, 1845–1855. Chairman of Royal Commission on Lighthouses.

HANSTEEN, Christopher (1784–1873, DSB): Norwegian astronomer and geophysicist.

HARRIS, William Snow (1791–1867, ODNB): Plymouth man of science who worked on electricity, particularly lightning conductors.

HERBERT, George (d.1866, age 49, GRO): Assistant Secretary at Trinity House, 1855–1865. *Royal Kalendar*.

HERBERT, Jacob (d.1867, age 79, *Gent. Mag.*, 1867, 3: 262): Secretary of Trinity House, 1824–1856. Chaplin (1950), 183.

HERRIES, Maria Julia (c.1820–1857, TNA HO107/1481, f. 49, p. 26; *Gent. Mag.*, 1857, 3: 347): Daughter of John Charles Herries (1778–1855, ODNB). Tory MP for Stamford, 1847–1853. Died in Savoy on 14 July 1857.

HIRST, Thomas Archer (1830–1892, ODNB): Taught mathematics at Queenwood School.

HOLMES, Frederick Hale (b.c.1812, TNA RG9/58, f. 123, p. 34): Professor of Chemistry at the Royal Panopticon of Science (*Lit. Gaz.*, 23 July 1853, p. 722) and one of the pioneers of electric light.

HOOKER, Joseph Dalton (1817–1911, ODNB): Botanist and Assistant Director of the Royal Botanic Gardens, Kew, 1855–1865.

HORNBLOWER, Jemima Hanbury (d.1861, age 61, GRO): Ran a school in Stamford Hill and was a deaconess in the Sandemanian Church. Cantor (1991), 48.

HUMBOLDT, Friedrich Wilhelm Heinrich Alexander von (1769–1859, DSB): German man of science and traveller.

HUXLEY, Thomas Henry (1825–1895, ODNB): Fullerian Professor of Physiology at the Royal Institution, 1855–1858 and Professor of Natural History at the Royal School of Mines.

JONES, Edward (d.1891, age 87, AC): Vicar of West Peckham, near Maidstone, 1839–1880.

KNOCHENHAUER, Karl Wilhelm (1805–1875, P1, 3): Director of the Realschule in Meiningen.

LAWES, James Bennet (1814–1900, ODNB): Agricultural chemist.

LEIGHTON, Minnie Ross (1857–1878, GRO): Daughter of Charles Ross Leighton (d.1861, age 39, GRO), modeller who was certified insane in June 1859, and Louisa Leighton, née Newall (b.c.1830, PRO RG9/197, f. 19, p. 1), dressmaker. Elected to the Infant Orphan Asylum in late 1860, see Candidates Book, RLSA 90/21/20/4, f. 277.

LIEBIG, Justus von (1803–1873, DSB): Professor of Chemistry at Giessen University, 1825–1851. Moved to Munich in 1852. Worked especially on organic and agricultural chemistry.

LYELL, Charles (1797–1875, ODNB): Geologist.

MACFARLANE, Donald: William Thomson's assistant at the University of Glasgow.

MAGNUS, Heinrich Gustav (1802–1870, DSB): Professor of Technology and Physics at Berlin University, 1845–1870.

MALLET, Robert (1810–1881, ODNB): Irish engineer and geologist.

MANBY, Charles (1804–1884, ODNB): Civil engineer and Secretary of the Institution of Civil Engineers, 1839–1856.

MARCET, Jane, née Haldimand (1769–1858, ODNB): Popular scientific writer.

MASSELIN, Armand (1829–1892, private communication from François Mousnier-Lompré): French-born manager of Lighthouse Department at Chance Bros.

MATTEUCCI, Carlo (1811–1868, DSB): Italian physiologist and physicist.

MATTEUCCI, Robinia Elizabeth, née Young (b.c.1811, GRO marriage certificate): Married Carlo Matteucci on 7 September 1846.

MAXWELL, James Clerk (1831–1879, ODNB): Professor of Natural Philosophy at Marischal College, Aberdeen, 1856–1860.

MERIAN, Rudolf (1797–1871, P2, 3): Professor of Mathematics at Basle.

MILLER, William Hallowes (1801–1880, ODNB): Professor of Mineralogy at Cambridge and Foreign Secretary of the Royal Society, 1856–1873.

MOIGNO, François Napoleon Marie (1804–1884, NBU): French scientific writer.

MURCHISON, Roderick Impey (1792–1871, ODNB): Retired army officer and geologist.

MURRAY, John (1808–1892, ODNB): Publisher of 50 Albemarle Street.

NAPOLEON III (1808–1873, NBU): Emperor of France.

NEWMAN, John (1783–1860, ODNB): Scientific instrument maker.

NEWTON, Isaac (1642–1727, ODNB): Natural philosopher.

NICOL, William (c.1771–1851, ODNB): Inventor of a polarising prism.

NORMANDY, Alphonse René le Mire de (1809–1864, ODNB): French-born chemist.

OWEN, Richard (1804–1892, ODNB): Superintendent of natural history at the British Museum from 1856.

PALAGI, Alessandro: Physicist in Bologna.

PANIZZI, Anthony (1797–1879, ODNB): Principal Librarian of the British Museum, 1856–1866.

PARADISE, William (d.1866, age 78, GRO): A member of the Newcastle Sandemanian Church. Cantor (1991), 68.

PARKES, William (1822–1889, B2): Engineer who designed Red Sea Lighthouse.

PASLEY, Charles William (1780–1861, ODNB): Retired general in Royal Engineers.

PERCY, John (1817–1889, ODNB): Metallurgist in Birmingham and then taught at Royal School of Mines, 1851–1879.

PHIPPS, Charles Beaumont (1801–1866, ODNB): Private secretary to Prince Albert.

PILCHER, John Giles and Jeremiah and Sons: Suppliers of red lead of Morgan's Lane, Tooley Street. POD.

PLAISANCE, Anne-Charles Lebrun, Duc de (1775–1859, ANF): Grand Chancellor of the Legion of Honour.

PLATEAU, Joseph Antione Ferdinand (1801–1883, DSB): Professor of Physics at the University of Ghent, 1835–1872. Totally blind from 1843.

PLAYFAIR, Lyon (1818–1898, ODNB): Secretary for Science at the Department of Science and Art, 1853–1855 and from 1855 to 1858 Secretary of the entire department. Professor of Chemistry at Edinburgh, 1858–1869.

PLÜCKER, Julius (1801–1868, DSB): Professor of Physics, 1847–1868, at the University of Bonn.

POGGENDORFF, Johann Christian (1796–1877, DSB): Editor of *Annalen der Physik und Chemie*, 1824–1877.

POLLOCK, Juliet, née Creed (d.1899, age 80, GRO): Writer. Married William Frederick Pollock in 1844.

PONTIFEX, EDMUND AND WILLIAM AND WOOD: White lead suppliers of Millwall, Poplar and Shoe Lane. POD.

POULTER, Jonathan (d.1879, will in PRFDHC): Trinity House superintendent at Blackwall. *Parliamentary Papers*, 1861 [2793] XXV, volume 2, p. 33.

RANSOME, Frederick (1818–1893, B3): Inventor of an artificial sandstone.

REGNAULT, Henri Victor (1810–1878, DSB): Professor of Physics at the Collège de France, 1841–1854.

REID, Margery Ann (1815–1888, Reid, C.L. (1914)): Niece of Sarah Faraday. Lived with the Faradays at the Royal Institution between about 1826 and 1840.

REID, William Ker (1787–1868, Reid, C.L. (1914)): Silversmith and a brother-in-law of Faraday.

REYNAUD, François-Léonce (1803–1880, Vapereau (1880), 1532): Director of the French lighthouse service, 1846–1877.

RICHMOND, George (1809–1896, ODNB): Portrait painter.

RIESS, Peter Theophilus (1804–1883, ADB): Professor of Physics at the Academie der Wissenschaften in Berlin from 1842.

ROSCOE, Henry Enfield (1833–1915, ODNB): Studied chemistry at Heidelberg. Professor of Chemistry at Owens College, Manchester, 1857–1885.

RÜHMKORFF, Heinrich Daniel (1803–1877, DSB): German scientific instrument maker who worked in Paris.

RYDER, Alfred Phillipps (1820–1888, ODNB): Royal Navy officer and member of Royal Commission on Lighthouses.

SABINE, Edward (1788–1883, ODNB): Worked on terrestrial magnetism. Treasurer of the Royal Society, 1850–1861.

SAUSSURE, Horace Bénédict de (1740–1799, DSB): Swiss geologist and naturalist.

SAUTTER, Louis (1825–1912, Sautter (1915)): Paris lighthouse constructor who did work for Wilkins on Bishop Rock.

SCHOENBEIN, Berta (1846–1927, Nolte (1999), 295): Daughter of Christian Friedrich Schoenbein and Emilie Wilhelmine Luise Schoenbein.

SCHOENBEIN, Christian Friedrich (1799–1868, DSB): Professor of Physics and Chemistry at University of Basle, 1835–1852 and then Professor of Chemistry.

SCHOENBEIN, Emilie (1836–1859, Nolte (1999), 294): Daughter of Christian Friedrich Schoenbein and Emilie Wilhelmine Luise Schoenbein.

SCHOENBEIN, Emilie Wilhelmine Luise, née Benz (1807–1871, Nolte (1999), 295): Married Christian Friedrich Schoenbein in 1835.

SCHOENBEIN, Fanny Anna Franziska (1840–1921, Nolte (1999), 295): Daughter of Christian Friedrich Schoenbein and Emilie Wilhelmine Luise Schoenbein.

SCHOENBEIN, Wilhelmine Sophie (1838–1914, Nolte (1999), 294): Daughter of Christian Friedrich Schoenbein and Emilie Wilhelmine Luise Schoenbein.

SEGUIN, Marc (1786–1875, DSB): French physicist and engineer.

SENIOR, Nassau William (1790–1864, ODNB): Economist.

SHAKESPEARE, William (1564–1616, ODNB): Playwright.

SHEPHERD, John (d.1859, age 63, GRO, B3): Deputy Master of Trinity House, 1852–1858.

SMIRKE, Sydney (1798–1877, ODNB): Architect.

SMYTH, William Henry (1788–1865, ODNB): Foreign Secretary of the Royal Society, 1837–1839 and 1850–1856. Rear Admiral from 1853.

SOMERVILLE, Mary olim Greig, née Fairfax (1780–1872, ODNB): Scientific writer.

SOUTH, James (1785–1867, ODNB): Astronomer whose observatory was in Kensington.

STEVENSON, Thomas (1818–1887, ODNB): Scottish lighthouse engineer.

STOKES, George Gabriel (1819–1903, ODNB): Fellow of Pembroke College, Cambridge, 1841–1857. Lucasian Professor of Mathematics, University of Cambridge, 1849–1903. Taught at the Royal School of Mines, 1849–1856. Secretary of the Royal Society, 1854–1885.

SZERELMEY, Nicholas Charles (1803–1875, RNL): Hungarian-born Austrian army engineer colonel who served in Italy. By 1841 he had invented a process for stone preservation. Visited London in 1855.

TENNENT, James Emerson (1804–1869, ODNB): Secretary of the Board of Trade, 1852–1867.

THOMSON, James (1822–1892, ODNB): Professor of Engineering at Queen's College, Belfast, 1854–1873.

THOMSON, Margaret, née Crum (1827–1870, Smith and Wise (1989), 146): Married William Thomson in 1852.

THOMSON, William (1824–1907, ODNB): Fellow of Peterhouse, Cambridge, 1845–1852. Professor of Natural Philosophy, University of Glasgow, 1846–1899.

TREVELYAN, Walter Calverley (1797–1879, ODNB): Naturalist and antiquarian in North East England.

TWINING, Thomas (1806–1895, ODNB): Writer on technical education.

TYNDALL, John (1820–1893, ODNB): Taught science at Queenwood College, 1847–1848 and again 1851–1853. Studied at University of Marburg, 1848–1850. Professor of Natural Philosophy at the Royal Institution, 1853–1887. An editor of the *Phil. Mag.*, 1854–1863.

VICTORIA, Queen (1819–1901, ODNB): Queen of England, 1837–1901.

VINCENT, Benjamin (1818–1899, B3): Elder of the London Sandemanian Church, 1849–1864. Assistant Secretary of the Royal Institution from 1848 and librarian, 1849–1889.

VINCENT, Charles Wilson (1837–1905, GRO): Son of Benjamin and Janet Vincent. Assistant Librarian at the Royal Institution, 1851–1857, and a student at the Royal College of Chemistry, 1854–1855. Later worked as an industrial chemist. Cantor (1991), 75–6.

VINCENT, Janet Young, née Nicoll (1811–1863, GRO): Wife of Benjamin Vincent.

VOLTA, Alessandro Giuseppe Antonio Anastasio (1745–1827, DSB): Italian natural philosopher.

WALKER, James (1781–1862, ODNB): Civil and marine engineer.

WAY, John Thomas (d.1883, age 62, GRO): Electric lamp inventor and consulting chemist to the Royal Agricultural Society.

WELD, Charles Richard (1813–1869, ODNB): Assistant Secretary of the Royal Society, 1843–1861.

WHEATSTONE, Charles (1802–1875, ODNB): Professor of Experimental Philosophy at King's College, London, 1834–1875. Worked on sound and electricity.

WHEWELL, William (1794–1866, ODNB): Master of Trinity College, Cambridge, 1841–1866. Primarily an historian and philosopher of science.

WIEDEMANN, Gustav Heinrich (1826–1899, DSB): Professor of Physics at University of Basle, 1854–1863.

WILKINS, William Crane (1813–1884, GRO): Lighthouse and patent lamp manufacturer of 24 and 25 Longacre.

WINSLOW, Charles Frederick (1811–1877, ACAB): American physician.

WÖHLER, Friedrich (1800–1882, DSB): Professor of Chemistry at Göttingen, 1836–1882.

The Correspondence

Letter 3033

Faraday to Carlo Matteucci

2 November 1855

From Bence Jones (1870a), 2: 365–9

2 Nov 1855

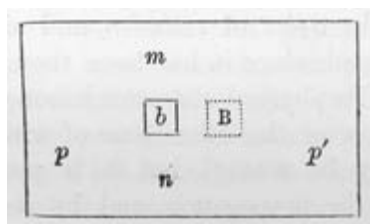
My dear Matteucci,

When I received your last, of October 23¹, I knew that Tyndall would return from the country in a day or two, and so waited until he came. I had before told him of your desire to have a copy of his paper², and I think he said he would send it to you; I have always concluded he did so, and therefore thought it best to continue the same open practice and show him your last letter, note and all. As I expected, he expressed himself greatly obliged by your consideration³, and I have no doubt will think on, and repeat, your form of experiment; but he wished you to have no difficulty on his account. I conclude he is quite assured in his own mind, but does not for a moment object to counter views, or to their publication: and I think feels a little annoyed that you should *imagine for a moment* that he would object to or be embarrassed by your publication⁴. I think in that respect he is of my mind, that we are all liable to error, but that we love the truth, and speak only what at the time we think to be truth; and ought not to take offence when proved to be in error, since the error is not intentional; but be a little humbled, and so turn the correction of the error to good account. I cannot help thinking that there are many apparent differences amongst us, which are not differences in reality. I differ from Tyndall a good deal in phrases, but when I talk with him I do not find that we differ in facts. That phrase *polarity* in its present undefined state is a great mystifier (3307, 3308)⁵. Well! I am content, and I suppose he is, to place our respective views before the world, and there leave them. Although often contradicted, I do not think it worth while reiterating the expressions once set forth; or altering them, until I either see myself in the wrong or misrepresented; and even in the latter case, I let many a misrepresentation pass. Time will do justice in all these cases.

One of your letters asks me, "What do you conceive the nature of the lines of magnetic force to be?" I think it wise not to answer that question by an assumption, and therefore have no further account to give of such physical lines than that is already given in my various papers. See that referred to already in the "Philosophical Magazine" (3301–3305)⁶; and I would ask you to read also 3299, the last paragraph in a paper in the "Philosophical Magazine," June 1852⁷, which expresses truly my present state of mind.

But a physical line of force may be dealt with experimentally, without our *knowing its intimate physical nature*. A ray of light is a physical line of force; it can be proved to be such by experiments made whilst it was thought to be an *emission*, and also by other experiments made since it has been thought to be an *undulation*. Its physical character is not *proved* either by the one view or the other (one of which must be, and both may be wrong), but it is proved by the *time* it takes in propagation, and by its curvatures, inflexions, and physical affections. So with other physical lines of force, as the electric current; we know no more of the physical nature of the electric lines of force than we do of the magnetic lines of force; we fancy, and we form hypotheses, but unless these hypotheses are considered equally likely to be false as true, we had better not form them; and therefore I go with Newton when he speaks of the *physical lines of gravitating force* (3305 note), and leave that part of the subject for the consideration of my readers⁸.

The use of *lines of magnetic force* (without the *physical*) as true representations of nature, is to me delightful, and as yet never failing; and so long as I can read your facts and those of Tyndall, Weber⁹, and others by them, and find they all come into one harmonious whole, without any contradiction, I am content to let the erroneous expressions, by which they *seem* to differ, pass unnoticed. It is only when a fact appears that *they cannot* represent that I feel urged to examination, though that has *not yet* happened. All Tyndall's results are to me simple consequences of the tendency of paramagnetic bodies to go from weaker to stronger places of action, and of diamagnetic bodies to go from stronger to weaker places of action, combined with the true polarity or direction of the lines of force in the places of action. And this reminds me of a case you put in one of your letters, which to me presents no difficulty:—*"a piece of bismuth on which the pole p*



acts suffers an action on the part of the pole p' , which is the same as if the pole p' did not act or was a pole of the contrary name." p , being an S pole, repels b , and sends it from a stronger into a weaker part of the field, i.e. from

from a stron
 b to B ;
 3 pole B is n

then p' being brought up, and being also an S pole, B is no longer the weaker place of action but b ; and hence the bismuth goes back. And that it is the weaker place of action can be shown by a minute magnetic needle or a crystal of bismuth, and in many other ways (3341, &c., especially 3350)¹⁰. But suppose p' is selected, an N pole, then the lines of force between p' and p are greatly strengthened in power, and the small needle, or crystal bismuth, shows it to be so; but still B is no longer a weaker place of power than b , and if the bismuth can only move along the line pp' it must move from B to b , for b is the weakest place of action in that line; but this is a place of unstable equilibrium, and, as you know very well, if it can move in the line mn , it will move either towards m or towards n , as it happens to be on one or the other side of the axial line of the magnetic field.

These principles, or rather laws, explain to me all those movements obtained by Tyndall against which your note is directed, and therefore I do not see in his experiments any proofs of a defined or inverse polarity in bismuth, beyond what we had before. He has worked out *well* the antithetical relations of paramagnetic and diamagnetic bodies; and distinguished mixed actions, which by some have been much confused; but the true nature of polarity, and whether it is the same, or reversed in the two classes, is to my mind not touched. What a quantity I have written to you, all of which has no doubt been in your own mind, and tried by your judgment. Forgive me for intruding it.

Ever truly yours | M. Faraday

I am sorry to refer you to the "Philosophical Magazine." I have a third volume of my "Experimental Researches"¹¹ on my desk waiting for you; it contains them all. I have not yet found a means of sending it.

1. Not found.

2. Tyndall (1855).

3. Tyndall, *Diary*, 29 October 1858, 6a: 208 recorded 'A long & pleasant conversation with Faraday'.

4. On this see Eve and Creasey (1945), 57.

5. Faraday (1855b), ERE[29b], 3307–8.

6. Faraday (1855b), ERE[29b], 3301–5.

7. Faraday (1852d), ERE[29a], 3299.

8. Faraday (1855b), ERE[29b], 3305. A reference to Newton to Bentley, 25 February 1692/3, Turnbull (1961), letter 406 which was quoted to this effect here. Faraday could have read this

letter of Newton's in a number of places including the entry on Newton in *Biographia Britannica*, 6 volumes, London, 1747–1766, 5: 3244.

9. Wilhelm Eduard Weber (1804–1891, DSB). German physicist.

10. Faraday (1855b), ERE[29b], 3350.

11. Faraday (1855c).

Letter 3034

George Gabriel Stokes to Faraday

5 November 1855

From the original in IET MS SC 2

69 Albert St Regents' Park, London | 5 Nov 1855

My dear Sir,

I have arranged to read your paper¹ on the day of the first meeting (Thursday the 15th). I intend only to take the first two sections that day and to take the 3rd section the next.

Would you have the goodness to favour Dr. Sharpey² with an abstract of the paper, such as you would wish to appear in the Proceedings³?

I have been thinking over the setting of phosphorus, and it certainly seems to me that a non-crystalline and unstrained elongated diamagnetic body in a *perfectly* uniform field ought to set along the lines of force. I recollect that Thomson, in the paper in the Cambridge mathematical journal in which he first obtained mathematically the law which you had previously enunciated about going from places of stronger to places of weaker force, stated without demonstration that a sufficiently small elongated body whether paramagnetic or diamagnetic would set along the lines of force⁴. Such I imagine ought to be the way in which an elongated piece of phosphorus would set in an *absolutely* uniform field. That such a set is not observed in experiment is I think no valid argument against the theoretical conclusion. For even if we could obtain a mathematically uniform field the setting force would be so excessively small that it seems doubtful whether it could be experimentally observed, and the field between flat poles is demonstrably not mathematically uniform. The setting force arising from any deviations from uniformity in the field would depend on the first power of the inductive capacity of the substance, whereas the setting force in a *perfectly* uniform field would depend upon its square, and would be as nothing in comparison with the former, the inductive capacity of all diamagnetics being very small. Could a diamagnetic be discovered at all approaching in inductive capacity to soft iron I suppose the setting along the lines of force could be easily observed.

Believe me | Yours very truly | G.G. Stokes

Endorsed by Faraday: Par. 2812. 2813. Exp. Res.⁵

1. Faraday (1856c), ERE30.

2. William Sharpey (1802–1880, ODNB). Secretary of the Royal Society, 1853–1872.

3. The reading was continued on 22 November 1855 when the abstract was published, *Proc. Roy. Soc.*, 1855, 7: 523–6.

4. Thomson (1847).

5. Faraday (1851b), ERE26, 2812–13.

Letter 3035

Faraday to Christian Friedrich Schoenbein

6 November 1855

From the original in UB MS NS 419

Royal Institution | 6 Nov 1855

My dear Schoenbein,

It is quite time I should write you a letter even though I may have nothing to say and yet I surely have something to write though it may not be philosophy for I trust affection will last out philosophy and indeed were it not so I should fear that I was indeed becoming a worn out worthless thing. But your last letter¹ abounded in *all* matter both the *philosophical* & also the *domestic* and *kind* and I thank you heartily for it. That one day in the country how I wish I had been with you – but I could not now walk in Switzerland as I have done in former years. All things suffer a change. May your changes be long deferred for you must be very happy as you are. And so am I but my happiness is of a quieter kind than it used to be and probably more becomes a man 64 years of age: and as we, i.e. my wife & I go on our way together our happiness arises from the same things and we enjoy it together with I hope thankfulness to the giver of every good & perfect gift²[.]

I tried an experiment or two with the oxide of silver & obtained some results but not equal to those you sent it nor was it to be expected that I should reach the results of a Master in this subject[.] Your accounts & observations are most interesting & exciting but I dare not try to pursue the subject for even the matter I have in Magnetism is often too much for me & I am obliged to lay it by for a while so that I am forbidden by nature to take up any new series of thought. But that Ozone that oxygen which makes up more than half the weight of the world, what a wonderful thing it is and yet I think we are only at the beginning of the knowledge of its wonders.

By the bye your letters often contain much that I should like others here to see and I want to ask you whether there is any objection to my shewing them to Tyndall and letting him as one of the Editors of the Phil Mag print any of the philosophical parts that he may select in the Magazine. There are full three pages of your last which if I were an Editor I should have selected:– at the same time you must not in any way alter the pleasant tone & current of your epistles – or else I shall be a great loser.

I cannot now remember how I received your letter and whether I saw M. Schweitzer³ – I rather think not but whether I was out of town or whether he *sent* me the letter by some one I am unable to call to mind. – I have received

no parcels for you as yet, but will take care of any that come. I sent you Vol III of Experimental Researches⁴ by Mr. Twining & have no doubt it has reached you – but I have not seen Mr. Twining since his return from your country^[1]

The General board of health here published a report on the Cholera epidemic of 1854⁵ and since that a thick 8vo volume of Appendix. In the latter I am glad to see they refer to Ozone in several places p. p. 71. 89. 103⁶ and of course to you, but whether the observations (by Dr. Moffat⁷) are well made⁸ and considered I do not know – in any case it indicates that ozone is gaining a growing attention amongst medical men.

My kindest remembrances to Madame Schoenbein & to those whom by a stretch of imagination I strive to see around here [sic] i.e. to the party of the day's excursions and my very kind remembrances to M. Wiedemann also. It is delightful to see thinking workers rise up in Science. Believe me to be, my dear Schoenbein

Your faithful friend | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | on the Rhine

1. Schoenbein to Faraday, 26 May 1855, letter 2985, volume 4.

2. James 1: 17.

3. Unidentified.

4. Faraday (1855c).

5. *Parliamentary Papers*, 1854–5 [1980], XXI.

6. *Parliamentary Papers*, 1854–5 [1996], XXI, pp. 71, 89, 103.

7. Thomas Moffat (d.1882, age 69, *Medical Directory*, 1883, p. 1375). Physician at Harwarden.

8. Moffat's work on ozone and his observations at Harwarden were referred to in *Parliamentary Papers*, 1854–5 [1996], XXI, pp. 71, 89, 103.

Letter 3036

George Gabriel Stokes to Faraday

7 November 1855

From the original in IET MS SC 2

69 Albert Street Regent's Park, London | 7 Nov 1855

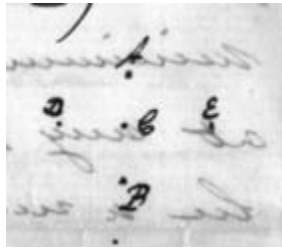
My dear Sir,

I should not have ventured to write as I did, considering how deeply you have thought over the subject and how little I have attended to it, were it not that in our conversation I understood you to regard the so-supposed setting of a diamagnetic body in a uniform field across the lines of force as a mystery. However I did not without consideration; and having carefully considered the matter since I see no reason to change my opinion.

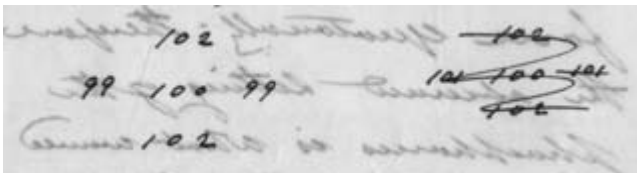
I do not question the accuracy of your experiment, Exp. R. 2812¹, nor object to the use of protosulphate of iron, but I do not see in the result anything more than the effect of the tendency of each particle of the phosphorus to go from places of stronger to places of weaker force.

I have obtained the following result mathematically.

Let a field of force be symmetrical about an axis; then if in travelling along the axis of force be a minimum or maximum at any point, it must be a maximum or minimum in travelling equatorially on arriving at the same point, and the variation of the square of the force in receding from that point axially is double the variation, of contrary sign, in receding from it equatorially. Thus, if C be the centre of a field which is symmetrical, not only round a horizontal axis, but also on opposite sides of a vertical plane perpendicular to that axis (i.e. when the north and south ends are alike) C must be a place of strongest or weakest force



in comparing any points along the axis, although it may be that the variation of force is but slight; and if A, B, C, D [sic] be 4 points near to C and equidistant from it, two A, B in the axial, and two D, E in the equatorial direction, the square of the force at D or E is half as much less or greater than at C as the square of the force at A or B is greater or less than at C. Thus the numbers might be suppose



If I recollect right Thomson at Liverpool² enunciated the result which I have mentioned, that if the force be at any point of the axis a maxm or minm going axially it must be a minm or maxm going equatorially. (See note B at end).

Now in your experiment the phosphorus as a whole went axially to the centre of the field; therefore the centre must have been a place of weakest force axially see note A at end; therefore it must have been a place of strongest force

equatorially; therefore the observed setting of the phosphorus is what would result from the known law about going to places of weakest force; therefore we are not entitled from the result to draw any new conclusion.

I do not think the experiment need be repeated, but if you are not satisfied with the explanation I have given it might be worth while to remount the apparatus, to suspend a round piece of phosphorus by means of a lever and cocoon silk, so as to be free to move in the middle plane across the lines of force, and see whether when slightly displaced from the axis it would not tend to recede further from it.

By considering the mathematics of a body in a magnetic field, the body being composed of particles which severally tend to go from places of stronger to places of weaker force, but which do not sensibly influence one another, I arrived at the following conclusion.

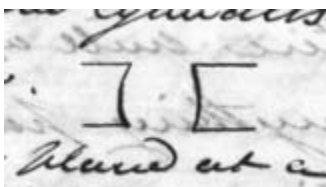
If a small non-crystalline elongated body be suspended horizontally, so as to be free to turn round a vertical axis passing through its centre of gravity and be placed in an arbitrary magnetic field, there are two rectangular directions in one or other of which it will set, according as it is paramagnetic or diamagnetic. These directions have no immediate relation to the directions of the lines of force, though in ordinary fields they are, approximately at least, parallel and perpendicular to these lines. There may exist in the field what I will call dead points, that is, points where the setting force vanishes, and there may exist what I will call points of rest, that is, points where the whole force (or rather its square) is ultimately constant in passing to a consecutive point, so that the points of rest are positions of equilibrium of a small body free to move in all horizontal directions. But the points of rest (if any) are not in general dead points, and the dead points (if any) are not in general points of rest. Thus with pointed poles in the same horizontal plane



the centre of the field would be a point of rest, but not a dead point. A dead point would be to the setting of the body something like what a magnetic pole of the earth is to the setting of the ordinary needle. In going once completely round it at a little distance, the direction of either set (i.e. para- or diamagnetic) would turn round through two right angles only. The lines of force would in general show nothing particular in the neighbourhood of a dead point, and thus we should have the phenomenon of a set taking place in all sorts of ways relating to the lines of force.

I don't know whether there will appear to you anything paradoxical in this, or whether it will appear all natural. In the former case it might be worth while to investigate the thing further, so as to try to point out the mode of realising a dead point in experiment.

I have thought of a way in which the tendency of a bar, whether para- or diamagnetic, to set parallel to the lines of force, in consequence of mutual action, might perhaps be rendered sensible in experiment. Let two poles be prepared which I believe had best be, at the ends of the form of broad cylinders rendered concave.



If the poles were placed at a distance from each other, the centre of the field would be a point of axially minimum and therefore equatorially maximum force, but when the poles were near together the centre of the field would be a point axially maximum and equatorially minimum force. In the former case a paramagnetic bar would set axially and a diamagnetic bar equatorially; in the latter case the settings would be reserved. Call the distance of the poles at which the transition takes place the critical distance, and suppose it determined experimentally. If over and above the set due to the tendency of each particle to go to places of stronger or weaker force in the otherwise undisturbed field (i.e. not deemed to be sensibly disturbed by all the other particles) there exists a cause of set (mutual disturbance) tending to make a bar whether para- or diamagnetic set along the lines of force, and therefore axially, it is plain that the axial setting is favoured thereby, and therefore the critical distance as determined by the use of a paramagnetic bar ought to come out somewhat less than as determined by the use of a diamagnetic bar, and less with a weakly than with a powerfully diamagnetic bar. Of course all the bars ought to be as nearly as possible of the same size and shape.

Although this method seems to me sound in principle, I doubt whether the forces would not be too small to allow the result to be sensible in experiment, except perhaps in the case of iron.

Note A. p. 6 I presume at least the effect was due to this cause, and not any sensible reaction of the phosphorus on the iron poles[.]

Note B. On second thoughts I believe that what Thomson said had reference to a single flat pole and was not quite the same as this.

Believe me | Yours very truly | G.G. Stokes

1. Faraday (1851b), ERE 26, 2812. This is the reference with which Faraday endorsed Stokes's previous letter, 3034.

2. This would appear to be a reference to an otherwise unrecorded intervention by Thomson following a paper by Tyndall on the subject (Tyndall (1854b)) to the Mathematical and Physical Science Section (of which Stokes was President) at the British Association meeting held in Liverpool in 1854.

Letter 3037

John Stevens Henslow¹ to Faraday

8 November 1855

From the original in IET MS SC 2

Hitcham, Bildeston, Suffolk | 8 Nov 1855

My dear Faraday,

I had no sooner intimated my intention of giving a lecture at Ipswich this season on "Quartz – its abundance in nature & applications in the Arts" than I accidentally stumbled on two notices, one in the Art. Union & another in Chambers Journal² – stating that Mr. Barlow had given a Lecture at the Royal Institution on these subjects – Can you tell me whether he has published his lecture³? (Is Kuhlmann's⁴ paper on soluble silicates published⁵?) Perhaps he might not be disinclined, if I were to call next week when in town, to show me any interesting specimens or illustrations he may possess upon the subject – I am not sufficiently acquainted with him to take the liberty of writing – but as you must know him well, if you think he would not be unlikely to indulge me may I ask you to pop the question to him? I had fully intended, when in Paris lately, to have procured a specimen of Aluminium for the Ipswich Museum – Mr. Brodie kindly gave me the fabricators address – But really we had so much to see in a short time that I could find no opportunity of getting to the shop – Perhaps you can put me on the right scent for procuring a small lump to add to our Illustration of Elementary Substances – Some man called yesterday when I was at the Museum to ask my opinion of some well worked clay he had prepared from one of his fields, & which he fancied might be very serviceable for the manufacture of Aluminium, of which he had read some recent accounts, & supposed (I believe) he had hit upon a vein which was to make his fortune! I was sorry to undeceive him by revealing the facts that his clay was not better than plenty elsewhere, & that the extraction of Aluminium is a costly process –

Believe me | Very truly Yours | J.S. Henslow

1. John Stevens Henslow (1796–1861, ODNB). Rector of Hitcham, 1837–1861. Professor of Botany at Cambridge University, 1825–1861. President of the Ipswich Museum, 1850–1861.

2. *Chambers J.*, 22 July 1854, 2: 61.

3. Barlow (1854), Friday Evening Discourse of 7 April 1854.

4. Charles Frédéric Kuhlmann (1803–1881, DBF). French chemist.

5. Kuhlmann (1855).

Letter 3038

Jean-Baptiste-André Dumas to Faraday

c.10 November 1855¹

From the original in IET MS SC 2

Ministère de l'Instruction Publique et des Cultes.
Paris, le 185

Mon cher et illustre ami,

Vous n'avez pas voulu nous venir voir à l'occasion de l'exposition universelle, ce serait très mal, si nous n'y avions trouvé l'occasion de Vous prouver qu'absent ou présent vous êtes toujours l'objet de notre admiration et de notre affection la plus profonde.

La classe de chimie et de physique sur ma proposition a demandé qu'une Grande médaille d'honneur fut décernée à l'auteur des découvertes qui ont porté une lumière si vive et si féconde dans les phénomènes autrefois si obscurs de l'électricité².

Accueillie avec acclamation, cette proposition a été confirmée dans la dernière séance du conseil des présidents cette nuit [sic].

Il m'est d'autant plus doux de vous transmettre cette bonne nouvelle qu'elle me donne quelque espoir de vous voir.

L'Empereur aimerait, je le sais, que vous lui donniez l'occasion de vous remettre lui-même une marque de la reconnaissance du monde savant et industriel, qui est en même temps un témoignage de l'estime particulière qu'il vous porte depuis bien longtemps.

Veuillez, mon cher et illustre ami, croire que rien ne peut ajouter à mes sentimens pour vous et que je suis le plus heureux de tous ceux qui ont pris part à cette délibération.

Tout à vous | J. Dumas

Mille choses bien respectueuses à Madame Faraday.

TRANSLATION

Ministry of Public Education and Religion,
Paris, on 185

My dear and illustrious friend,

As you were unable to come to see us on the occasion of the universal exhibition, it would have been very bad if we had not found an opportunity to prove to you that, absent or present, you are always the object of our most profound admiration and affection.

The chemistry and physics class, at my suggestion, asked that a Grand Medal of Honour should be awarded to the author of discoveries, which have thrown such a lively and fertile light on the phenomena of electricity that were previously so obscure².

Welcomed with unanimity, this proposal was confirmed tonight at the meeting of the council of presidents.

It is all the more pleasant for me to pass this good news to you because it gives me some hope of seeing you.

The Emperor would be very pleased, I know, if you gave him the opportunity for him to present personally a sign of the gratitude of the world of savants and industry, which is at the same time a testimony of the particular esteem that he has had for you for a very long time.

Please, my dear and illustrious friend, believe that nothing can add to my sentiments for you and that I am the happiest of all those who have taken part in this decision.

Yours sincerely | J. Dumas

A thousand respectful greetings to Mrs. Faraday.

1. Dated on the basis that Faraday received this letter in Brighton on 14 November 1855 and replied the following day. Letter 3044.

2. This was announced in *Moniteur*, 16 November 1855, p. 1270, col. d.

Letter 3039

George Gabriel Stokes to Faraday

13 November 1855

From the original in IET MS SC 2

69 Albert Street Regent's Park, London | 13 Nov 1855

My dear Sir,

I forgot to say in my last letter¹ that there was no particular hurry about the abstract of the paper².

You will probably have found before this on reading my letter again that it was not *independently* but *in consequence*, and only in consequence, of mutual influence of the parts of the bar that I asserted that a non-magnecrystallic bar would set in a mathematically uniform field, and that, along the lines of force, whether the bar was para- or diamagnetic.

On referring to Thomson's paper in the 2nd vol. of the Cambridge & Dublin Maths Journal³, I find that he *has*, as I supposed, stated that such a bar would set as I have said. He has merely stated the result, but I have no doubt it was to the mutual influence of the parts of the bar that he attributed the setting.

You said it was a mystery to you why a diamagnetic bar should appear less obstacle to the passage of the lines of force when it was perpendicular than when it was parallel to them. I asked if the fact was so, and you referred me to the experiment with the phosphorus. But after having maturely



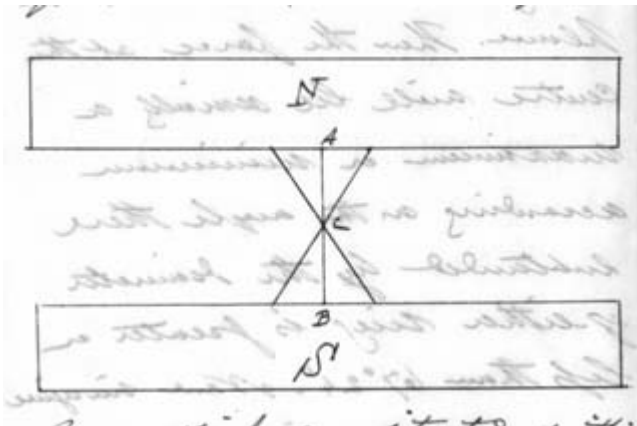
Plate 2. George Gabriel Stokes. From RI MS Conybeare album, f. 8.

considered the thing I believe the true way of explaining the mystery is by denying the fact. I do not of course mean questioning the result obtained with the phosphorus. That was no doubt all right; but the setting of the phosphorus is explicable otherwise, namely from the non-uniformity of the field of force.

I think theory may be trusted for the mode of setting of a bar in a perfectly uniform field. At the same time on careful consideration the experiment I proposed seems to me quite sound in principle, though the forces may be too feeble to allow the result to be exhibited experimentally. I think it likely enough that by care a distance between the poles might be found such that a little bar of soft iron and a bar of phosphorus placed successively in the centre of the field would set axially; but I doubt whether the setting in the same direction of a bar of phosphorus in a solution of protosulphate of iron and of, suppose, a tube of protosulphate of iron in water would not be a result too delicate to obtain.

I have obtained the following result.

Let there be a pair of rings of poles, north and south, symmetrically situated with respect to a plane bisecting at right angles the line joining the centres of the rings, the planes of the rings being both parallel to this plane. Then the force at the centre will be axially a maximum or minimum according as the angle there subtended by the diameter of either ring is greater or less than $67^{\circ}24'$. Now imagine a pair of opposite flat poles divided similarly into concentric rings having their centres in the axis. We may apply the above result to each pair of opposite rings. Hence if N, S be two flat poles, AB the axis, and C the centre of the field, and if round AB we describe a cone having its vertex at C, and its semi-vertical angle equal to $33^{\circ}42'$, those points of the



poles which are situated within the cone will tend to make the force at C axially a minimum, and those parts which are situated outside the cone will tend to make it axially a maximum. If the poles were infinite, and uniformly magnetised, the two tendencies would exactly neutralise each other. Bearing this in mind, and referring to your sketch, I think we might have predicted that the force would be axially a minimum in the centre of the field.

By rendering the poles concave, it seems to me that we could so weaken the effect of those parts of the poles which lie within a cone of $67^{\circ}24'$ as to cause the transition from axial minimum to axial maxm to take place when the poles were moved from a distance apart greater than to one less than a certain very manageable distance.

Believe me | Yours very truly | G.G. Stokes
Professor Faraday | &c &c &c

1. Letter 3036.
2. That is the abstract of Faraday (1856c), ERE30 published in *Proc. Roy. Soc.*, 1855, 7: 523–6.
3. Thomson (1847).

Letter 3040

George Gabriel Stokes to Faraday

13 November 1855

From the original in IET MS SC 2

69 Albert Street Regent's Park, London | 13 Nov 1855

My dear Sir,

As I was walking into town after I had posted your letter¹ I thought I recollected having made an error through inadvertence. The tangent of the $\frac{1}{2}$ vertical angle of the cone is $\sqrt{\frac{2}{3}}$. I wrote down the logs of 2 and 3 from memory and subtracted but forgot to divide by 2. The true angle is $39^{\circ}14'$ giving for the whole angle of the cone $78^{\circ}28'$. This emendation as far as it goes favours the explanation which I gave of the cause of the setting of the phosphorus in your experiment.

As I don't know whether simply Brighton will find you I direct to the Royal Institution.

Yours very truly | G.G. Stokes
Professor Faraday

1. Letter 3039.

Letter 3041**George Biddell Airy to Faraday****14 November 1855****From the original in IET MS SC 2**

Royal Observatory Greenwich | 14 Nov 1855

My dear Sir,

Will you let me ask you instruction on a chemical matter.

We have photographic registers of the movements of our magnets, of which I desire to multiply copies. This we can do photographically, but the expence is alarming. In the largest number, I do not think that we can make them for less than 6d a leaf, small in itself, but large when multiplied annually by 22,000.

Now would it be possible for the action of sun-light through our primaries or secondaries to produce an action on the surface of any metal plates &c which would make them competent to print like zincographs?

Or can you suggest any other and wholesale way of producing say 30 copies of every photographic sheet?

I am, my dear Sir | Yours very truly | G.B. Airy

Professor Faraday | &c &c &c

Letter 3042**Jean-Baptiste-André Dumas to Faraday****14 November 1855¹****From the original in IET MS SC 2**

Mon cher monsieur et ami,

Je suppose que vous n'avez pas reçu la petite lettre que j'ai eu la satisfaction de vous écrire², il y a quelques jours, et quoique je sois très incertain, en conséquence de savoir si celle ci vous trouvera à Londres, Je veux bien que vous sachiez quelle est la part que j'ai prise et où cette part s'arrête dans les deux faits qui vous concernent au sujet de l'exposition.

J'ai émis le vœu que vous fussiez porté pour la grande médaille d'honneur et j'ai donné comme spécification vos *travaux sur l'Electricité*. Il me semblait inutile d'y ajouter un épithète. Le commis chargé de la correction du travail a pensé que cela ne suffisait pas pour justifier la grande médaille et il y a ajouté de son chef le mot *importants*. Je crois bien, *importants*³, quelle intelligence! Ce commis eut sans doute cru nécessaire de mettre dans une dépêche: annonce de la destruction de Sébastopol, *nouvelle importante*.

Mais, voilà bien pire ce soir. J'avais émis le vœu que la croix de commandeur de la Légion d'honneur vous fut donnée, comme on l'a donnée à Lord hereford⁴ [sic] et Lord Arsbashton⁵ [sic], et l'empereur l'a décidé avec la meilleure grace⁶.

J'en étais très heureux, quand je trouve ce soir dans le journal qui donne le tableau des récompenses une désignation qui placée à côté de votre nom si illustre et si élevé me cause une vive peine⁷.

Je crains qu'elle ne vous cause à vous-même quelque ennui. Ce qui est sûr c'est que le commis qui a fait cette nouvelle sottise n'y a pas mis de la méchante intention, bien au contraire. Il a trouvé très admirable de prouver qu'un ouvrier pouvait s'élever très haut. Mais je ne me serais jamais imaginé qu'on aurait été placer [sic] dans un décret ce que nous appelons en France un cancan de portier.

Ce qu'il y a de vrai, cher monsieur et ami, c'est que le Jury a voulu vous donner la grande médaille d'honneur et l'Empereur la croix de commandeur.

La lettre ne regarde ni le Jury, ni l'Empereur. Je serais très heureux de penser que vous n'en êtes pas contrarié et encore plus d'apprendre que le double témoignage dont vous avez été l'objet vous a fait plaisir

Tous mes Compliments et toutes mes amitiés | J. Dumas

TRANSLATION

My dear Sir and friend,

I suppose you have not received my little letter which I had the satisfaction of writing to you a few days ago², and although I am uncertain, consequently, if this one will find you in London, I would like you to know what part I have played and where this part finished in both the events which concern you regarding the exhibition.

I expressed the wish that you should be nominated for the Grand Medal of Honour and I gave as specification your *works on electricity*. It seemed unnecessary to me to add an explanation. The clerk responsible for the preparation of the work thought that was not sufficient to justify the Grand Medal and added, on his own initiative, the word *important*³. Of course *important*, what intelligence! This clerk would no doubt have believed it necessary to write in a dispatch: announcement of the destruction of Sebastopol, *important news*.

But worse was to come this evening. I had expressed the wish that the Cross of the Commander of Legion of Honour should be given to you, as it was given to Lord Hertford⁴ and Lord Ashburton⁵, and the Emperor most willingly agreed to it⁶.

I was very pleased about this until I found this evening, in the journal which gives the table of honours, a description, which placed beside your illustrious and exalted name, causes me real pain⁷.

I fear lest this should be a cause of annoyance to you. What is certain is that the clerk who made this foolish change did not put it there with evil intent, quite the contrary. He found proving that a worker could rise so high most admirable. But I would never have imagined that one would have placed in a decree what we call in France porter's gossip.

What is true dear sir and friend is that the jury wanted to give you the Grand Medal of Honour and the Emperor the Cross of Commander.

The letter concerns neither the jury nor the Emperor. I would be very happy to think you are not angry and even more so to learn that the double testimony of which you have been the object has given you pleasure.

All my compliments and all my best wishes | J. Dumas

1. Dated that on the basis that this was written the same day as the decree making Faraday a Commander of the Legion of Honour.
2. Letter 3038.
3. As reported in *Moniteur*, 16 November 1855, p. 1270, col. d.
4. Richard Seymour-Conway, 4th Marquess of Hertford (1800–1870, CP). Diplomat and art collector.
5. William Bingham Baring, 2nd Baron Ashburton (1799–1864, ODNB). Conservative politician.
6. The decree from the Tuileries, dated 14 November 1855, was published in *Moniteur*, 17 November 1855, p. 1273, col. d.
7. *Ibid.* mentioned that Faraday was formerly a working bookbinder.

Letter 3043

Duc de Plaisance to Faraday

14 November 1855

From the original in RS MS 241, f. 141

Grande Chancellerie, (Angleterre)
Ordre Impérial de la Légion d'Honneur,
Nomination de Commandeur

S.M. l'Empereur, par Décret du quatorze novembre mil huit cent cinquante et cinq, a promu au grade de Commandeur de l'Ordre Impérial de la Légion d'honneur M | Faraday, à Londres. | Services éminents rendus à la Science (Exposant) | pour prendre rang à dater du même jour.
Paris, le 14 novembre 1855

Le Grand Chancelier de l'Ordre Impérial de la Légion d'Honneur | Duc de Plaisance

TRANSLATION

Grand Chancery, (England)
Imperial Order of the Legion of Honour,
Nomination of Commander

H.M. the Emperor, by decree of 14 November 1855, has promoted to the rank of the Imperial Order Commander of the Legion of Honour Mr. Faraday, in London, for eminent services to science (Exhibitor), to take his place from the same date.

Paris, 14 November 1855

The Grand Chancellor of the Imperial order of the Legion of Honour |
Duc de Plaisance

Letter 3044

Faraday to Jean-Baptiste-André Dumas

15 November 1855

From the original in AS MS

A Monsieur | Monsieur le M. Dumas | &c &c &c &c
Brighton | 15 Nov 1855

My dear & most esteemed friend,

You can hardly conceive the pleasure your letter¹ gave me;— the sight of the hand writing, the old kind remembrances, and the present affection — all contributed to move me greatly. The honor had little or nothing to do with the matter;— it was the feeling that you had not forgotten me, but that I was on your mind as I had been;— and for this I must truly thank you:— and before I leave this ground of true pleasure must ask you to join it with my most respectful remembrance to Madame Dumas; to whom also my dear wife sends her kindest remembrances as well as to yourself.

I have not been well; and having been ordered out of town, did not receive your letter until yesterday and as it bore no date, was not aware that I ought to have written on the instant. This morning I have received a letter from Mr. Cole², which leads me to suppose the great day of the Exposition is to day. In any case I could not have been present.

You must not think that I do not esteem *very highly* the great honor, which you, and the Council have done me; and could I have been present on the great occasion, and so had the opportunity of beholding the Emperor, a flood of remembrances would have come back upon me (as they do now) of his extreme kindness on former occasions³. I do not suppose that he can ever think of me; there is, now, no occasion; but if the circumstances now existent should cause any sound of my name before him in your presence, I hope you will (if proper) express to him the deep feelings of my heart; both in former things and in the present happy & I trust enduring union of France & England under his reign[.]

I am My dear friend | Most truly Yours | M. Faraday

1. Letter 3038.

2. Henry Cole (1808–1882, ODNB). Secretary of the Department of Science and Art and General Commissioner for Britain at the Paris exhibition.

3. For example see Napoleon to Faraday, 1840 and 23 May 1843, letters 1228, volume 2, and 1496, volume 3, respectively.

Letter 3045**Faraday to George Biddell Airy****16 November 1855****From the original in RGO6/678, f. 351**

65 Kings Road, Brighton | 16 Nov 1855

Dear Sir,

I am sorry I cannot help you in the Photographic matter¹ – but I have no experience;– and no useful thought occurs to me. As regards transfer and engraving you have in Mr. Brooke all that is known in that direction[.]

Every Truly Yours | M. Faraday

G.B. Airy Esqr | &c &c &c

1. See letter 3041.

Letter 3046**Faraday to James David Forbes****17 November 1855****From the original in RI MS F1 A23**

Brighton | 17 Nov 1855

My dear Sir,

I am sorry I have no index to the continued researches:– but the circumstances attendant upon Vols II & III 8vo¹. were not the same as for Vol I². When I reach home I dare say I shall find the XII & XIII³ that you send me. Series XXX⁴ is in the hands of the Royal Society and I expect in due time to have a copy to send you[.]

Ever Truly Yours | M. Faraday

Prof J.D. Forbes | &c &c &c

1. Faraday (1844, 1855c).
2. Faraday (1839).
3. Faraday (1838b, c), ERE12 and 13.
4. Faraday (1856c), ERE30.

Letter 3047**Faraday to Roderick Impey Murchison****17 November 1855****From the original in GS MS M/F4/4**

Brighton | 17 Nov 1855

My dear Sir Roderick,

I am as above for health – I shall be in town this day fortnight & probably not before. If then I can be useful at the B.M.¹ I shall be happy. But how

is it that they ask for advice *after* the matter is arranged & not before. It may be that I should not have hesitated to give a free opinion before hand and may not like to do so when the matter is complicated by proceedings taken[.]

Ever My dear Sir Roderick | Yours Truly | M. Faraday

Endorsed by Murchison: British Museum Asphalt

1. The Trustees of the British Museum at their meeting on 10 November 1855 asked that Faraday be consulted about the use of asphalt for the new Round Reading Room. (Trustee Minutes in BM CA).

Letter 3048

Faraday to Jean-Baptiste-André Dumas

17 November 1855

From the original in AS MS

Royal Institution | Saturday Evening, 17 Nov 1855

My very dear friend,

Coming home for an hour or two only I find your last letter¹ & hasten to reply by the post. I trust you have my former one² from Brighton by this time. Do not think for a moment that any expression meant kindly can be to me any thing but a pleasure – I do not know to what you refer. I should be grieved at any word touching my honor or affecting my moral character but any thing else is to me perfectly indifferent. What I chiefly & above all rejoice in is your good opinion & great kindness in the first place & then the added approbation & aid of others upon your acts. To the Emperor I owe a deep debt of gratitude for I do not think that in this case he acts merely upon the conclusions of others but accompanies his most gracious act with a thought of good will on his own part. It is considerations of this nature which makes the honor so acceptable to me – for its own sake it is precious for the sake of these associated feelings it is very dear[.]

Ever My dear friend | Yours most truly | M. Faraday

A Monsieur | Monsieur Dumas | &c &c &c &c

Address: A Monsieur | Monsieur Dumas | &c &c &c &c | l'Ministre de l'instructions publiques | &c &c &c | à Paris

1. Letter 3042.

2. Letter 3044.

Letter 3049**Pierre Guillaume Frédéric Le Play to Faraday¹****22 November 1855****From the original in RS MS 241, f. 141**

Commission Impériale, de, l'Exposition Universelle,
 Commissariat-Général. Palais de l'Industrie
 (Champs Elysées), Paris | le 22 Nov 1855

Monsieur,

J'ai l'honneur de vous prier d'adresser *au bureau de la Comptabilité de la Commission Impériale, au Palais de l'Exposition*, un reçu de la grande médaille d'honneur qui vous a été remise par S.M. l'Empereur dans la séance solennelle du 15 de ce mois².

Recevez, Monsieur, l'assurance de mes sentiments les plus distingués.
 Le Commissaire Général | F. Le Play

TRANSLATION

Commission Impériale, de, l'Exposition Universelle,
 Commissariat-Général, Palais de l'Industrie
 (Champs Elysées), Paris | 22 Nov 1855

Sir,

I have the honour of sending you from the *office of the Accounts Department of the Imperial Commission, in the Palace of the Exhibition*, the Grand Medal of Honour which was awarded to you by H.M. the Emperor at the solemn meeting of the 15th of this month².

Receive, Sir, the assurance of my most distinguished sentiments.
 The Commissioner General | F. Le Play

1. Pierre Guillaume Frédéric Le Play (1806–1882, NBU). French engineer and Commissioner General of the 1855 Paris Universal Exhibition.

2. See letter 3044.

Letter 3050**Charles Etler¹ to Faraday****22 November 1855****From the original in IET MS SC 2**

South Barracks, Halifax NS | 22 Nov 1855

Sir,

Imagining that I have discovered an easy method of measuring the distance and size of distant objects I have taken the liberty of communicating it to you, partly because I have no friends of my own who could take the matter in hand but chiefly, because I can confide in your ability to judge of its merits. The idea of constructing a something which I have presumptuously

termed A Telemeter occurred to me about twelve months ago when engaged in writing the enclosed essay²: but it was only a few weeks since that I had opportunity of verifying my suppositions, which I did, although very imperfectly, by means of an old tin tube about 12 inches in length and 2 in diameter, in which was inserted a slide fitting the tube and having in it a small aperture through which the extremities of the object to be measured must be seen. The size of this aperture may be either given or unknown. In the figure given in explanation of the principles of the instrument aa, bb, cc, and rr represent the aperture, and the numbers on Cy the scale of inches marked on the tube. A want of funds has prevented me from proving whether or not lenses may be adapted to the instrument.

I am a young man belonging to Somersetshire. About $4\frac{1}{2}$ years ago having become very unsettled in my religious opinion I left home and came to America, in various parts of which I have since been wandering. My father is a carpenter now living in London. He is an old man and has a family dependent upon his exertions for their daily bread.

When in Canada about 18 months ago, I began a Poem with the intention of using it as a means of procuring me some kind of situation on my return to England; but finding my circumstances too indifferently suited for carrying out my design and with the same end in view, I attempted to write a short story, and chose for the sake of novelty and originality, *The Aurora Borealis* for its subject. Of the absolute merits of the Essay my ignorance of Science prevents me from forming an opinion, I have only to affirm that little as it is, it has cost me many sleepless hours, and that although I am little better than an ignoramus in scientific matters I consider myself as much entitled to sit in judgement on Nature as some of those philosophers who assert that the polar regions must needs be very hot in consequence of the conveyence of the magnetic rays, and that the sun was seen to emit flames 20,000 miles in height. I concluded it rather abruptly for the more I examined the subject the less able did I feel to proceed, and the more apparent did my ignorance become; but I fondly hoped that someday I might become a Gibeonite³ in the Temple of Science so that I might be able to peer still further into nature's mysteries.

Arriving in Nova Scotia I found myself a few months since in such straightened circumstances that I was fain to enlist and am at present known as Charles Etler. Grenr. 76th Regt. and since my hopes of returning home have now forsaken me, I have at least ventured to address you, trusting that you will give me credit for an honest motive, and for a diffidence, which although unexpressed, is not the less felt; for I know full well the fallacy of those expectations which imagination and poverty are apt to excite. If my speculations shall have the effect of causing the fire at home to burn more cheerfully during the ensuing winter I shall be happy, and perhaps encouraged to work my way through difficulties which at present indeed appear insurmountable.

Endorsed by Faraday: From a soldier | Halifax | Dec 1855

Address: Professor Faraday | Care of Messrs Bradbury⁵ & Evans⁶ | No 11 Bouverie Street | London | England

1. Unidentified.
2. 'Panurgon, or A short Essay on Cause & Effect Being an attempt to explain the nature and causes of Light, Heat, Gravitation, Terrestrial Magnetism &c', IET MS SC 2.
3. 2 Samuel 21.
4. Unidentified.
5. William Bradbury (1800–1869, ODNB). Printer of 11 Bouverie Street, London.
6. Frederick Mullett Evans (1803–1870, ODNB). Printer of 11 Bouverie Street, London.

Letter 3051

Faraday to William Hughes¹

27 November 1855

From the original in SI D MS 554 A

Brighton | 27 Nov 1855

Dear Sir,

I return the accounts – Newmans is much wrong by £13 over charge. I conclude the bill for 1854 was paid – but there is only a receipt for one part, attached to it, the others I have no doubt are right[.]

Hoping you are quite well

I remain | Very Truly Yours | M. Faraday

Mr. Hughes

1. William Hughes (d.1870, age 64, GRO). Clerk of accounts of the Royal Institution, 1846–1870. RI MM, 30 November 1846, 9: 420; 6 February 1871, 12: 355.

Letter 3052

Third Earl of Rosse¹ to Faraday

29 November 1855

From the original in IET MS SC 2

The Castle Parsonstown | 29 Nov 1855

Dear Faraday,

I perceive by the papers that my name is in the list of Knights; and that you are our worthy general². Should I write to any one to return thanks and if so to whom? I am here quite out of the world and have no means of knowing what others have done, will you therefore kindly let me know what is right and proper[.]

Believe me [to] be | Truly Yours | Rosse

1. William Parsons, 3rd Earl of Rosse (1800–1867, ODNB). Irish astronomer. President of the Royal Society, 1848–1854.
2. *The Times*, 20 November 1855, p. 8, col. c reported that Faraday had been made a Commander of the Legion of Honour and Rosse a Knight.

Letter 3053**Faraday to George Biddell Airy****30 November 1855****From the original in RGO6/678, f. 352**

R Institution | 30 Nov 1855

Dear Sir,

I met with a gentleman in the train who thinks there must be some mistake about the high price of copying your registers¹. I promised to look for your note & send it to him but I cannot find the note. He seems to understand the matter & would have great pleasure in giving you information if you would drop him a note Mr. H. Medlock². 20 Great Marlborough Street Regent St³[.]

Ever Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. See letters 3041 and 3045.

2. Henry Medlock (1825–1875, B6). Industrial chemist.

3. Airy to Medlock, 1 December 1855, RGO6/678, f. 353.

Letter 3054**Christian Friedrich Schoenbein to Faraday****30 November 1855****From the original in UB MS NS 420**

Bâle | 30 Nov 1855

My dear Faraday,

Having these many months heard any thing neither from nor of you I had already begun growing anxious about the state of your health when to my great satisfaction I was released from my anxiety by your kind letter of the 6th instant¹ which has made upon my mind the impression that you are a perfectly well doing man. May it please kind providence to preserve you both to your friends and Science for many years to come! This is one of my most ardent wishes, which I cannot help expressing you over and over again; for I see that you have as yet much work to do, many a problem to solve and more than one mystery to divulge to the philosophical world. Indeed, we cannot yet do without the seer and prophet of nature.— Since I wrote you last, we for the first time had here the most unwelcome visit of the Cholera, but thank God, its stay was short and my family as well as my friends were left untouched by it. During the summer Mrs. Schoenbein and the girls spent a couple of months in the Jura mountains, where I joined them now and then to their great satisfaction, the father's disposition for rambling on green and wooded hills being their's too, and certainly we did not fail gratifying

it to a great extent, walking very often for six or eight consecutive hours together. You will perhaps smile when I tell you that Miss Schoenbein has of late become a very zealous english scholar, reading, writing and speaking away your native tongue in rather a fluent and elegant style. But pray, do not imagine the father to have any part in the proficiency of the daughter, for you must be aware that a parent always proves a very bad master to his children. Certainly on being sometimes called upon to look over the tasks of the young scholar's, I have an occasion to make some use of the little bit of English I am as yet master of. Number 2 and 3 having caught that liking from her eldest sister, have become pupils of her's and are going on well enough in their studies. As to me, I can, of course, have no objection to that taste for an outlandish tongue and literature, being myself somewhat suspected of "Anglomanie". Once speaking of my daughter's accomplishments I may as well add that she is a partly good musician too, playing the piano not only with facility but I think also with some taste. Being myself a great admirer of the heavenly art of music and after my morning's schoolmastering sometimes feeling inclined to take a little rest on the sofa, I in a half dozing state of mind listen to Beethoven's², Mozart's³, Weber's⁴ &c delightful compositions being performed by Miss Schoenbein. Having for half an hour or so enjoyed that dreaming pleasure and taken a cup of coffee served up to me by my second daughter I rise again quite refreshed, light my segar and go to my laboratory or to some other business. If you should once mark two o'clock after noon, you will hardly miss the truth, if you imagine your friend lying on his couch and listening to music, an attitude not very picturesque indeed, but nevertheless proving to be an agreeable one to him, that assumes it. Now to finish with my talking of Miss Schoenbein, I will tell you, that she is very desirous of visiting England and seeing the wonders of your country, but I am afraid that her wishes will never be fulfilled, i.e. that she will never prevail upon her father to carry her there. Now before speaking of philosophy, for I cannot help talking to you of my bride, allow me to make a proposal to you, but pray, do not be angry about it. I cannot bear the Idea of seeing you no more in this world and it being very unlikely that I shall be able to cross once more the water and you in comparison to your friend being an independent i.e. moveable man, could you not make up your mind to come over to us with Mrs. Faraday next year and spend a month or two, I won't say, in Bâle itself but in our fine hilly neighbourhood, where I should try to find out for you a quiet snug corner in which you might carry on a sort of life quite congenial to your taste and Mrs. Faraday's too. You were, as I hear in Glasgow some months ago⁵; now a journey to Switzerland is not a bit more than a trip to Scotland and in two days, sleeping included you may be here with ease by the way of France. Pray, take that proposal into serious consideration and believe me that nothing in the world could prove to me and my family more gratifying than a visit of your's and Mrs. Faraday's would do.— The book⁶, of

which I once talked to you⁷, has been out these last three months and as soon as a proper opportunity will offer itself, you shall have it. Being written in german, you will declare it to be a sealed book to you, but you may easily find out a friend of your's being capable of opening it to you and, indeed, I should like you would acquaint yourself at least with some part of its tenor, as they contain a sort of profession of faith of a friend of your's. – The third volume of your Researches⁸ has as yet not reached me and putting a high value upon its possession, I beg you to be kind enough as to inquire a little after the fate of that volume.

Now let me talk a little of philosophy and what should or could I begin with but with my favorite subject oxygen, the mere name of which is hated by Mrs. Schoenbein, having become jealous as well as afraid of that seducing and mighty body. Being not quite sure whether I have written you since I got some very remarkable results, even on the risk of telling you the same story twice, I give you a short account of them. You know that I hold oxygen both in its free and bound state to be capable of existing in two allotropic modifications: in the ozonic or active and the ordinary or inactive condition. All the oxy-compounds yielding common oxygen at a raised temperature, I consider to contain ozonized oxygen and I am further inclined to believe that the disengagement of common oxygen from those compounds depends upon the transformation of their ozonized oxygen into inactive one, or as I use to denote that allotropic change of \ddot{O} into O . Now a general fact is that the oxygen thus set free always contains traces of \ddot{O} more or less, according to the degree of temperature at which the oxygen happens to be disengaged from those compounds. The lower that degree, the larger the quantity of \ddot{O} mixt with O , though I must not omit to state, that in all cases that quantity happens to be exceedingly small in comparison to that of O obtained at the same time. The best means of ascertaining the presence of \ddot{O} is the alcoholic solution of guajacum recently prepared. You know that O does not in the least change the color of that resiniferous liquid, whilst free \ddot{O} or $PbO + \ddot{O}$ &c. have the power of coloring it deep-blue. The blue matter is, as I think I have proved it, nothing but guajacum + \ddot{O} . Now if you heat the purest oxide of gold, platinum, silver, mercury, the peroxides of manganese, lead &c, in fact any substance yielding oxygen, within a small glass tube into which you had previously introduced a bit of filtering paper being impregnated with the said guajacum solution, you will see that bit of paper turning blue so soon as the disengagement of oxygen begins to take place. And all the circumstances being the same, you will farther perceive that the paper is colored most deeply and rapidly by the oxygen being eliminated from that oxycompound, which requires the lowest temperature for yielding part or the whole of its oxygen. Thus the oxygen being disengaged from the oxides of gold, platinum and silver acts more energetically upon the guajacum solution, than the oxygen does being eliminated from the oxide of mercury, the peroxide of manganese &c. I trust these results will be obtained in the Royal Institution

just as well as I get them in the laboratory of Bâle, or else my discovery shall be a very poor thing. As there cannot, I should think, be any doubt that all the oxygen being contained for instance in the oxide of silver previously to that compound being decomposed by heat, exists but in one state be that state what it may, how then does it happen, we may ask, that at the same time two different sorts of oxygen O and \ddot{O} are disengaged from the compound named? The answer to this question seems to me to be, that one of the two kinds of oxygen eliminated must be engendered at the expence of the other, or to speak more correctly, that during the act of the elimination of oxygen from the oxide of silver, part of that oxygen suffers a change of condition. Now the oxides of gold, silver &c. enjoying the power of coloring blue the guajacum solution just so as free \ddot{O} does I draw from that fact the conclusion that the condition of the oxygen being contained in the oxides of gold, silver &c., is the ozonic one and farther infer, that by far the greatest portion of that \ddot{O} , under the influence of heat, is transformed into O. Why not the whole of the oxygen being disengaged from those oxides happens to be O, I certainly cannot tell, but I think that the very fact of the mixt nature of the oxygen in question is, in a theoretical point of view, highly important and speaks in favor of my notions rather than against them. Although I have already heavily taxed your patience I am afraid, I cannot yet release you from farther listening to my philosophical talkings, for I have still to speak of a subject that has of late deeply excited my scientific curiosity and taken up all my leisure-time. But to give you an Idea of what I have been doing these last two months, I must be allowed prefacing a little. You know that I entertain a sort of innate dislike to touch any thing being in the slightest way connected with organic Chemistry, knowing too well the difficulty of the subject and the weakness of my powers to grapple with it, but in spite of this wellgrounded disinclination, I have of late and as it were by mere chance been carried in the midst of that field upon the intricacies and depths of which I have been used all my life to look with feelings of unbounded respect and even awe. The picking up of a mushroom has led to that strange aberration of mine and you will ask how such a trifling occurrence could do that. The matter stands thus: What the botanists tell me to be called "*Boletus luridus*" with some other sorts of mushroom has the remarkable property of turning rapidly blue when their hat and stem happen to be broken and exposed to the action of the atm. air. On one of my ramblings I found a specimen of the said *Boletus*, perceived the change of color alluded to and being struck with the curious phenomenon, took the bold resolution to ascertain if possible its proximate cause. I carried home the part, set to work and found more than I looked after a thing which luckily enough happens now and then. By the short space being allowed even to the longest letter being prevented from entering into the details of the subject, I confine myself to stating the principal results obtained from my mushroom researches.— *Boletus luridus* contains a colorless principle being easily soluble in alcohol and as to its relations to Oxygen bearing the closest

resemblance to Guajacum, as it appears from the fact, that all the oxidizing agents having the power of blueing the alcoholic solution of guajacum also enjoy the property of coloring blue the alcoholic solution of our mushroom principle and all the desoxidizing substances by which the blue solution of guajacum is uncolored also discharge the color of the blue solution of the Boletus matter. From this fact and others I infer that this mushroom principle, like guajacum, is capable of being associated to \ddot{O} and is not affected by O . Now the occurrence of a matter being a true brother to guajacum in a mushroom is a fact pretty enough of itself but as to scientific importance far inferior to what I am going to tell you. The fact that the resinous Boletus principle, after having been removed from the mushroom (by means of Alcohol) is not able to color itself spontaneously in the atmospheric air, whilst it seems to have that power so long as it happens to be deposited in the parenchyma of the Boletus, led me to suspect that there exists in the Boletus luridus besides the guajacumlike substance another matter being endowed with the property of exalting the chemical powers of common oxygen and causing that element in its O condition to associate itself to the resinous principle of the mushroom. And Mr. Schoenbein conjectured correctly, for I found, that in the juice being by pressure obtained from a number of mushrooms belonging to the genera of Boletus and Agaricus and notably from Agaricus sanguineus (upon which I principally worked) an organic matter is contained enjoying the remarkable power of transforming O into \ddot{O} and forming with the latter a compound from which \ddot{O} may easily be transferred to a number of oxidable matters both of an inorganic and organic nature and I must not omit to state that the peculiar Agaricus matter, after having been deprived of its \ddot{O} may be charge with it again by carrying through its solution a current of air. The easiest way of ascertaining the presence of \ddot{O} in the said Agaricus juice is to mix that liquid with an alcoholic solution of guajacum or the resinous matter of the Boletus luridus. If the juice happens to be deprived of \ddot{O} , the resiniferous solutions will not be colored blue, but if it contains \ddot{O} , the solutions will assume blue color, just so as if they were treated with peroxide of lead, permanganic acid, hyponitric acid &c. From the facts stated it appears that the organic matter in question is a true carrier of active oxygen and therefore, when charged with it, an oxidizing agent. Indeed, that matter may in many respects be compared to NO_2 , which as it is well known enjoys to an extraordinary extent the power of instantaneously transforming O into \ddot{O} and forming a compound ($NO_2 + 2\ddot{O}$) with that \ddot{O} , from which the latter may easily be thrown upon a multitude of oxidable matters. Now in a physiological point of view the existence of such an organic substance is certainly an important fact and seems to confirm an old opinion of mine, according to which the oxidizing effects of the atmospheric Oxygen (of itself inactive) produced upon organic bodies, such as blood &c., are brought about by the means of substances having the power both of exciting and carrying oxygen. Before dropping this subject I must not omit to mention a fact or

two more. The peculiar matter being contained in the juice of *Agaricus sanguineus* &c., and charged with O , gives up that oxygen to Guajacum and the latter to the resinous matter of the *Boletus luridus* so that different organic matters capable of being associated to O as such exhibit different affinities for that oxygen a fact not without physiological importance. Another fact worthy of remark is the facility with which the nature of our agaricus matter may be changed. On heating its aqueous solution, that has the power of deeply blueing the guajacum solution, to the boiling point, it not only loses that property but also the capacity of becoming an oxidizing agent i.e. carrier of oxygen again, however long it may be put in contact with atmospheric air. I am very sorry to be prevented from entering more fully into the details of the subject, but from the little I have said about it you may easily understand why that mushroom affair has of late so much engaged my attention. As to the little bit of philosophical matter, which now and then my letters to you may happen to contain, you have, of course, full liberty to do with it what you think fit.

Before I conclude I must not omit to tell you, that the Swiss Association, which as you know has become the mother of all rambling societies of Europe, will meet at Bâle next mid-summer under the presidency of Peter Merian⁹ a most intimate friend of mine. Now if you should comply with my wishes above expressed, it would be very easy to leave your retreat in our neighbourhood and join us for a day or so. I have no doubt, you would like our meeting, which being a more familiar and much smaller one than that of the british or german association, is for that very reason more comfortable and agreeable. On coming here, you will see many of our Swiss philosophers amongst others our friend de la Rive. I promise you a day in Bâle, which shall please you and remain, I trust, in your memory all your lifetime. If it fall to my lot to see such a day, I shall be the happiest man in the world.

And now I have done, my dear Faraday and ask your kind indulgence for the unusual length of my letter. Pray do not keep so long your silence, as you did the last time and believe me

Your's | most faithfully | C.F. Schoenbein

All the members of my family beg to be kindly remembered to you and in doing so I ask you the favor to present my best compliments to Mrs. Faraday. | S.

Mr. Wiedemann also charges me with his compliments to you.

1. Letter 3035.

2. Ludwig van Beethoven (1770–1827, GDMM). German composer who worked mainly in Vienna.

3. Wolfgang Amadeus Mozart (1756–1791, GDMM). Austrian composer.

4. Carl Maria von Weber (1786–1826, ADB). German composer.

5. At the meeting of the British Association.

6. [Schoenbein] (1855a).

7. Schoenbein to Faraday, 27 February 1855, letter 2943, volume 4.
8. Faraday (1855c).
9. Peter Merian (1795–1883, NDB). Swiss geologist and politician.

Letter 3055

César Mansuète Despretz to Faraday

30 November 1855

From the original in IET MS SC 2

30 Nov 1855

Monsieur et cher Confrère,

Quoique je n'aie eu l'honneur de vous voir qu'une seule fois, il y a peut être une douzaine d'années à Paris dans une Séance de l'académie¹, car dans mes deux voyages à Londres, vous étiez absent, permettez que je vous adresse une notice imprimée de mes faibles titres scientifiques augmentées des choses que j'ai faites depuis ma nomination à l'académie².

Une des raisons pour lesquelles je vous envoie le résumé de mes recherches, est celle ci. En parcourant récemment un ouvrage de physique Anglais j'ai à peine vu mon nom cité. J'en ai conclu à tort ou à raison, que les résultats Scientifiques que j'ai obtenus dans mes essais incessants, sont peu connus ou peu estimés à Londres.

Si vous lisez, mon cher Confrère avec quelque attention ma notice³ et l'addition qui est jointe vous admettez au moins que les faits du décroissement de la compressibilité des liquides⁴, de l'accroissement de la compressibilité des gaz et de l'inégale compressibilité des gaz⁵, de la loi de la propagation de la chaleur dans les liquides⁶, des *oscillations* du zéro du thermomètre⁷, la solution complète de la question du maximum de densité des dissolutions aqueuses⁸, la démonstration directe de l'interférence du son, la relation entre la longueur de l'arc voltaïque vertical ou horizontal et le sens du courant⁹, sont des résultats si je ne me fais illusion, qui me sont dus incontestablement.

Cette manière de procéder de ma part, a peut être quelque chose de singulier. Cela est possible. Mais il est bien dur de consacrer tout son temps, et le peu qu'on possède à l'avancement de la science et de voir ses travaux si peu appréciés.

Recevez, Monsieur et cher Confrère, l'assurance des sentiments distingués, avec lesquels je suis

votre dévoué Confrère | C.M. Despretz | M. de l'Institut

Le 30. 9bre 1855

PS. peut être que la démonstration de la généralité de la loi de la propagation de la chaleur dans les corps solides, bons ou mauvais conducteurs¹⁰, de la non influence de la tension ou de la quantité sur la composition de la lumière électrique¹¹, de l'indépendance de la chaleur dégagée, de la pression,

pour les corps qui ne changent pas le volume du gaz oxygène¹² a aussi une certaine importance.

Monsieur Faraday | Membre de la société royale de Londres, membre associé étranger de l'Institut de France.

TRANSLATION

Sir and dear Colleague,

Although I have not had the honour of seeing you but once, perhaps a dozen or so years ago, in Paris, at a meeting of the Academy¹, as on both my trips to England you were away, allow me to send you a printed note of my poor scientific titles, augmented by the things that I have done since my nomination to the Academy².

One of the reasons why I am sending you a summary of my research is this. Skimming through an English work on physics recently, I hardly saw my name cited. I have concluded rightly or wrongly that the scientific results which I have obtained in continuous experiments, are little known or little appreciated in London.

If you read, my dear Colleague, with a little attention my note³ and the appendix attached, you will admit that the facts on the decreased compressibility of liquids⁴, the increased compressibility of gases and on the unequal compressibility of gases⁵, the law of heat propagation in liquids⁶, the *oscillations* of zero on the thermometer⁷, the complete solution to the question of the maximum density of aqueous solutions⁸, a direct demonstration of the interference of sound, the relationship between the length of a vertical or horizontal voltaic arc and the direction of the current⁹, are results which, if I am not mistaken, are incontestably due to me.

This manner of proceeding on my part is perhaps unusual. That is possible, but it is very hard to dedicate all one's time, and the little one possesses, to the advancement of science and to see one's efforts so little appreciated.

Please accept, Sir and dear Colleague, the assurance of my distinguished sentiments, with which I am

Your devoted Colleague | C.M. Despretz | of the Institute

PS: Perhaps the demonstration of the of law, applicable to most cases, of the propagation of heat in solids, good or bad conductors¹⁰, of the non-influence of tension or quantity on the composition of electric light¹¹, of the independence of radiant heat and of pressure on bodies which do not change the volume of oxygen gas¹², also has some importance.

Mr. Faraday | Member of the Royal Society of London, Foreign Associate Member of the French Institute.

1. Presumably when Faraday was admitted a foreign associate of the Académie in 1845. See Sarah Faraday and Faraday to Reid, 28 and 29 July 1845, letter 1762, volume 3.

2. Despretz was elected a member of the Académie des Sciences in 1841.
3. Not found.
4. Despretz (1845).
5. Despretz (1827).
6. Despretz (1839).
7. Despretz (1837).
8. Despretz (1839–40).
9. Despretz (1853).
10. Despretz (1852).
11. Despretz (1853).
12. Despretz (1828).

Letter 3056

Faraday to Third Earl of Rosse¹

1 December 1855

From the original in The Birr Scientific and Heritage Foundation MS J14

1 Dec 1855

My dear Lord Rosse,

I do not know what ought to be done² but as I have received no official information as yet so I am waiting until I do. At the same time I should say that Dumas wrote to me in the way of private friendship³ & I have written back to him⁴[.]

I do not doubt that some communication will come to us in due time[.]

Ever Very Truly Yours | M. Faraday

1. William Parsons, 3rd Earl of Rosse (1800–1867, ODNB). Irish astronomer. President of the Royal Society, 1848–1854.

2. Letter 3052.

3. Letter 3045.

4. Letter 3048.

Letter 3057

Faraday to Roderick Impey Murchison

4 December 1855

From the original in BM CA

Royal Institution | 4 Dec 1855

My dear Sir Roderick,

Dr. Arnott¹ & I have been over the Museum building this morning², very carefully:– have seen the drawings, papers &c; and have had I think, no great difficulties in forming our opinions:– I now give you mine. In regard to the asphalt as used at the building, *i.e.* considering its mixed condition, its position and small quantity; I find no reason to be alarmed at it. I do not see how, either by lightning or otherwise, from external causes, it could be a source of fire to the building; nor, if the building itself were on fire in any

part, do I think it would tend to spread fire to other parts. I see no objection to its use as it is now applied.

As to the hot water pipes; we examined certain of the spaces and passages, in which those of one of the hottest arrangements are placed. I saw no signs of their ever having been overheated; and, if the precautions which are given as instructions to the man in charge are attended to, I see no reason why the heat should ever rise to a dangerous point. The spaces where the pipes pass, seem to be well guarded; as they ought to be. I understand the arrangements have been in use many years; and yet I am told that no signs of scorching, either of dust, brooms cloths, pieces of wood, paper or other such substances have appeared any where: Of course I can give only a limited opinion for I cannot tell what may be, that I have not seen. But in regard to what I have seen (and there was no variation) there was nothing which raised in my mind any doubt of the good and safe action of the arrangements.

I spoke of the lightning arrangements to Mr. Denison³ the Clerk of the works; he will tell you, what I said should be done.

I am My dear Sir Roderick | Ever truly Yours | M. Faraday
Sir Roderick Murchison Bart | &c &c &c

1. Neil Arnott (1788–1874, ODNB). Scientific writer.

2. See letter 3047.

3. W. Dennison given as clerk of works at the British Museum in *Royal Kalendar*, 1858, p. 163.

Letter 3058

Roderick Impey Murchison to Faraday

6 December 1855

From the original in IET MS SC 2

Up Park | 6 Dec 1855

Dear Faraday,

I beg to acknowledge the receipt of your letter of the 4th Dec¹ which is so *very satisfactory* that I have enclosed it to Sir H. Ellis² to be be [sic] laid before the Trustees on Saturday next³, when I am sure they will unite with me in returning to yourself & Dr. Arnott⁴ our grateful thanks.

Dr. Percy has written to me about some diagrams for your lectures⁵ & I have replied in the affirmative, provided I have the power, & if not I will obtain the permission of a board & for the purpose, as I should consider it a disgrace to myself if our Institute⁶ did not do every thing in our power to enable you to enlighten the rising generation[.]

Yours most sincerely | Roderick Murchison

I hope to be in Town tomorrow night

1. Letter 3057.

2. Henry Ellis (1777–1869, ODNB). Librarian of the British Museum, 1827–1856.

3. That is 8 December 1855.
4. Neil Arnott (1788–1874, ODNB). Scientific writer.
5. That is for Faraday's Christmas lectures on the 'Distinctive Properties of Common Metals'. Faraday's notes are in RI MS F4 J17.
6. That is the Royal School of Mines.

Letter 3059

Friedrich Wöhler to Faraday

10 December 1855

From the original in IET MS SC 2

Göttingen | le 10 Dec 1855

Monsieur,

Permettez moi d'abuser pour quelques moments de vôtre temps si précieux pour la science. Mr. Stein¹, dont j'ai fait la connaissance il y a quelques années et qui possède des mines et des usines d'argent en Mexique, est retourné de là à Darmstadt et a rapporté une masse de fer météorique pesante 109 Kilogrammes (218 livres) et trouvée dans la vallée de Toluca². Il m'en a fait couper un échantillon et me l'a envoyé. C'est du véritable fer météorique, caractérisé par les belles figures, dites de Widmannstädten³, qui se sont montrées en traitant une surface polie avec de l'acide nitrique étendue, et par une grande quantité de Nickel, qu'il contient. En outre on remarque sur sa surface un peu oxydée du sulfure de fer et du phosphore double de fer et de Nickel (le Schreibersit).

Mr. Stein désire vendre cette masse en entier et il croit pouvoir demander 350 livres Sterling. Il l'a déjà offert aux cabinets de Mineralogie à Vienne et à Berlin, mais on a répondu, que dans ce moment on n'ait pas disponible une telle somme d'argent pour un seul objet. Il a donc cru que peut-être le British Museum voudra acheter ce fer, et je lui ai promis de m'adresser à cet égard à Vous, Monsieur, n'ayant pas l'honneur de connaître le directeur de la section de minéralogie du British Museum.

J'ose donc vous prier, de vouloir bien me dire en deux mots, si peut-être à Londres on veut acheter cette masse remarquable. Si vôtre réponse est Non, Mr. Stein se décidera probablement de faire couper la masse et de la vendre divisée en des petits morceaux de 1 ou de 2 Kilogr. de pesanteur. Dans ce cas vous voulez bien me dire, si le british Museum ou quelque autre personne désirera acheter des tels échantillons. Je pense qu'un morceau de 1 Kilogr. coutera à peu près 4 livres Sterling.

Agréez, Monsieur, l'expression de la plus profonde considération, avec laquelle j'ai l'honneur de me nommer

Vôtre | très dévoué | Wöhler

TRANSLATION

Göttingen | 10 Dec 1855

Sir,

Permit me for a few moments to take up your time, so precious for science. Mr. Stein¹, whom I met a few years ago and who owns some mines and silver factories in Mexico, has returned from there to Darmstadt and has brought back an iron meteorite, weighing 109 kilogrammes (218 pounds) which was found in the Toluca valley². He cut a sample and sent it to me. It is a real meteorite, characterised by the beautiful figures, called after Widmanstätten³, which can be seen by treating a polished surface with dilute nitric acid, and by the large quantity of nickel. Moreover, one can see on its slightly oxidised surface, iron sulphide and double iron phosphide and nickel (der Schreibersit).

Mr. Stein would like to sell the block intact and thinks he can ask £350 sterling for it. He has already offered it to the mineralogical collections in Vienna and Berlin, but has received the reply that, at present, they do not have those kinds of funds available for a single object. He has thus thought that the British Museum might like to buy this meteorite and I promised that I would write to you in this regard, Sir, not having the honour of knowing the director of the mineralogy department at the British Museum.

I therefore dare to ask you kindly to tell me in two words, if perhaps London would like to buy this remarkable meteorite. If your reply is negative, then Mr. Stein will probably decide to have the block cut and to sell it divided into small pieces of 1 or 2 kilogrammes in weight. In that case, could you kindly tell me if the British Museum or anyone else would like to buy such samples. I think that a piece of 1 kilogramme will cost about £4 sterling.

Your | most devoted | Wöhler

1. G.A. Stein according to Wöhler (1856), 127.

2. A description of this meteorite is given in Grady (2000), 492–3.

3. The characteristic patterns in a cut meteorite face which has been etched, discovered by Aloys Joseph Beck Edler von Widmannstätten (1754–1849, DSB). Director of the Imperial technical collection in Vienna.

Letter 3060**Faraday to George Robert Waterhouse¹****12 December 1855****From the original in Natural History Museum L. Handwriting collection**

Royal Institution | 12 Dec 1855

My dear Sir,

You will see the nature of the inclosed note². I am told that you are the person in whose hands I ought to place it. I do not know any thing of the

specimen or the fitness of the offer: but Wohler is a man of high science – I should be glad to have the letter back again by & bye that I may write him a reply –

I am My dear Sir | Very Truly Yours | M. Faraday
– Waterhouse Esqr | &c &c &c

1. George Robert Waterhouse (1810–1888, ODNB). Keeper of the mineralogical and geological collections at the British Museum, 1851–1857.

2. Letter 3059.

Letter 3061

Faraday to William Snow Harris

21 December 1855

From a copy in University of British Columbia MS VF-46

My dear Harris,

I want the pleasure of writing a few words to you – I have nothing to say about Science and I do not know what it is just now that moves me except it be the glance at your last Book¹ just now as it came before me on the Desk & then the long train of pleasant recollections which it brought back with it from the olden times. Oh! We are getting older – I am at least and the excitement of former times & things becomes much chastened. Not so the recollections – they become more & more and they bring back with them such a flood of thoughts of kindness which I have received from all that I am ready at times to weep with gentle joy. Amongst all this comes our old acquaintance and it is that which sets my mind running in an almost unintelligible manner.

Every good wish towards you my dear old friend from one who has had much of your kindness and hopes to be grateful towards Him who has directed it.

Ever truly Yours | M. Faraday

To Sir W. Snow Harris

Royal Institution | 21 Decr. 1855

1. Harris (1856a).

Letter 3062

William Snow Harris to Faraday

22 December 1855

From the incomplete original in IET MS SC 2

6 Windsor Villas, Plymouth | 22 Dec 1855

My dear Faraday,

I was made very happy in the receipt of your most kind letter¹ by this mornings post: and you may rest assured that no one has a stronger

hold of my affection respect, and esteem, than yourself; few very few so strong. I therefore fully appreciate the kindness & honor you have shown me by this communication and reciprocate with all my Heart your good wishes toward me –. It is as you say no recent recollections which thus associate us in the bond of friendship no events of yesterday – but our mutual esteem has been the growth and experience of many years[.] On my part it is the humble tribute of a pupil to the virtues and Talents of *a* if not *the* great Master of the Science of Modern Days – I think I might have done something more considerable in Science had my life been less embarrassed by the presence of worldly circumstances – “He that hath Wife and Children hath given hostages to Fortune”² so says our great Philosopher³ the author of the Inductive Philosophy –. I have had much I do assure you within the last 10 years more especially, to distress and embarrass me – In a large Provincial Town like this as you may imagine, with station to maintain public duties to be fulfilled and a domestic establishment with all its attendant *meteors* to reconcile one has really not that repose of mind & Time at command requisite to the carrying out intricate important and expensive researches in Science – Thank God I am I hope somewhat more clear of this now and have lately commenced a renewal & revisit of all my Scientific Instruments – I have *turned every thing out* restored improved & arranged my apparatus – I have some things I should much like you to see – The long battle I had with the Government relative to my permanently-fixed Lightning Conduction for the Navy – was a source of great trouble and vexation to me especially the illiberal delay I experienced at the hands of persons who should have known better – It is after all some consolation to me to know that a good thing has certainly been done for the Navy and some light thrown on an interesting Department of Physics by my researches – It was an Herculean Task to hunt out all the dusty ships records deposited at the Admiralty – and obtain substantial evidence of the terrible loss of Life and property &c by strikes of Lightning at sea. Ten to Twenty men at a time more or less paralyzed whilst unfurling a topsail – The Admiralty would not believe all this at first and I was looked on as a sort of visionary in proposing to defend Ships from Lightning by a systematic application of a metallic Conductor of great capacity carried throughout the masts & Hull. It is all very well to say now that the thing is done how easy this problem & that all I accomplished was to nail a little copper to the spars.– but at the time when

M. Faraday Esq FRS. LLD

1. Letter 3061.

2. Francis Bacon, essay 7, ‘Of marriage and single life’.

3. Francis Bacon (1561–1626, ODNB). Lawyer and philosopher of science.

Letter 3063**Faraday to Margery Ann Reid****24 December 1855****From the original in the possession of Elizabeth M. Milton**

R Institution | 24 Dec 1855

My dear Maggie,

Finding that my wife had made a vague promise of dining with you tomorrow on her way to Cross St.¹ I write to say that I have dissuaded her from it. She has not slept well – is still in bed ($\frac{1}{2}$ p 9 o'clk) & it would be unwise of her to try to [do] much tomorrow. We shall go together to Cross street and expect to meet you there for a little while. Our love to your father² – I saw him yesterday³ and that he looked (as he always does) well but I hope to hear from himself how he is tomorrow[.]

Your Affectionate Uncle | M. Faraday

1. Where Sarah Faraday's brother, John Barnard, lived.

2. William Ker Reid.

3. The 23rd being a Sunday, this would have been at the Sandemanian meeting house.

Letter 3064**Henry Ellis¹ to Faraday****27 December 1855****From the original in RI MS Conybeare Album, f. 40**

British Museum | 27 Dec 1855

My dear Sir,

A young friend (Mr. Hutchinson²) is very desirous of hearing one of your Lectures, and if you could favor him for that of Saturday next³ you would very much oblige me[.]

Ever Sincerely Yours | Henry Ellis

Michael Faraday Esq | &c &c &c

1. Henry Ellis (1777–1869, ODNB). Librarian of the British Museum, 1827–1856.

2. Unidentified.

3. That is 29 December 1855 when Faraday delivered his second Christmas lecture in the series on the 'Distinctive Properties of Common Metals'. Faraday's notes are in RI MS F4 J17.

Letter 3065**John Percy to Faraday****28 December 1855¹****From the original in RI MS Conybeare Album, f. 7**

[Royal Institution embossed letterhead] | Friday evening

My dear Dr. Faraday,

I trust you had what you wanted yesterday. The nugget was not the same as that you saw at the museum; Mr. Reeks², and myself thought it would answer your purpose better than the other. It was modelled from a Californian specimen. If you will let Reeks know tomorrow morning which you require, one of my men shall take the things to the Institution[.]

Ever yours | John Percy

Dr. Faraday

1. Dated on the basis that Faraday lectured on gold on 27 December 1855 in his Christmas lectures on the 'Distinctive Properties of Common Metals'. See RI MS F4 J17, p. 1.

2. Trenham Reeks (c.1823–1879, B3). Curator and librarian of the Museum of Economic Geology and registrar of the School of Mines, 1851–1879.

Letter 3066**Faraday to John Percy****31 December 1855****From the original in YUL Brush Papers, Box 7**

Royal Institution | 31 Dec 1855

My dear Percy,

Herewith I return your aluminium with very many thanks¹.To day I only want Table B.2².

For *Thursday*³. I have as many as Six tables down namely 13. 15⁴. 17. 18. 19. 26. all B's and also I long for several illustrations specimens namely

Iron paper

Tough Iron bent

Iron cast of Crow fish

Iron punched. 7 inches diameter

Steel illustrations – fracture &c

May I send Newmans men with a cart for these? – and when?

Ever Truly Yours | M. Faraday

1. Which Faraday used on 29 December 1855 when he delivered his second Christmas lecture in the series on the 'Distinctive Properties of Common Metals'. Faraday's notes are in RI MS F4 J17.

2. Faraday's third Christmas lecture.

3. Faraday's fourth Christmas lecture delivered on 3 January 1856.

4. Endorsed 'No 16' here.

Letter 3067**Faraday to George Gabriel Stokes****7 January 1856****From the original in RS MS RR 3.158**

Royal Institution | 7 Jan 1856

My dear Sir,

I do not see that I can change a single word of my former letter (6 July 1855)¹. Mr. Joules² paper³ is intended for the use of the mathematicians and is one that I cannot pretend to judge on its merits. The merits of the philosopher I know very well but you know them as well as I do

Ever Truly Yours | M. Faraday

G.G. Stokes Esq | &c &c &c

I return herewith the paper & my former note. MF.

Endorsement: 1856 Joule by Faraday additional letter

1. Faraday to Weld, 6 July 1855, letter 3002, volume 4.

2. James Prescott Joule (1818–1889, ODNB). Manchester physicist.

3. Joule (1856).

Letter 3068**Faraday to Roderick Impey Murchison****7 January 1856****From the original in GS MS M/F4/5**

Royal Institution | 7 Jan 1856

My dear Sir Roderick,

I am now so near to the end of my lectures¹ & have had such opportunity of profiting by your kindness that I think I may not improperly thank you most heartily for your willing aid² though I have not as yet released the last favour. I am sure it will give you pleasure to know that the Royal Princes have been daily indebted to your kindness as well as myself_[.] It is no small proof of the liberality of thought & mind about our throne that their Royal Highnesses should have been allowed to come to an Institution which forms no part of the recognised schools of learning. I only hope that what has passed here will give no cause to Her Majesty & Prince Albert to regret the honor & condescension shewn to us³_[.]

I am My dear Sir Roderick | Ever Truly Yours | M. Faraday

1. Faraday delivered the sixth and final Christmas lecture in the series on the 'Distinctive Properties of Common Metals' on 8 January 1856. Faraday's notes are in RI MS F4 J17.

2. See letter 3058.

3. The first lecture was attended by Prince Albert and his sons the Prince of Wales and Prince Alfred and was depicted by the Sandemanian and artist Alexander Blaikley (1816–1903, AIKL) in his painting of the event (Plate 3). The young princes attended the remainder of the course without their father. See James (1999b), 6.

Letter 3069**Faraday to Thomas Sterry Hunt¹****c.14 January 1856²****From the original in BUL Newell Collection**

Many thanks to you my dear Sir for your kind information on the matter of Dumas. Our meeting has been short I can only say that if it had been longer I should most probably have made the favour greater and increased my obligations to you[.]

Yours Very faithfully | M. Faraday

T.S. Hunt Esqr | &c &c &c

1. Thomas Sterry Hunt (1826–1892, ANB). American-born chemist in who worked for the Geological Survey of Canada.

2. Dated on the basis of the reference to Hunt in letter 3070.

Letter 3070**Faraday to Jean-Baptiste-André Dumas****14 January 1856****From the original in AS MS**

Royal Institution | 14 Jan 1856

My dear friend,

Wheatstone in telling me that he had seen you gave me impressions very different to those that I had received from your letters¹:- he seemed to think that it was a duty for me to appear at Paris and went so far as to imply that you had said the Emperor expected it. I should be very grieved to think I could have appeared unthankful or less grateful than I ought to feel either in the Emperors eyes or in yours whose kind affection & approbation I most highly esteem; and having finished a course of lectures honored by the daily presence of our Royal Princes² I new feel able to leave town for a few days[.] So I propose running over to Paris & back in the middle of next week but as I have no other object than to see you and do as I ought to do, so if you were not there I should have no desire to be there[.] Tell me therefore whether I shall see you (and Madame Dumas) if I come and tell me whether on the whole I ought to come. Mr. Hunt³ of Canada has just given me your correct address⁴ & so I hope my letter will find you at once[.] Our joint & most sincere respects & remembrances to Madame Dumas.

Ever My dear friend | Your devoted & indebted | M. Faraday
a Monsieur | Monsieur Dumas | Secretaire | &c &c &c

1. Letters 3038 and 3042.

2. See note 3, letter 3068.

3. Thomas Sterry Hunt (1826–1892, ANB). American-born chemist in who worked for the Geological Survey of Canada.

4. See letter 3069.

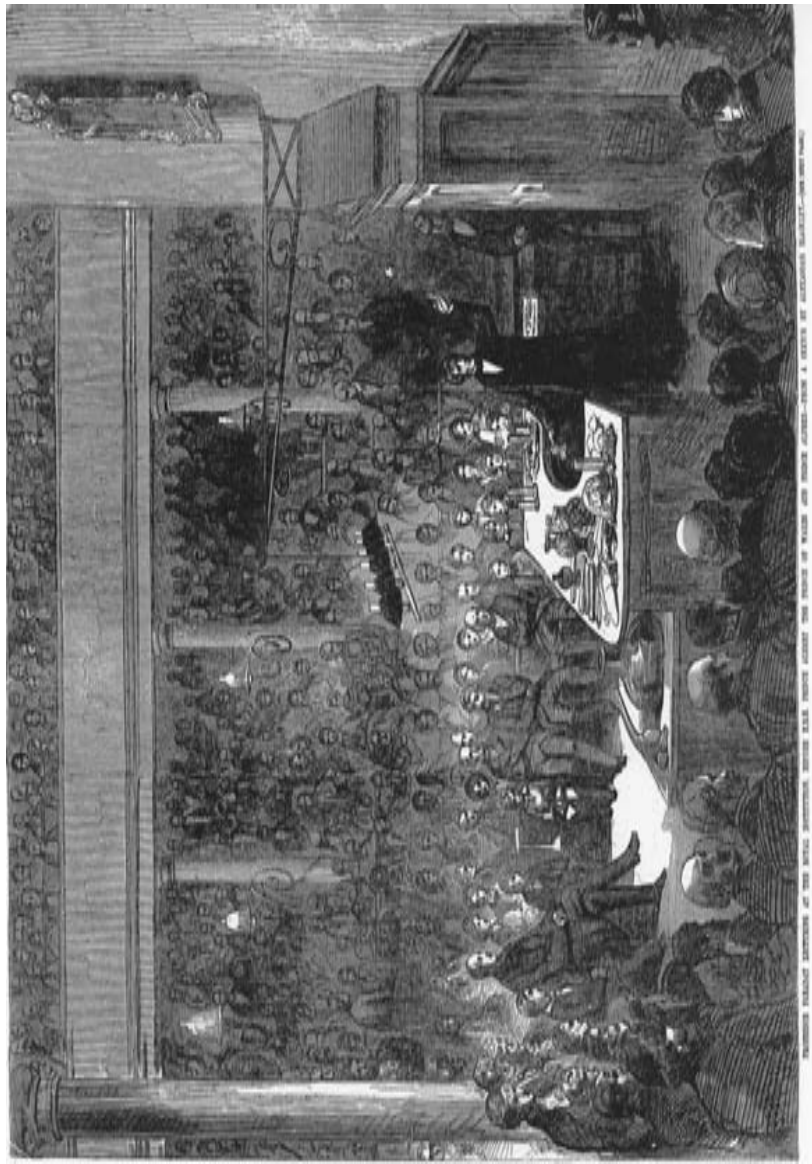


Plate 3. Michael Faraday delivering a Christmas lecture before Prince Albert, the Prince of Wales and Prince Alfred, 27 December 1855. Engraving after Alexander Blaikley, *Illustrated London News*, 1856, 28: 177.

Letter 3071
Prince of Wales to Faraday
16 January 1856
From the original in RI MS F1 H2

Windsor Castle | 16 Jan 1856

Dear Sir,

I am anxious to thank you for the advantage I have derived from attending your most interesting Lectures¹. Their subject, I know very well, is of great importance, and I hope to follow the advice you gave us of pursuing it beyond the Lecture Room; and I can assure you that I shall always cherish with great pleasure the recollection of having been assisted in my early studies in Chemistry by so distinguished a man.

Believe me, Dear Sir | Yours truly | Albert Edward

1. See note 3, letter 3068.

Letter 3072
Prince Alfred to Faraday
16 January 1856
From the original in RI MS F1 H3

Windsor Castle | 16 Jan 1856

Dear Sir,

I write to thank you very much for the pleasure you have given me by your Lectures¹, and I cannot help hoping they will not be the last I shall hear from you. Their subject was very interesting and your clear explanations made it doubly so.

Believe me | Dear Sir | Yours truly | Alfred

Address: M. Faraday Esq.

1. See note 3, letter 3068.

Letter 3073
Jean-Baptiste-André Dumas to Faraday
16 January 1856
From the original in IET MS SC 2

Paris | 16 Janvier 1856

Mon cher ami,

Certainement vous serez le bien venu à Paris et vous y causerez une grande joie¹. Nous essaierons de vous distraire en vous montrant toutes nos curiosités. Nous essaierons de vous montrer nôtre amitié en vous prouvant que la distinction exceptionnelle dont vous avez été l'objet de la part de l'Empereur n'a fait que ratifier le sentiment public.

Venez donc à paris passer quelques jours avec nous, si vous en avez la liberté en ce moment.

Mais, si comme je le suppose, vous avez fait savoir à l'Empereur que vous aviez été sensible à son bon souvenir, ne craignez pas d'avoir manqué à aucun égard envers lui.

Aussi, si vous trouvez que la saison vous rende le voyage pénible et que vous préféreriez l'ajourner au mois de mai, ne vous croyez pas obligé à le faire en ce moment. Si votre Santé le permet, que la saison ne Vous contrarie pas et que vous soyez au goût de distinction venez en chercher ici et nous serons très heureux et très fiers de vous posséder.

Vous le voyez, Je suis partagé entre le desir de vous voir et de vous entendre et la crainte de penser que le voyage ne vous soit importune. Je suis bien de l'avis de notre ami M. Wheatstone que vous devez une visite à paris cette année, mais J'en diffère un peu en ce que Je pense que vous etes libre d'en choisir l'époque. Celle-ci est bonne, mais, si vous veniez au mois d'avril ou en mois de mai, vous auriez un temps plus doux.

Quelle que soit votre décision, écrivez-moi un môt pour me prévenir de votre arrivée. Je serais heureux de me mettre à votre disposition et je ferais tout ce qui dépendrait de moi pour vous faciliter l'accomplissement de vos désirs à Paris.

Tout à vous de Coeur | J. Dumas

TRANSLATION

Paris | 16 Jan 1856

My dear friend,

You will certainly be welcome in Paris and you will cause great joy¹. We will try to amuse you by showing you all our curiosities. We will try to show you our friendship proving to you that the exceptional honour with which you have been distinguished by the Emperor has merely ratified public sentiment.

Come, therefore, to Paris to spend some days with us, if you are at liberty at the moment.

But, as I expect, you have made known to the Emperor that you have been sensible to his kind memory, do not fear any lessening of his regard.

Also, if you find that the season makes the journey difficult and you would prefer to postpone until the month of May, do not feel obliged to travel at this time. If your health permits, the season does not bother you and you would like some distraction, come and look for it here and we shall be most happy and proud to have you.

You see, I am divided between the wish to see and hear you, and the fear of thinking that the journey might be inconvenient. I completely agree with our friend, Mr. Wheatstone, that you should visit Paris this year, but I differ a little in that I think you should be free to choose when to come. This

is a good time but if you came in the month of April or in the month of May, you would have pleasanter weather.

Whatever your decision, write me a word to forewarn me of your arrival. I would be pleased to put myself at your disposal and would do everything in my power to ensure your wishes are fulfilled in Paris.

All yours from my heart | J. Dumas

1. See letter 3070.

Letter 3074

Faraday to Prince of Wales

18 January 1856

From the original in RAW T1/102

Royal Institution | 18 Jan 1856

To | His Royal Highness | The Prince of Wales
Sir,

The favour Your Royal Highness has conferred upon me by a letter in your own hand¹, and the kind expressions it conveys, sink deep into my heart. I am most happy to think that the lectures have been a source of pleasure, and should be indeed glad if their most worthy subjects continued to interest you in the future². The remembrance of them was already very dear to me, but Your Royal Highness has added much to it.

That all good should be Yours is the earnest hope and wish of Your Royal Highness's

Most grateful & most humble Servant | M. Faraday

1. Letter 3071.

2. See note 3, letter 3068.

Letter 3075

Faraday to William Thomson

18 January 1856

From the original in ULC Add MS 7342, F35

Royal Institution | 18 Jan 1856

My dear Sir,

It so rejoices me to see Your name upon our list of Friday Evening¹ that I cannot help but write to congratulate not you but myself on the delight I shall have. My head gets weary & dull or else I should often trouble you with a letter for it seems to me I could often ask & as yet you have always answered. I do not mean in mere form but to my judgement and understanding. I wish I could continually sit under your wing –

I understand Tyndall has undertaken to get all prepared for you that we can do here or else you know how glad I should be to be useful_[.]

Ever Truly Yours | M. Faraday
Professor W. Thomson | &c &c &c

1. Thomson (1856a), Friday Evening Discourse, 29 February 1856.

Letter 3076

Faraday to Comte de Persigny¹

19 January 1856

From the original copy in IET MS SC 3

Royal Institution | 19 Jan 1856

To His Excellency | the Count Fialin de Persigny | &c &c &c
M le Count,

I am led to believe that I ought to thank the Emperor personally for the high honor he has done me in creating me a Commander of the Legion of Honour², especially when I call to remembrance circumstances of personal communication in former times³_[.] May I beg the favour of conveyance of the enclosed⁴ to its high destination.

I have the honor to remain | Your Excellencys most | humble Obedient
Servant | M. Faraday

1. Jean Gilbert Victor Fialin, Comte de Persigny (1808–1872, NBU). French ambassador to London.

2. Letter 3043.

3. For example, see Napoleon to Faraday, 1840 and 23 May 1843, letters 1228, volume 2, and 1496, volume 3, respectively.

4. Letter 3077.

Letter 3077

Faraday to Napoleon III

19 January 1856

From the original copy in IET MS SC 3

Royal Institution | 19 Jan 1856

To His Imperial Majesty | The Emperor
Sire,

I fear to intrude yet I also fear to seem ungrateful; and before Your Majesty I would rather risk the former than the latter. I know not how to return fit thanks for the high and most unexpected honor which your Imperial Majesty has conferred upon me in the gift of the Degree of Commandant of the Legion of Honor¹. I cannot promise to deserve it by the future for the effects of time tell me there are no hopes that I should hereafter work for science as in

past years[.] I can only offer a most grateful & unfailing remembrance of that which to me is more than honor – of the kindness of Your Imperial Majesty to one such as I am:– and I feel deeply affected by the thought that even I, by Your Majestys favour form one link, though a very small one, in the bonds which I hope will ever unite France & England[.]

Hoping & believing that Your Majesty will accept my earnest thank[s] & deep seated wishes for Your Majesty in all things I venture to sign Myself as

Your Imperial Majesty's | most humble & most Grateful Servant |
M. Faraday

1. Letter 3043.

Letter 3078

Faraday to Jean-Baptiste-André Dumas

19 January 1856

From the original in AS MS

Royal Institution, London | 19 Jan 1856

My dear kind friend,

Yours of Yesterday has decided me¹. I will come but I will come when the year is more advanced. But your letter has made me very bold and I have done a thing which I should not have had courage to do without it. I have written to the Emperor² and sent it through your Ambassador here³. I had had a notice from the Chancellerie of the Legion of Honor⁴ and wrote a reply to it with sincerest thanks but durst not do more. Now I have been very bold I hope not too bold and I am sure that *you* will do me the justice to believe I would not intrude. Thinking to see you before very long. I am ever more & more

Yours affectionately | M. Faraday

1. Letter 3073.

2. Letter 3077.

3. Jean Gilbert Victor Fialin, Comte de Persigny (1808–1872, NBU). French ambassador to London. See letter 3076.

4. Letter 3043.

Letter 3079

William Thomson to Faraday

29 January 1856

From the original in IET MS SC 2

2 College, Glasgow | 29 Jan 1856

My dear Sir,

Although I hope soon to see you in London, I cannot delay till then thanking you for your letter of the 18th¹ and for the very kind expressions

it contains. Such expressions, from you, would be more than a sufficient reward for anything I could ever contemplate doing in science. I feel strongly how little I have done to deserve them but they will encourage me with a stronger motive than I have ever had before, to go on endeavouring to see in the direction you have pointed, which I long ago learned to believe is the direction in which we must look for a deeper insight into nature.

I cannot express to you how much I fall short of deserving what you say, but must simply thank you, most sincerely, for your kindness in writing as you have done.

Believe me, Ever yours truly | William Thomson
Professor Faraday

1. Letter 3075.

Letter 3080

Jacob Herbert to Faraday

30 January 1856

From the original in GL MS 30108/2/67

Trinity House, London | 30 Jan 1856

Sir,

The Elder Brethren have deemed it necessary to make enquiry of the Irish Light House Board, as to the practical effect of certain vertical Lenses which have been introduced by Mr. Wilkins into the Lighting Apparatus in the Ballycotton Light House, and have also addressed the Light House Board in Paris on the subject of those Lenses, and the Board being desirous of having the benefit of your observations thereon, I am directed to transmit to you in original, the replies which have been received from those Bodies, and to request the communication of any remarks which may occur to you on the subject:—

I shall be obliged by your returning the enclosures, and remain,

Sir | Your most humble Servant | J. Herbert

M. Faraday Esq.— | &c. &c. &c.

Letter 3081

Faraday to Jacob Herbert

1 February 1856

From the original copy in GL MS 30108/2/67

Royal Institution | 1 Feb 1856

Sir,

In returning the letters from Ireland & France and according to your request¹ accompanying them by such observations as they may suggest I can

only say that nothing of a negative character occurs to my mind:– the principles & object of both are good; of the practice of either you will not expect me to give an opinion.

In carrying out the principle I agree entirely with the French letter, in recommending that the light should be refracted & sent if possible at once in its right direction & not to have to pass through a second set of prismatic lenses; since in the latter case much of it must be lost. Even if the expence of both methods had been the same there would still remain this preservation of light for the French plan as well as greater final simplicity in the state of the apparatus[.]

Though I do not pretend to judge of the proper duration of a flash, still in observing that given by a revolving dioptric light apparatus I have often thought that it came on & passed away very suddenly & therefore the plan referred to in the French letter of making the Crown flashes precede [sic] by 4° those of the under lenses commends itself to my mind².

I am Sir | Your Very faithful Servant | M. Faraday
Jacob Herbert Esq | &c &c &c

1. Letter 3080.

2. This letter was read to Trinity House Court, 5 February 1856, GL MS 30004/26, pp. 250–1. It was referred to the Deputy Master, Wardens and Light Committees.

Letter 3082

Anthony Panizzi to Faraday

1 February 1856

From the original in IET MS SC 2

British Museum | 1 Feb 1856

My Dear Sir,

I am directed by the Trustees of the British Museum to request that you will be so good as to assist their architect, Mr. Smirke, in choosing the best substratum on which to lay the gold which is to be applied to ribs, mouldings and other parts of the cupola over the new Reading Room.

The Trustees would also feel obliged if you would mention to Mr. Smirke the quality of gold-leaf which in your opinion ought to be preferred and the thickness of metal which would be desirable so that the gilding might be finished in a superior and lasting manner¹.

Believe me, My Dear Sir | Yours faithfully | A. Panizzi
Professor Faraday | &c &c &c

1. This seems to have been the initial stimulus for Faraday's extensive experimentation on the transmission of light through gold which he began the following day. Faraday, *Diary*, 2 February 1856, 7: 14243–90. See James (1985), 152–4, Tweney (2002, 2006) and Tweney *et al.* (2002, 2005).

Letter 3083**Robert Stephenson¹ to Faraday****5 February 1856****From the original in IET MS SC 2**

34 Gloucester Sq | 5 Feb 1856

My dear Sir,

Bidder's² lecture is postponed for a week³. I will take care if any change take place that you are informed_[.]

Yours Sincerely | Rob Stephenson

1. Robert Stephenson (1803–1859, ODNB). Railway engineer and President of the Institution of Civil Engineers, 1856–1857.

2. George Parker Bidder (1806–1878, ODNB). Civil engineer.

3. Bidder (1856) read to the Institution of Civil Engineers on 19 February 1856.

Letter 3084**Henry Creswicke Rawlinson¹ to Faraday****9 February 1856²****From the original in IET MS SC 2**

Saturday | 8 [sic] Feb

My dear Sir,

I shall be much obliged if you will lend me the printed lists of Babylonian & Assyrian Kings prepared for the Royal Institution last year³ – as I have to give a lecture on the subject on Monday next⁴ –

They shall be returned to the Institution on Tuesday.

Yours faithfully | H. Rawlinson

1. Henry Creswicke Rawlinson (1810–1895, ODNB). Assyriologist and diplomat.

2. Dated on the basis of the reference to Rawlinson's Friday Evening Discourse and that this loan was noted in RI MS F5B, p. 2.

3. Rawlinson (1855), Friday Evening Discourse of 15 June 1855. This list is referred to here.

4. That is 11 February 1856.

Letter 3085**Warren De La Rue to Faraday****9 February 1856****From the original in IET MS SC 2**

London, 110 Bunhill Row | 9 Feb 1856

My dear Mr. Faraday,

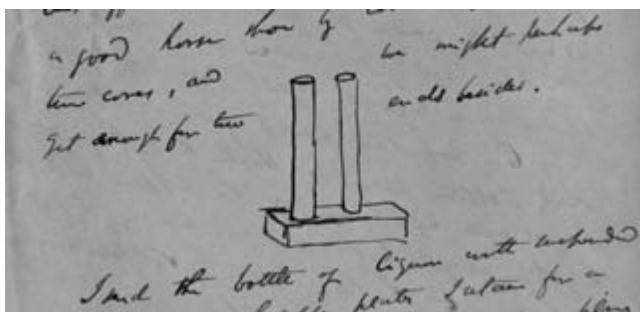
Subsequent experiments have shewn me that we were a little too impatient the other day¹:- the solution of gold I now have, and of which you have a portion gives films, but they are much thicker than those first obtained by me – and I fail even now in getting thin ones. A little free *hydrochloric* acid

very much modifies the colour of a neutral solution of the chloride, and is apt to cause the gold to become crystalline, but I have not yet succeeded in so tempering the solution so as to get the first degree of tenuity.

One important result of our failure is this that I obtain films so thick that they may be as well lifted up on the copper with the long hole as gold leaf – and on Monday² I will endeavour to thicken some by Voltaic means and let you know the result.

Glass, Clock-glasses; Chas. Wm. Price. 20 Clerkenwell Green³
 Gold Leaf (our man) Law No 1–2 Northside Bethnal Green⁴
 Glass Plates Edward & Wm. H. Jackson, 315 Oxford Street⁵

I find that bad as my memory is that I was right and that we have another broken spindle similar to one we have turned down to make a cone for you – if you will kindly let me know its diameter and length I will make this one the same; it is now being softened (annealed). We have moreover a broken square spindle 27 inches long which will plane up to 4? square and this might be cut off to the length you desire bent to make a good horse shoe by combining with the two cones, and we might perhaps get enough for two ends besides.



I had the bottle of liquor with suspended gold in it – the copper plates I retain for a day or two in the hope of giving you some films on them.

I am Very truly Yours | Warren De La Rue

1. Faraday was at De La Rue's on 6 February 1856. Faraday, *Diary*, 6 February 1856, 7: 14319.
2. That is 11 February 1856.
3. Charles William Price. Coloured glass merchant of 20 Clerkenwell Green. POD.
4. John Law. Gold beater of 3–4 Northside, Bethnal Green. POD.
5. Edward and William H. Jackson. Glass merchants of 315 Oxford Street. POD.

Letter 3086**Robert Stephenson¹ to Faraday****12 February 1856²****From the original in RI MS Conybeare Album, f. 39**

34 Gloucester Sq | Tuesday Morning

My dear Sir,

Mr. Bidder's³ lecture is again put off until this day week when I hope it will be quite convenient for you to attend⁴[.]

Yours Sincerely | Rob Stephenson

Professor Faraday

1. Robert Stephenson (1803–1859, ODNB). Railway engineer and President of the Institution of Civil Engineers, 1856–1857.

2. Dated on the basis that this continues on from letter 3083.

3. George Parker Bidder (1806–1878, ODNB). Civil engineer.

4. Bidder (1856) read to the Institution of Civil Engineers on 19 February 1856.

Letter 3087**Jacob Herbert to Faraday****13 February 1856****From the original in GL MS 30108/2/79**

Trinity House London | 13 Feb 1856

Sir,

I am directed to forward to you the accompanying Samples of White Lead, from the parties whose names are marked thereon, who have sent Tenders for the Supply required for this Corporation's Service, and to request that you will be pleased to analyze the same, and report the result for the Board's information¹.–

I am | Sir | Your most humble Servant | J. Herbert

M. Faraday Esqr. D.C.L. | &c &c &c

1. Faraday's report has not been found but it was noted as being dated 20 February 1857 in Trinity House Wardens Committee, 26 February 1856, GL MS 30025/24, pp. 147–8.

Letter 3088**Faraday to Ernst Becker****16 February 1856****From the original in RI MS F1 D28**

[Royal Institution embossed letterhead] | 16 Feb 1856

My dear Sir,

I return the Niepce¹ memoir and papers² with many thanks[.] The account of his life is very interesting:– he must have much happiness in the

pursuit of his subject. The subject [word illegible] you is a result of his method described in the first paper in the book[.]

I am | My dear Sir | Very Truly Yours | M. Faraday
E. Becker Esqr | &c &c &c

1. Joseph Niépce (1765–1833, DSB). French inventor of photography.
2. Niépce (1841).

Letter 3089

Faraday to G.S. Price¹

18 February 1856

From the original in BL add MS 70951, f. 320

Royal Institution | 18 Feb 1856

Sir,

I have no lectures going on at the Royal Institution or elsewhere[.] Next Friday I shall give a discourse² but only those are admitted who are Members or to whom a Member gives an admission he having the right to admit two on each Friday evening[.] The inclosed paper will probably give you any further information you may desire[.]

I am | Your Obedient Servant | M. Faraday
G.S. Price Esq | &c &c &c

1. Unidentified.
2. Faraday (1856a), Friday Evening Discourse, 22 February 1856.

Letter 3090

James Emerson Tennent to Faraday

19 February 1856

From the original press copy in TNA MT4/20, p. 839

19 Feb 1856

acquaint you that my Lords having undertaken to assist in the Establishment and Construction of Lighthouses in the Colonies they would be glad if you would undertake for this Board the same Inspection of the Lighting Apparatus for these Lighthouses as you now perform for the Elder Brethren of the Trinity House in respect of English Lights.

My Lords Request that you will have the goodness to inform them whether you can undertake this duty and if so whether the terms would be in any way different from those adopted in your arrangement with the Elder Brethren.

My Lords would in all cases give you notice when any Apparatus was ready for inspection and in almost all cases the Apparatus would be constructed in London.

I am | Sir | Your obedient Servant | J.E. Tennent
Faraday Esqre F.R.S. | &c &c &c | 21 Albemarle Street

Letter 3091

Faraday to James Emerson Tennent

21 February 1856

From the original in TNA MT10/128, file M2082

Royal Institution | 21 Feb 1856

Sir,

I feel myself honored by your application to me on the part of the Lords of the Committee of Privy Council for Trade¹ in like manner as I feel honored by the Spirit of my occupation & engagement with the Trinity House. I am quite willing to accept the same duty for My Lords as that I perform at the Trinity House under the same circumstances and conditions^[1]

I am Sir | Your very Obedient Servant | M. Faraday
Sir Jas. Emerson Tennent Bart | &c &c &c

Endorsement: The Trinity House give Mr. Faraday a salary of 200£ a year.

1. Letter 3090.

Letter 3092

Lord Wrottesley¹ to Faraday

26 February 1856

From the original in IET MS SC 2

1 Albemarle St | 26 Feb 1856

My dear Sir,

I shall be happy to receive from you & (if possible) before the 13th of March any suggestions which you may be kind enough to favour me with as to the disposal of the 4 R.S. medals this year².

Yours truly | Wrottesley
Professor Faraday

1. John Wrottesley (1798–1867, ODNB). Astronomer. President of the Royal Society, 1854–1858.

2. There is no record of any discussion of Royal Society medals in RS CM, 13 March 1856, 2: 344–5.

Letter 3093**Faraday to Charles Manby****27 February 1856****From the original in WIHM MS FALF**

Royal Institution | 27 Feb 1856

My dear Sir,

I send you the *old notes* I referred to¹. They were not intended for the eyes of others than myself. So keep the book to yourself.

Ever Truly Yours | M. Faraday

C. Manby Esqr | &c &c &c

1. This letter refers to Faraday's examination of the American calculating boy Zerah Colburn (1804–1840, Smith (1983), 181–210) on 1 July 1816. See letter 3094, Davy to Faraday, 1 July 1816, letter 65, volume 1 and IET MS SC2 CPB, pp. 87–100 – presumably the old notes referred to here. Colburn was mentioned specifically in Bidder (1856), 253.

Letter 3094**Faraday to Charles Manby****28 February 1856****From the original in WIHM MS FALF**

R Institution | 28 Feb 1856

My dear Sir,

You are at full liberty to use the information in the account of Zerah Colburn¹ in any way you please²[.]

Ever Truly Yours | M. Faraday

C. Manby Esqr | &c &c &c

Address: Charles Manby Esq | &c &c &c | 25 Gt. George Street | Westminster

1. Zerah Colburn (1804–1840, Smith (1983), 181–210). American calculating boy.

2. See note 1, letter 3093.

Letter 3095**Faraday to John Murray****29 February 1856****From the original in NLS JMA**

Royal Institution | 29 Feb 1856

My dear Mr. Murray,

I am sorry I cannot accept your kind invitation but have you not heard that I never dine out & have accepted no invitations for many years. Indeed however much I might wish it in any particular case I dare not dine out

because of the offence which such an act would justly give to many dear friends[.] With many thanks for your kindness

I am nevertheless | Your most truly Obligated | M. Faraday
J. Murray Esqr | &c &c &c

Letter 3096

Thomas Henry Farrer to Faraday

29 February 1856

From the original press copy¹ in TNA MT4/20, p. 1095

29 Feb 1856

20820

21st. instant², and to inform you that my Lords hear with pleasure that you are willing to give them your valuable assistance.

You state that you are quite willing to accept the same duty for my Lords as that which you perform at the Trinity House under the same circumstances and conditions.

The duties which my Lords will ask you to perform are the following, viz. The inspection of the Lighting Apparatus at the makers in London, and advice in putting the work out to contract, in order to ensure the apparatus being made in a way which would satisfy you when you inspect it. As my Lords anticipate that your service will be required on very few occasions, they propose that your remuneration should be either according to the number of inspections actually performed, or a fixed Salary of say One Hundred Pounds per annum, as may be most agreeable to you[.]

In the hope that these arrangements will meet with your views, my Lords direct me to inform you that the Apparatus for the Lighthouse at Cape Race, Newfoundland, is now at Messrs. De Ville³ in the Strand, and ready for inspection at a day's notice; and my Lords will be glad if you will name an early day to meet the Engineer for the Lighthouse⁴ and Admiral Beechey⁵ at that place.

I am | Sir | Your obedient Servant | T.H. Farrer
M. Faraday Esqr F.R.S | &c &c &c | Royal Institution

1. The draft of this letter is in TNA MT10/128, file M2082.

2. Letter 3091.

3. Deville and Co. Lampmakers of 367 Strand. POD.

4. Alexander Gordon.

5. Frederick William Beechey (1796–1856, ODNB). Naval officer, hydrographer and President of the Royal Geographical Society, 1855–1856.

Letter 3097**Thomas Henry Farrer to Faraday****6 March 1856****From the original press copy¹ in TNA MT4/21, p. 1226**

6 Mar 1856

2446

1st. instant²; and to acquaint you that the arrangement therein mentioned is perfectly satisfactory to their Lordships.

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
M. Faraday Esqr F.R.S. | &c &c &c | Royal Institution | Albemarle Street

1. The draft of this letter is in TNA MT10/128, file M2446.
2. Not found but evidently Faraday's reply to letter 3096.

Letter 3098**Edward Sabine to Faraday****6 March 1856****From the original in IET MS SC 2**

The Royal Society, Somerset House | 6 Mar 1856

Dear Faraday,

Mr. Bate¹ has sent me (with a letter from himself which I enclose) a note from Sir J. Lubbock² (also enclosed) recommending that certain papers in corners marked from No.1 to No.6 should be referred to you; and I accordingly do so. Mr. Bate seems to think that after the inspection of these papers, you may feel free to give such an opinion regarding his claims as will be satisfactory to all parties; that is to say I presume that will satisfy the Treasury of the propriety of paying Mr. Bate³.

Sincerely Yours | Edward Sabine

1. Bartholomew Bate (1806–1895, GRO). Son of Robert Brettel Bate (1782–1847, ODNB), scientific instrument maker in the Poultry, London.

2. John William Lubbock (1803–1865, ODNB). Banker, astronomer and Treasurer of the Royal Society, 1830–1835 and again 1838–1845.

3. This letter refers to the long-running saga of the pursuit for payment for the work done by Bate's father for the Royal Society Excise Committee, of which Faraday had been a member, in the 1830s. See Faraday to Weld, 24 June 1850, Faraday to Sabine, 30 November 1854, and Sabine to Faraday, 4 December 1854, letters 2301, 2922 and 2923, volume 4, McConnell (1993), 54–6, RS CM, 15 June 1854, 2: 288–9 and 26 October 1854, 2: 294.

Letter 3099**Faraday to Walter Baldock Durrant Mantell¹****8 March 1856****From the original in National Library of New Zealand MS Papers 83
(Mantell family) folder 275**

R Institution | 8 Mar 1856

My dear Sir,

Undated tickets are things given on very special occasions. I send you a ticket for next Friday – there are no meetings on the 21st. & 28th. & if after that you wish to come to any meeting & will let me know I hope & dare say I shall be able to grant your request. Next Friday is Mr. Barlow on Aluminium²[.]

Ever Truly Yours | M. Faraday
Walter Mantel Esqr. | &c &c &c

Address: Walter Mantell Esq | &c &c &c | 30 Charing Cross

1. Walter Baldock Durrant Mantell (1820–1895, ODNB). Civil servant in New Zealand on leave of absence.
2. Barlow (1856), Friday Evening Discourse of 14 March 1856.

Letter 3100**Thomas Henry Farrer to Faraday****8 March 1856****From the original press copy in TNA MT4/21, p. 1273**

8 Mar 1856

2531

5th instant, stating that you have examined the Lighting Apparatus for the Lighthouse upon Cape Race Newfoundland¹.

My Lords desire me to inform you that directions have been given to Mr. Gordon the Engineer for the Lighthouse, to have the reflectors put up for experiments as suggested by you.

Mr. Gordon has also been requested to inform you when the reflectors are ready for your experiments.

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
M. Faraday Esqr F.R.S. | Royal Institution | Albemarle Street

1. See letter 3096.

Letter 3101**Faraday to W. Wright¹****11 March 1856****From *Notes and Queries*, 1873, 11: 73**

Royal Institution | 11 Mar 1856

Sir,

I cannot give you my autograph for sale². I always decline to give it, for doing so would be unsatisfactory to my feelings. I send you five shillings, which you may put to the funds, with the letter F. only associated with it; or if such a sum is valueless, give it to any really poor case that may come under your notice.

Will you excuse me, if I point out one or two circumstances in your mode of application which might raise a shadow of doubt in one's mind? In the first place, I do not find your name among the printed names on the papers you sent with your letter. In the next, your letter is dated the 5th of March, and says within that the bearer will call for the papers on the next day (Thursday) evening, and yet it did not arrive here before the 10th March, and the packet containing it has written on it 'will be called for to-morrow evening.' I do not find your name in the *Court Guide*, though you use a seal and motto; neither is it contained in the *Post Office Directory*, at page 625 under the head of Princes St., Stamford St. I have no time to enquire further, but trust that no person is using your name for improper purposes, or any other purpose other than that expressed on the papers.

I am, Sir, your obedient servant | M. Faraday
Mr. W. Wright

1. Unidentified.

2. Wright had asked a number of eminent individuals for their autograph which would then be sold to raise funds for the London Female Dormitory in the Euston Road. *Notes and Queries*, 1873, 11: 73.

Letter 3102**Henri Etienne Sainte-Claire Deville¹ to Faraday****14 March 1856****From the original in IET MS SC 2**

14 Mar 1856

offert à M. Faraday par son Ami par son bien reconnaissant Serviteur H. Deville, Cet échantillon d'aluminium n'est pas pur: il a été malheureusement préparé dans des vases de Cuivre et il Contient assez de ce

métal pour que:

1° il ait perdu une partie de sa malliabilite et de sa durabilite,

2° il noircisse à l'air par fait de la formation de Sulfure ou de l'oxyde de Cuivre –.

3° il laisse un residu de liquidation lors qu'on le fond et qu'on le coule à basse température.

4° Sa sonorité fût notablement diminuée

H. Sainteclair Deville

TRANSLATION

14 Mar 1856

Offered to Mr. Faraday by his most grateful servant H. Deville. This aluminium sample is not pure: it was unfortunately prepared in copper vessels and it contains enough of this metal such that:

1st It has lost one part of its malleability and its durability.

2nd It darkens in air because of the formation of copper sulphide or oxide.

3rd It leaves a liquid residue when it is melted or cast at low temperature.

4th Its sonority is notably diminished.

H. Saint-Claire Deville

1. Henri Etienne Sainte-Claire Deville (1818–1881, DSB). Professor of Chemistry at Ecole Normale Supérieure, 1851–1880.

Letter 3103

Alexander Fischer von Waldheim¹ and Karl Ivonoviè Renar² to Faraday

16 March 1856

From the original in RS MS 241, f. 142

Société Imperiale des Naturalistes de Moscou

Moscou | le 16/28 Mars 1856

Monsieur,

La Société Impériale des Naturalistes de Moscou, dans sa séance solennelle du 28 Décembre 1855 à l'occasion du Jubilé de son cinquantième anniversaire Vous a nommé son membre honoraire et s'empresse de vous en faire parvenir le diplome.

Le Vice-Président A. Fischer de Waldheim.

Le Premier Secrétaire Dr. Renard

à Monsieur le Professeur Michel Faraday à Londres.

TRANSLATION

Imperial Society of Naturalists in Moscow
Moscow | 16/28 Mar 1856

Sir,

The Imperial Society of Naturalists in Moscow, in its ceremonial meeting on 28 December 1855, on the occasion of the jubilee of its fiftieth anniversary, has nominated you an honorary member and hastens to send you the diploma.

The Vice President A. Fischer de Waldheim.

The First Secretary Dr. Renard

To Professor Michael Faraday in London

1. Alexander Fischer von Waldheim (1803–1884, DBE). German-born botanist working in Moscow.

2. Karl Ivonoviè Renar (1809–1886, ES). Russian physician.

Letter 3104

Faraday to Edward Frankland

18 March 1856

From the original in the possession of Mrs. Raven Frankland

Royal Institution | 18 Mar 1856

My dear Frankland,

I cannot assent [to] such an application & shall wait your instructions. I shall be out of London for the next fortnight. – I send you back the excellent Resin though greatly tempted to keep it. If you have several of these as illustration of the [word illegible] & can spare me one how grateful I shall be.]

Ever Truly Yours | M. Faraday

Address: Dr. Frankland | &c &c &c | St. Mary's Terrace | Windermere

Letter 3105

Karl Wilhelm Knochenhauer to Faraday

20 March 1856

From the original in IET MS SC 2

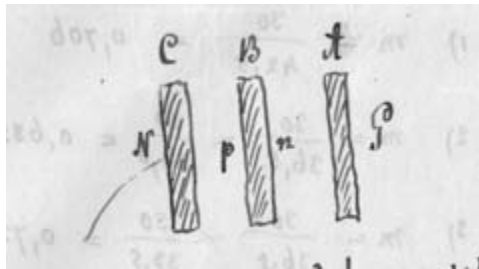
Meiningen (duché de Saxe) | le 20 Mars 1856

Monsieur,

Je Vous prie d'excuser, que j'ose Vous incommoder encore une fois, mais Votre lettre adressée à Mr. Riess¹, que j'ai trouvée dans les Annales de Poggen-dorff et que j'ai lue avec un très grand intérêt, contient un passage, dont je désirerais une courte explication. Vous dites dans la note (8), ajoutée à la lettre de Mr. Riess, "que les phénomènes de la capacité inductive spécifique sont acceptés aujourd'hui"². Ne croyant pas, que de ces expériences de Mr. Riess, qui effacent presque toute différence entre les corps isolateurs et les corps conducteurs, Vous dériviez une approbation de Vos phénomènes, je vous prie de

me dire, où se trouvent de nouvelles expériences publiées sur cet objet. Elles auraient pour moi un intérêt particulier, parce que moi aussi j'ai publié après ce Mémoire de Mr. Riess, que Vous avez examiné si complètement, quelques expériences sur la capacité inductive des isolateurs, qui sont insérées dans les Annales de Pogg. (1854) XCII[I] p 407³. Les anciennes disputes qui ont eu lieu entre Mr. Riess et moi ne me permettaient pas de discuter toutes ses expériences; c'est pour cela que je me suis borné à répéter et à prouver Vos expériences données dans la Série XI⁴, croyant que ces expériences ne peuvent être expliquées selon „la théorie ancienne”⁵, nom fort bien choisi.

Si vous ne le trouvez pas arrogant de ma part, j'ajouterai qu'il y a peut-être une explication plus facile au lieu de celle, que Vous avez donnée dans la note (5)⁶. D'après mes expériences citées dans „Beiträge zur Electricitätslehre” p. 79⁷ la quantité (m) de l'électricité induite par une plaque sur une autre est donnée par la formule $m = a/b + x$, où a et b sont des constantes, qui dépendent de la grandeur des plaques et x en mesure la distance. Pour mes grandes plaques je trouvai $a = 37$ et $b = 37,5$, supposé que x soit donné en lignes⁸. Maintenant



soit la plaque A chargée d'électricité positive et B représente une plaque de métal isolée, interposée entre A et la plaque C non-isolée, sur laquelle on cherche l'électricité induite; supposons que la distance x entre A et C soit de 12 lignes et que B se trouve à 6 lignes de A, supposons outre cela, que dans notre cas, où les plaques sont plus petites, les constantes a et b soient = 30,0 et 30,5 et que les quantités d'électricité positive et négative sur les deux faces de B soient égales, nous aurons dans les cas divers (1) que la plaque B est éloignée (2) que cette plaque a une épaisseur de 0,04 pouces = 0,5 lignes (3) que la même plaque a une épaisseur de 0,25 pouces ou de trois lignes, la quantité (m) d'électricité négative sur la plaque C :

1. $m = 30/42,5 = 0,706$
2. $m = 30/36,5 \times 30/36,0 = 0,685$
3. $m = 30/36,5 \times 30/33,5 = 0,736$

Ce sont les mêmes résultats que ceux qu'ont fournis les expériences de Mr. Riess, mais qui sont tout-à-fait d'accord avec Vos idées.

Je saisis cette occasion de Vous renouveler l'assurance de la haute estime avec laquelle j'ai l'honneur d'être

Monsieur | Votre très dévoué et très | obéissant serviteur |
K.W. Knochenhauer.

TRANSLATION

Meinigen (Duchy of Saxony) | 20 Mar 1856

Sir,

Please forgive that I dare to inconvenience you once more, but your letter to Mr. Reiss¹, which I found in Poggendorff's *Annalen* and read with great interest, included a passage of which I would like a short explanation. You say in note (8), added to Mr. Reiss' letter, "that the phenomena of the specific inductive capacity are now accepted."² Not believing that from these experiments of Mr. Reiss', which efface nearly the whole difference between insulating and conductive bodies, you would derive confirmation of your own phenomena, I ask you to tell me, where I can find published some new experiments on this subject. They would have a particular interest, because after this Paper of Mr. Reiss', that you examined so carefully, I too published some experiments on the inductive capacity of isolators, which are inserted into Poggendorff's *Annalen* (1854) XCIII p. 407³. The old quarrels between Mr. Reiss and myself do not permit me to discuss all his experiments; it is because of this that I am limited to repeating and proving your experiments given in Series XI⁴, believing that these experiments cannot be explained according to "the ancient theory"⁵ – a well chosen name.

If you do not find it presumptuous on my behalf, I would add that there is perhaps an easier explanation than the one you have given in note (5)⁶. According to my experiments quoted in *Beiträge zur Electricitätslehre* p. 79⁷, the quantity (m) of electricity induced by one plate on another is given by the formula $m = a/b + x$, where a and b are constants which depend on the size of the plates, and x represents the distance between the plates. For my large plates I found $a = 37$ and $b = 37.5$, supposing x is in lines⁸. Now let plate A be charged with positive electricity and B represent a plate of insulated metal, placed between A and the non-insulated plate C, on which we try to find induced electricity; let us suppose that the distance x between A and C is 12 lines and that B is 6 lines from A, let us suppose, in addition, that in our case, where the plates are smaller, the constants a and b are 30.0 and 30.5 and that the quantities of positive and negative electricity on the two faces of B are equal, we shall have in these various cases (1) that plate B is further away,

(2) that this plate is 0.04 inches = 0.5 lines thick (3) that the same plate is 0.25 inches or 3 lines thick, the quantity (m) of negative electricity on sheet C:

1. $m = 30/42.5 = 0.706$
2. $m = 30/36.5 \times 30/36.0 = 0.685$
3. $m = 30/36.5 \times 30/33.5 = 0.736$

These are the same results that were provided by Mr. Reiss' experiments, but are not altogether in agreement with your ideas.

I seize this opportunity to renew the assurance of the high esteem with which I have the honour of being | Sir | Your very devoted and | very obedient servant | K.W. Knochenhauer.

1. Faraday to Riess, 19 November 1855, in Faraday and Riess (1856), 415–28.
2. *Ibid.*, 440.
3. Knochenhauer (1854a).
4. Faraday (1838a), ERE11.
5. Faraday and Riess (1856), 440.
6. *Ibid.*, 439.
7. Knochenhauer (1854b), 79.
8. A unit of length (=2.256 mm) used by watchmakers. Darton and Clark (1994), 248.

Letter 3106

Faraday to Arthur-Auguste De La Rive

21 March 1856

From the original in BPUG MS 2316, f. 85–6

Royal Institution | 21 Mar 1856

My dear De la Rive,

Though unable to write much I cannot longer refrain from acknowledging your kindness in sending me such a remembrance of you as the *Vol II*¹ and in giving utterance to the great delight with which I have read it. I rejoice to think that such a work should be reprinted in the English language for now when asked for a good book on Electricity, I know what to say. I will not say that I envy you for your wonderful stores of knowledge regarding all that concerns our beloved science but I cannot help contrasting your power with mine and wishing for a little of the ability by which a mind such as yours calls up to present remembrance what it had found worthy to lay up in its treasury. But we both have reasons of a higher nature than any that science can afford to be thankful for that we have received & not to forget the many benefits bestowed upon us and I hope that I am not envious of you or of any man but would rather rejoice in your exaltation. With the kindest remembrance of Madame de la Rive & of yourself

I am My dear friend | Most truly Yours | M. Faraday

Address: Monsieur | Mons A. de la Rive | Geneva

1. De La Rive (1853–8), 2, published in 1856.

Letter 3107

Faraday to Christian Friedrich Schoenbein

21 March 1856

From the original in UB MS NS 421

Royal Institution | 21 Mar 1856

My dear Schoenbein,

The heartiest & the kindest wishes to you and the best thanks for Your last letter¹_[.] I have it not here (Norwood, for I am resting a head like a sieve) but I know it was very pleasant, and I think contained some family details which made me long to be with you – but the fact is that when I am with a friend I soon need to get away again because of the labour of conversation and its strain upon recollection. That which induces me to write just now is the fact, that a letter has been sent to me addressed Dr. Schoenbein Royal Institution Albemarle Street & then in a corner care of Dr. Faraday. I have waited a little while to see if any instructions would arrive, but have heard no more_[.] Now what would you wish me to do with it? It is a heavy letter and if sent by Post would cost about ten shillings and yet I cannot help suspecting that it is only a pamphlet. So I thought it best to write & ask you what you would wish me to do & whether you know of and are expecting it.

I do not recollect any news and I shall be glad to lay my head down again, so with kindest remembrances to Madame Schoenbein & your damsels remain as ever | Your Affectionate friend | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | on the Rhine

1. Letter 3054.

Letter 3108

Mario Giardini¹ to Faraday

24 March 1856

From the original in IET MS SC 2

Naples | 24 Mar 1856

Sir,

A discovery which must interest you, as it relates to the application of the terrestrial magnetism alone to set in motion a magnet of great magnitude to produce such currents as to show brilliant sparkles, intolerable electric shocks, and the quick decomposition of water. This magnet may activate the electric Telegraphs; for I have demonstrated that its energy follows the ratio

of the masses of iron, which are used with the correspondens of multipliers. It would be interesting to me to learn your opinion, Sir, concerning the explanation of the strange phenomena physical – chemical – physiological, which are excited in a temporary magnet from the terrestrial magnet alone. The apparatus is identical to that of Clark; it only differs from it in the dimensions. It consists of 3 bands of iron wire twice made red hot, of the diameter of 2 millimetres, and 1 meter and 12 centimetres long: each weighing 18 grammes: the 3d bent in the shape of a horse shoe, so that the straight points remain distant from each other 95 centimetres: 4 Tubes are placed at hard friction: 2 in the whole length of the long bands, and 2 in those which are supported by the bending band, which will act as anchor of the magnet. Over it others identical are there placed to a light rubbing, to be easily taken away when requisite. Around the circumferences of these 2 tubes, circular brass fenders are to be soldered. To the whole length of the fenders, place 7 rows of spirals of copper wire of 2 millimetres and half diameter, covered with cotton ribbands wet in a solution of gum lack. On a fitted apparatus they are placed parallel to the horizon, and 25 centimetres distant from each other, and their extremities free are opposite to the ends of the 2 iron [m]asses of the 2 multipliers. In each ball are involved 1000 metres of Copper wire, covered with Silk a millimetre diameter. The multiplier with a fit engine may wheel rapidly almost in contest with the free extremity of the magnet, while the opposite ends are brought in contact with the bending band. It must be observed that the phenomena do not fail taking away the anchor. After keeping these magnetic bands one month between the magnetic poles of the earth, without the spirals, they are seen lightly magnetized so as to draw the end of a very thin iron wire, holding in the fingers the opposite end. By giving rotatory motion to the multiplier the band shall not only attract more strongly the iron wire, but shall hold it in adherence to them, and others much heavier shall be attracted by the same bands: but acting the rotation of the multipliers both wires drop. This experiment shows that during their motion, the magnetisation of the bands increases: and to demonstrate with more evidence it is sufficient to place 2 magnetized needles suspended into the inside of 2 crystal balls, and adapted to the ends of one of the bands, shall readily show that their ends are magnetized with bipolar-magnetism, and shall place themselves parallel to the direction of the band. By the rotatory motion of the multipliers the needles shall incline towards the same band, which shows the increase of its magnetization: at the end of the rotation of the multipliers the needles shall resume their former position. Placing a torpid Galvanometre to a needle within the circuit of the currents of the bare bands, making use of balls having 600 metres of copper wire involved each of the diameter of 2 millimetres, covered with silk, in one half rotation of it the needle describes an arch of 10 degrees, setting in action those of tension the needles scarcely deviating of a few degrees. Whether employing the multipliers

of tensions, or those of quantity none of the 3 phenomena appears. Placing the spirals in the 3 bands, and giving action first to the long ones with the multipliers of quantity, and placing within the circuit the Galvanometre, in one half rotation of the balls, the needle describes an arch of 110 degrees. It is surprising to observe that the spirals having their ends free in the air, or joined to the close circuit the galvanometre being in communication with the little cups of mercury, double compartment, the direction of the needle shall also be of 110 degrees. How can this phenomenon be explained? It must be acknowledged that the magnet being in the magnetic ambient, the dis-equilibrium induced by the extremely high magnetism of positions of the bands, determines the absorption of the of magnetism of the same ambient by the free ends of the spirals: or when these are joined to the close circuit for it is impossible, that the high magnetism of the bands, which causes the needle to deviate 10 degrees could excite a current introduced into the spirals, to cause the galvanometre needle to describe an arch of 110 degrees: and it is known that in the temporary magnetic moved by the Voltaic Pile, the current enters through the ends of the spirals, magnetising the iron, round which it revolves: likewise the terrestrial magnetism derives from the external part of the spiral on the iron wire bands to magnetize it energetically absorbing it, whether through the free ends of it, or through its mass when disposed in close circuit.

Phenomena which appears in the apparatus set in action.

The sparkle is observed as well in the brass commutators as in the paring disks in which splits are cut on the front face of it, when they are placed in the axes of rotation, and fixing the end of a copper wire in the hole through the plate of the hoele, while the opposite end already sharpened opposite to the splits of the brass plate, wheeling the multipliers, the sparkles appears, and in Various lines according to the metal used. In the mercurial apparatus the Sparkle is extremely brilliant and constant, and by placing in the circuit the flat spirals 400 metres long becomes even more brilliant, placing in the circuit of that current 6 masses of coals being 100 kilograms each, residue of the distillation of the coal the sparkle will be more and more brilliant.

The shoke and the decomposition of water shall be obtained as in Clark's apparatus: exceedingly strong, and almost intolerable is the former, most distinct the latter: and these phenomena are observed nearly with the same intensity, bringing the magnet into the various arimists on the horizon, and what is more wonderful even being perpendicular to the plane of the magnetic meridian. The magnet is equally active when placed perpendicularly to the horizon near the multipliers – hence I feel more convinced that absorption of the terrestrial magnetism on the bands of the spiral excited the dis-equilibrium caused in the one, and in the others from the currents of the magnetism of position of the bands, at the moment of the rotation of the multipliers be the true cause of so singular phenomena: therefore it shall not be

strange to assert, that as the electric machinery desires upon its Conductor the statical electricity of the earth, so to the temporary magnet flows the dynamical electricity from the earth itself.

And when you, Sir, have granted approbation to these theoretical conceits I can freely tell to the european Physicians conform yourselves to my theory, because the first physician of our ages, Farad[a]y, agrees with me.

Pray, Sir, honour me with a speedy answer | while I am | Your most obedient Servant | The Physical Professor of the | Neapolitan V. University | Mario Giardini

Address: À Monsieur | Mr. le Profr. Faraday | à | Londres

1. Unidentified.

Letter 3109

Julius Plücker to Faraday

24 March 1856

From the original in IET MS SC 2

Bonn | 24 Mar 56

Dear Sir!

Looking at the date of your last kind letter¹, I am very much surprised my silence had been so long a one. Being another time by election at the head of the University, I am for a year rather intirely distracted from scientific working. Therefore, that I may not fall into my former indolence, I write to you the very first day of "vacances".

My best thanks for your last interesting paper². There is one conclusion in it, I fully adopt. The paramagnetic induction is augmented by the reciprocal action of the particles of the induced body. The diamagnetic induction must be *weakened* by the same reason. When the contrary would be proved by experiment, I shall be forced to give up diamagnetic polarity. But ancient experiments dont encourage myself, to take up again this "experimentum crucis".—

Since several years doubts rose in my mind against the theory I had imagined to connect together the complicated magnetic phenomena, exhibited by crystals. In crystals with only one optic axis, acted upon by a magnetic pole, this axis may be theoretically regarded as a line of polarity, or as the direction of resulting magnetic action. In biaxial crystals I (badly) imagined two such lines of resulting magnetic action, of which depends the position of the crystal between the poles. No experiment contradicted this theory, till July last I undertook a new series of experiments. With the same flexibility of mind, I modified before the theory, when forced by new discoveries, I now altogether give it up, substituting to it a new one, which gives to the

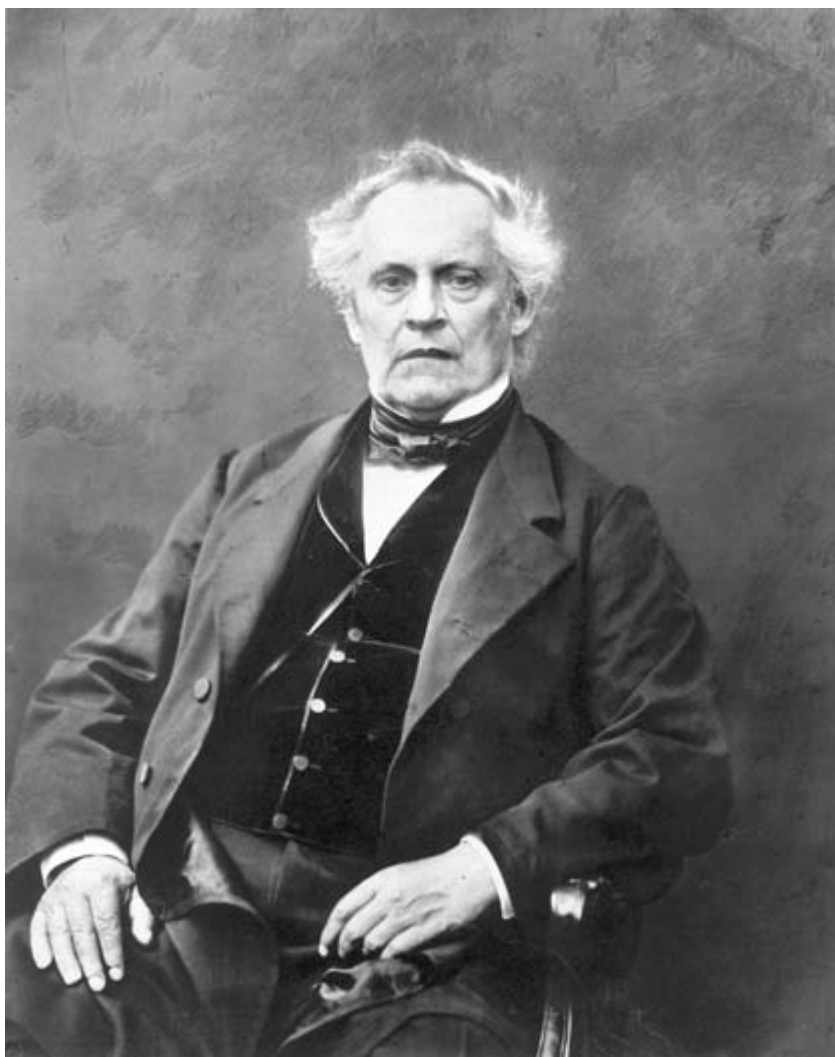


Plate 4. Julius Plücker. From RI MS F1 I188.

various observed facts a new interpretation. I communicated, on Mr. Moigno's demand, a first account of it in the *Cosmos*, September last³, when I was in Paris. Cutting of the introductory phrase and the Epilogue you will find my own words. Unhappily I got no copy to send it to you.— To the former two axes I substitute two new ones, depending upon the crystalline structure of the crystal. When the crystal suspended between the two poles, in such a way that one of these two axes be perpendicular, *there is no extraordinary magnetic action*. These two axes for instance, in the case of "cyanure rouge de fer et potasse" are placed in the plane of the two optic axes, both systems of axes have the same middle line, only the angle included by the magnetic axes is about 42° , while the angle included by the optic axes is only 20° &c &c.

When you say any part, that my theory does not hold, you mean surely that theory "octroyée" to me by Mr. Tyndall. The phrase I at first employed may be translated also into English: "the force which produces this repulsion is independent of the magnetic or diamagnetic condition of the mass" (Phil. Trans. 1855 p 2⁴) but its real meaning was "the force exists in both cases, the mass may be either magnetic or diamagnetic." Thus it was understood by all german philosophers I know.

Mr. Tyndall's laws printed with italic letters in his paper p. 12 & 13⁵ signifie, I think, nothing at all. Indeed how may you observe the magneto-crystallic action if not by the displacement of a diamagnetic body from the equatorial to the axial position, of a paramagnetic body from the axial to the equatorial? Mr. Tyndall fights against my theory of two conflicting forces, which never did exist (p. 13). The fact is that I gave [in] 1849 before he published any paper, an mathematical expression of the force (couple) emanating from the pole of a Magnet and acting upon an uniaxial crystal⁶. This expression contains two terms with opposite signs (\pm), indicating that the whole force may be mechanically divided into two forces, acting upon the crystal in opposite sense. You may call them conflicting forces; whether of both predominates depends on the distance from the poles. I *never* theoretically admitted a magnetic action on an ideal line, in conflict with the common magnetic or diamagnetic action. After fighting against a theory, which never was mine, Mr. T. gives in the Appendix (p. 44 seq.)⁷ the very same explication I gave [in] 1849 to explain by a popular fact the principle of my calculus, relating to uniaxial crystals.

After having given to you, I hope in an intelligible English, these incomplete explications, a stone, as we say in German, is fallen from my breast. Instead of fruitless discussions I think it more proper to present to the Roy. Society an elaborate paper, containing the general theory of meagnetocrystallic action founded on a new series of experiments⁸. I have all reason to hope that the new theory will be generally adopted; it is no

more contradicted by Poisson's⁹, Green's¹⁰ & Thomson's calculus based on molecular action.

Dear Sir! The extreme kindness, with which you received my very first experimental researches is that moment in my scientific life, at which I look bak with the greatest satisfaction. When recently I had the honour to be elected a Foreign membre of the Royal Society¹¹, the origin of it is to be found only in that kindness.

With all my heart and for ever | Yours | Plücker

1. Faraday to Plücker, 19 September 1854, letter 2901, volume 4.
2. Probably Faraday (1855b), [ERE29b].
3. Plücker (1855).
4. As quoted in Tyndall (1855), 2.
5. *Ibid.*, 12–13.
6. Plücker (1849), 427–31. See Plücker to Faraday, 4 December 1849, letter 2237, volume 4.
7. Tyndall (1855), 44–51.
8. Plücker (1858f).
9. Siméon-Denis Poisson (1781–1840, DSB). French mathematical physicist.
10. George Green (1793–1841, ODNB). Mathematician in Nottingham and then Cambridge.
11. He was elected on 14 June 1855.

Letter 3110

Christian Friedrich Schoenbein to Faraday

25 March 1856

From the original in UB MS NS 422

Easter-Tuesday 1856

My dear Faraday,

I hasten to tell you that there is not the slightest occasion for your being in a hurry regarding the parcel you talk of in your last letter¹. I do not know what it contains nor who sent it to you. Any opportunity therefore, an early or a late one, will do for sending the thing over to Basle.

I think I told you some months ago that the friend whom you charged with delivering the third volume of your researches² to me last year, has not performed his task, and not liking at all the idea of losing your valuable gift, I repeatedly ask you the favor of looking a little after the miscarried book.

This time I shall keep my peace on scientific matters from the simple reason that I could not tell you much even if I had the inclination to do so. It is true, I was not quite lazy nor did I work quite for nothing last winter, but the exploits I performed are, as we Germans use to say, but half-laid eggs and of such embryonic things it is not safe to talk.

I have however a mind to entertain you of another matter more interesting, at least less dry than that never ending subject of Oxygen.

Yesterday it was Easter-Monday and you must know that in our teutonic lands it is a great day to the whole juvenile world. I consider it as one of our specific national qualities that we are very fond of children and have marked out a number of days and times of the year round for the enjoyments of our little ones. Now such a time is Easter-time and such a day easter-Monday. Many, many weeks before it comes, the little prattlers talk of nothing but of the Easter-hare (Oster haase in german) and the gifts he may chance to bring and what that Easter hare means you will easily infer from what passed in the garden of Mrs. Wiedemann³ yesterday afternoon. A host of children were invited by that Lady (having herself a little boy of four years of age⁴) to make their appearance at her house at three o'clock, punctually. Mothers and elder sisters conducted the little guests to the appointed place at the fixed time and being assembled in a room they anxiously and impatiently expected there the announcement of "The hare has laid his eggs". No sooner had these words been finished than the rogues were seen running down the staircase into the garden, dispersing themselves in all directions and eagerly seeking for the eggs being put in hidden places: within hedges, behind bushes &c. The discovery of each egg was hailed with joyful exclamations and never failed proving both to the happy finders and the unsuccessful seekers a fresh stimulus to continue their searches. But you must not imagine those "hare eggs" to have been ordinary ones; they were beautifully colored: blue, red, yellow, lilac, brown, even variegated, and bearing all sorts of inscriptions: the names of the Children invited, the drafts of hares, foxes and other animals.

The eggs found by the boys and girls were put in a basket placed in the centre of the garden, as property of the little common-wealth to be equally divided at the end of the festival and carried home by the Children as the trophies of the day.

Such like fêtes there were hundreds in our town yesterday and I dare say millions all over the german lands. Great a philosopher as you are, sure am I, that such a sight would give you more pleasure than all the scientific institutions and all the curiosities of the whole civilized world together. Now I am at the end of my letter and have nothing more to say than that I shall ever remain

Your's | most faithfully | C.F. Schoenbein

Endorsed by Faraday: Sent by Mr. Roscoe about April 6th

Address: Dr. Michael Faraday | &c &c &c | Royal Institution | London

1. Letter 3107.

2. Faraday (1855c). See letter 3035.

3. Klara Wiedemann, née Mitscherlich who married Gustav Heinrich Wiedemann in 1851. She was the eldest daughter of the Professor of Chemistry at Berlin University, Eilhard Mitscherlich (1794–1863, DSB). Schütt (1997), 82.

4. Eilhard Ernst Gustav Wiedemann (1852–1928, P3, 6). Later a physicist.

Letter 3111**Pieter Bleeker¹ and J.J. Altheer² to Faraday****27 March 1856****From the printed original in RS MS 241, f. 147**Société des Sciences des Indes Néerlandaises,
Batavia | 27 Mar 1856

La Société des Sciences des Indes Néerlandaises à Batavia, désirant étendre de plus en plus le cercle de son activité, et faire hommage aux savants étrangers, illustres dans les Sciences Naturelles, a résolu dans la séance générale du 28. Février 1856, de vous nommer MEMBRE CORRESPONDANT.

Espérant que cette nomination vous sera agréable, nous vous prions de vouloir bien agréer l'assurance de la haute considération avec laquelle nous avons l'honneur d'être.

Vos dévoués Serviteurs,

Bleeker | Président

J.J. Altheer | Secrétaire

A | Monsieur M. Faraday | à Londres

TRANSLATIONSociety of Sciences of the Dutch Indies,
Batavia | 27 Mar 1856

The Society of Sciences of the Dutch Indies, in Batavia, would extend more and more the circle of its activity, and to pay tribute to the foreign savants, eminent in the Natural Sciences, resolved at its general meeting on 28 February 1856 to nominate you CORRESPONDING MEMBER.

Trusting that this nomination will be agreeable to you, we ask you to believe the assurance of the high consideration with which we have the honour of being

Your devoted servants,

Bleeker | President

J.J. Altheer | Secretary

To | Mr. M. Faraday | in London

1. Pieter Bleeker (1819–1878, NNBW). Dutch physician and naturalist in the East Indies.

2. Unidentified.

Letter 3112**Jacob Herbert to Faraday****28 March 1856****From the original in GL MS 30108/2/68**

Trinity House, London | 28 Mar 1856

Sir,

The Elder Brethren being desirous of obtaining such practical information in respect of the best Construction of Fog Bells and the Appliances to

be used in sounding them, as may enable them to adopt generally a more efficient system than that at present in use which has at several of the Light House Stations been found defective,— I am directed to signify their request to be favored with your opinion as to the best Scientific Machinist with whom you would recommend that they should place themselves in Communication on this Subject.—

I am, Sir | Your most obedient Servant | J. Herbert
M. Faraday Esq: F.R.S. | &c &c &c

Letter 3113

Faraday to Jacob Herbert

29 March 1856

From the original copy in GL MS 30108/2/68

Royal Institution | 29 Mar 1856

Dear Sir,

I regret that my pursuits have not brought me into connexion or acquaintance with the class of men to which your letter respecting Fog bells refers and am not able to name any one whom I could of my own knowledge recommend to the Elder Brethren¹. I have already sent to the Deputy Master² those papers which contain all the information I have regarding this subject[.]

I am | My dear Sir | Your faithful Servant | M. Faraday
Jacob Herbert Esq | &c &c &c

1. See letter 3112.

2. John Shepherd.

Letter 3114

Faraday to Edward Frankland

4 April 1856

From the incomplete original in the possession of Joan Bucknall

[Royal Institution embossed letterhead] | 4 Apr 1856

My dear Frankland,

Your letter puzzled me for I am averse to any personal activity in the declaration of my own character. Nevertheless I told a niece of mine:— and she has thrown together a few rough notes which I send you in a

they are left imperfect & bad in construction & grammar but if they go into German the writer will be able the more easily to deal with them¹ — I have not yet received the portraits but I take the opportunity of thanking you humbly for the

1. This may possibly refer to Faraday's entry in P1, published in 1863.

Letter 3115**George Gabriel Stokes to Faraday****7 April 1856****From the original in IET MS SC 2**

Pembroke College Cambridge | 7 Apr 1856

My dear Sir,

In looking over the proof of your paper for the Phil. Trans.¹ one or two things struck me as needing alteration, but as I always like to have the author's sanction for such things I return you the proof to look at. Please return it to Messrs Taylor & Francis as soon as you conveniently may. The following are my observations. Art 3371. One alteration frequently involves another which the alterer does not perceive because his attention is directed elsewhere. The "if" was formerly understood after "production" being expressed after "effects" but now the "if" is cut out. Would you like the sentence to read "To shut out currents of air as much as possible, and prevent their production"?

— Foot note to p. 8. I presume varnish should be varnished.

— Foot note p. 10. I have inserted "vol p. ".

— Art 3396 l.5 "they" qy "it"? i.e. the fluid.

— Art 3397 last line qy ^ an

— Art 3399 l.3 abstracted or subtracted?

— P. 14. l.1. Would "abundantly large enough" be better?

— Art 3426. l.5. from end. As the sentence stands "it" would refer to "change". I suppose what is meant is at high temperatures *the body* loses more or less of it.

— The queries at 3399 (end) and 3429 refer to a matter of taste.

I am dear Sir | Yours very truly | G.G. Stokes

Professor Faraday | &c &c &c

1. Faraday (1856c), ERE30. All these changes were made. See also the manuscript in RS MS PT 52.8.

Letter 3116**Faraday to Julius Plücker****8 April 1856****From the original in NRCC ISTI***Private*

Royal Institution | 8 Apr 1856

My dear Plücker,

I have been and am suffering from much weariness & giddiness in the head and have, in consequence, delayed writing several days in hopes I should feel better; but as that is not the case, I will not delay any longer replying to your last¹, since I think you will be wishing for my answer. I am very sorry for the sort of feeling which seems to have arisen between You

and Tyndall, and would do all I could to remove it; but I know the great difficulty having had several of these cases before, and never found that much good could be done. I laboured hard in that between Du bois-Reymond² & Matteucci³, but I do not think with any good result. It is a pity;— for I cannot but believe that Science may be pursued, and the most opposite notions be entertained, by two parties, who yet can be on most open & friendly terms with each other. My memory is so imperfect now, that I am in a very bad condition when I want to recall the points about which differences arise, and I cannot recollect to what you may refer at that part of your letter “when you say any part that my theory does not hold you mean surely that theory “octroyée” to me by Mr. Tyndall.” I think I have seen the gradual development of your thoughts since the first forthcoming of your most beautiful results in Magnecrystallic action. You like all of us, have *developed* and as the facts came forth, your views & reasoning enlarged with them, but I do not recollect that Tyndall ever offered or that I ever drew from him my understanding of your views. Tyndall and I differ in toto on some points – but we do not differ in our friendly views because of that. He considers that his last paper in the *Philosophical Transactions*⁴ establishes a reverse polarity in bismuth; I believe it simply shews in an extensive & perfect manner the complete antithesis of Iron & bismuth which however was known before.

Of course all the points regarding discovery or the precedence of one philosophers views before another, in respect of date, can be settled perfectly by reference to dates of published communications; and when I have occasion to make historical statements, which I have had to do in regard to both Davy & Wollaston⁵, I thought it best to make them as direct & brief as possible, – to publish them in the journals, & then to leave them. I never thought it wise or effectual to mix philosophical development and controversial matter together; and therefore I agree heartily with your words when you write “instead of fruitless discussions I think it more proper to present to the Royal Society an elaborate paper⁶ containing the general theory of magneto-crystallic action founded on a *new series* of experiments”. I have not been able for several years to occupy myself with the Royal Society or its management, & therefore do not know what its guiding principles are now; but the rules were (formerly) to admit no matter that had already been published elsewhere, nor any controversial matter, – nor any merely hypothetical matter; and on the whole I think they are very good rules, & have always tried to conform to them[.]

And now my dear friend I hope this cloud will soon pass away. I always try to forget such points as these; and when I meet with a controversial passage in a paper, endeavour to prevent it from exerting the least influence on my mind; and the fact is, that I cannot, & do not want to, remember the passages in Tyndalls paper which seem to have annoyed you. If I saw them now I do

not believe they would cause me to come to any conclusion that I should not arrive at without them; for I naturally doubt such passages in all cases.

Long may you labour as you have done for the good of science; & one thing I think I learn from your letter, – that you are in good health & excellent working condition. For my own part, I look with great thankfulness to my own personal share in the work, as to a thing that *has been* and is passing away; but it has left me cheerful, and happy in watching the pursuits of others – that band of brothers which has done so much in these last years of time for the advancement of science[.]

Ever My dear Plucker | Yours Most truly | M. Faraday

1. Letter 3109.

2. Emil Heinrich du Bois-Reymond (1818–1896, DSB). Electro-physiologist. Associate Professor at University of Berlin, 1855–1858.

3. See Faraday to Matteucci, 3 March 1853, letter 2647, volume 4.

4. Tyndall (1855).

5. William Hyde Wollaston (1766–1828, ODNB). Man of science. Worked on physiology, chemistry and physics. Secretary of the Royal Society, 1804–16, President 1820. For the historical statements see Faraday (1823, 1836).

6. Plücker (1858f).

Letter 3117

George Gabriel Stokes to Faraday

8 April 1856

From the original in IET MS SC 2

Pembroke College Cambridge | 8 Apr 1856

My dear Sir,

I quite forgot in writing to you to mention¹ one point which struck me, which is the running heads. I think that in a work like the *Philosophical Transactions*, consisting of various memoirs, there is an inconvenience in a variety of heads to the same paper. For suppose that one paper, say yours², had a variety of heads, and suppose some one was looking for a paper not yours, of which it might be he did not know the *exact* title. If each paper had the same running head throughout, when he came to a new heading, and having read it found it was not what he wanted, in turning rapidly over the pages his eye would tell instantaneously so long as he was turning over the leaves of the same paper; but when he came to a fresh heading he would have to pause. Your paper is divided into sections in such a manner that any part wanted can readily be found, so that a variation in the running heads is not required. For the reasons I have mentioned I should prefer the same heading throughout, but I don't much care. The volumes of the *Phil. Trans.* in my room do not happen to have your researches in them, so I don't know what your

previous practice has been, & it is too late now to refer to the library. Unless in your previous papers you have been in the habit of varying the running heads I should prefer a constant heading. If you have hitherto used a uniform heading for the same paper, and do not care about breaking through your old practice, perhaps you would be good enough to write to Messrs Taylor giving the heading you would wish to have; but if you have been in the habit of varying the headings it had best be let stand³.

I am dear Sir | Yours very truly | G.G. Stokes
Professor Faraday

1. In letter 3115.

2. Faraday (1856c), ERE30.

3. The running heads did vary in this paper as had been Faraday's earlier practice.

Letter 3118

Faraday to Edward Frankland

11 April 1856

From the original in the possession of Mrs. Raven Frankland

[Royal Institution embossed letterhead] | 11 Apr 1856

My dear Frankland,

Three times have I been just on the point of starting waiting only for the post & three times have your letters caught me. This morning I have been to Mr. Beard¹ in the city & I only hope the results may satisfy you as for me I never form a judgment in my own case. I have also received the *Rose* & think it excellent:— it will go into my book at once but I fear my inadvertent request has put you to some expence I only wish I knew what it was²[.]

Ever my dear Frankland | Yours most truly | M. Faraday

Address: Dr. E. Frankland | &c &c &c | Owen's College | Manchester

1. Richard Beard (1802–1885, B4). Photographer.

2. This refers to a lithograph of the Professor of Chemistry at Berlin University from 1835, Heinrich Rose (1795–1864, DSB), which Faraday put in his album of portraits. RI MS F1 H74.

Letter 3119

Faraday to Edwin Sidney¹

11 April 1856

From a photocopy in RI MS

Royal Institution | 11 Apr 1856

My dear Sidney,

I have just received your letter & also your lecture² & have read both & though as you know it is not my place to talk much of sacred matters I think

of them & thank you for the lecture. It would indeed well bear expanding into a book³[.]

I am glad you like Schoenbein's letter⁴[.]

And I am glad to hear of your continued exertions for the Idiot Asylum⁵[.] I found that Dr. Guggenbuhl⁶ is in town & has left me a couple of pamphlets⁷ but I have not yet seen him[.]

Ever Truly Yours | M. Faraday

Revd. E. Sidney | &c &c &c

1. Edwin Sidney (d.1872, age 74, B6). Rector of Little Cornard, 1847–1872, and lecturer at the Royal Institution and elsewhere. See also Fyfe (2004), 224–8.

2. Sidney (1856).

3. See Sidney [1867].

4. Letter 3054 part of which was published as Schoenbein (1856b).

5. That is Essex Hall, Colchester.

6. Johann Jakob Guggenbühl (1816–1863, DHBS). Swiss physician.

7. Probably including Guggenbühl (1853).

Letter 3120

Faraday and Sarah Faraday to Christian Friedrich Schoenbein

11 April 1856

From the original in UB MS NS 423

Royal Institution | 11 Apr 1856

My dear Schoenbein,

My dear wife purposes answering your kind letter herself; so I leave her pp. 1 & 2 and shall only say, most hearty thanks for your very pleasant, interesting, picture of juvenile life¹. I could have enjoyed it very much indeed. I suppose you were about the biggest child there[.]

The Volume² I sent, was by Mr. Twining, and I dare say it will make its appearance some day; for he was not going to Bale but perhaps near it. Now, however, I have committed another copy, & also the letter I wrote you about, to Mr. Roscoe, a student under Professor Bunsen at Heidelberg. He was to leave London this week for Heidelberg; & I trust you will soon have the volume;– which receive favourably for my sake[.]

Ever Yours | M. Faraday

Royal Institution | 11 Apr 1856

My dear Dr. Schoenbein,

The receipt of your letter was an unexpected pleasure & honour & I hasten to thank you for it & all the kind expressions it contains[.]

It would indeed be a great treat to me to visit Switzerland with my husband & to spend a little time quietly as you propose in the midst of your magnificent scenery, *if* I had the power of walking any distance, but that I have not, & I fear I should only be an incumbrance to my companions if I attempted it.

I am disabled by a Rheumatic affliction (I believe it is) but happily with little pain so that our home continues to be a cheerful & a happy one, as [sic] if you come to London we should be glad to shew you; Mr. Faraday too I am thankful to say is in better health than he was years ago when I think he suffered from too much study[.]

Pray present my best remembrances to Mrs. Schonbein & your daughters & Believe me dear Dr. Schonbein

Yours very sincerely | S. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Bâle | on the Rhine

1. Letter 3110.

2. Faraday (1855c).

Letter 3121

Faraday to James Gray¹

16 April 1856

From the original in SI D MS 554 A

[Royal Institution embossed letterhead] | 16 Apr 1856

My dear Sir,

I am very grateful for your kind remembrance of me and your liberal hospitality offered to me for the August meeting². I am unable to accept your kindness for if I go to Cheltenham (which is very uncertain) I am under engagement with a friend which would make our arrangement depend very greatly upon each other[.]

With sincerest thanks | I am My dear Sir | Yours Most truly | M. Faraday

Coll. J. Gray RA | &c &c &c

Endorsement: To | Colonel Gray | 10 Landsdown Terrace | Cheltenham

1. James Gray (d. 1857, age 75, GRO). Retired Royal Artillery officer.

2. Of the British Association.

Letter 3122**Faraday to Joseph Toynbee¹****17 April 1856****From the original in University College London MS add 122/1a***Private*

[Royal Institution embossed letterhead] | 17 Apr 1856

My dear Sir,

The subject of life is one that I do not profess to have any knowledge of or judgment upon and as an F.R.S. is supposed to be responsible in some degree for any paper he may present so I find myself unable to present the paper². Also if I mistake not the paper is of that hypothetical character as to render it unfit for the R.S.; so when I have any hypothetical matter to put forth as for instance on *the possible physical character of the lines of magnetic force*, I send it to the Philosophical Magazine³ not because I do not think it good matter but because I suppose it is not real enough for the Royal Society[.]

Some of the expressions at pp 12. 13 are rather loose[.] Electricity never passes freely along a wire however small the quantity sent there is always a resistance a proportionate resistance &c &c[.]

Ever My dear Sir | Very Truly Yours | M. Faraday
Joseph Toynbee Esqr | &c &c &c

1. Joseph Toynbee (1815–1866, ODNB). Ear surgeon.

2. This paper does not exist in the archives of the Royal Society and does not appear to have been submitted.

3. Faraday (1852d), ERE[29a].

Letter 3123**Peter Henry Berthon to Faraday****17 April 1856****From the original in GL MS 30108/2/79**

Trinity House London | 17 Apr 1856

Sir,

I am directed to forward to you the accompanying sample of white Lead from the delivery made by the Contractor at the Trinity Buoy Wharf, for the Service of the Corporation,— and to request that you will be pleased to analyze the same, and report the result thereof for the information of the Board¹.

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esqr. D.C.L. | &c &c &c

1. Faraday's notes of his analysis are on the back of this letter.

Letter 3124

Karl Wilhelm Knochenhauer to Faraday

17 April 1856

From the original in IET MS SC 2

Meiningen | le 17 Avril 1856

Monsieur,

En vous remerciant sincèrement de la lettre, dont Vous avez eu la bonté de m'honorer, j'ose Vous présenter une traduction du mémoire publié en 1854 dans les annales de Poggendorff¹. Ce n'est pas que j'espère Vous communiquer rien de nouveau, car mes expériences ne sont qu'une répétition de Vôtres, le seul point, auquel je Vous prie de prêter quelque attention, c'est l'observation, qu'une plaque d'un corps isolateur n'a qu'une faible influence sur l'induction, quand elle est séparée des deux plateaux condensateurs, tant de l'un que de l'autre, tandis que son influence s'augmente, quand elle touche le plateau, qui contient l'électricité induite, et encore plus, quand elle est en contact avec le plateau, qui reçoit l'électricité libre. (Voyez Votre Série XI-1271²). Quelle que soit la cause de ce résultat, soit que la formation des lignes de force, douées de polarisation, en pénétrant par divers milieux s'affaiblisse par une espèce de réflexion, comme nous le trouvons dans la lumière, soit que l'affaiblissement provienne de ce que les molécules de deux corps, qui touchent, ne sont pas en contact parfait, peut-être cette observation Vous donnera l'occasion de demander à Mr. Riess selon l'ancienne théorie une explication complète des observations, où les plaques isolatrices remplissent tout l'espace entre les plateaux condensateurs. Il me serait très important, que par Votre autorité Vous puissiez amener une telle explication, car comme elle ne me paraît pas possible sans supposer que l'électricité pénètre par le corps isolateur même et outre cela par les corps de diverse épaisseur jusqu'à une profondeur dépendante de l'épaisseur, on pourrait examiner très facilement cette hypothèse, en employant des *plateaux* condensateurs, d'où les corps isolateurs se laissent retirer commodément, aussi ceux, qui doivent s'être remplis d'électricité selon l'ancienne théorie. Une telle explication complète me serait outre cela très importante pour les expériences, que j'ai publiées depuis quelques années sur le courant de la batterie secondaire et surtout pour celles de cette espèce qui sont communiquées dans les rapports des séances de l'Académie de Vienne en novembre 1855³. Bien sûr que ces expériences ne peuvent être expliquées sans supposer un certain arrangement des molécules ou, comme je le nomme, une chaîne formée des molécules du fil, que parcourt le courant de la batterie primaire, d'ailleurs sans supposer que des lignes de force transfèrent cet arrangement sur le fil conducteur de la batterie secondaire, et que cette nouvelle chaîne cause la charge de cette batterie et par cela le courant qu'elle excite, il m'est impossible à présent de trouver en Allemagne des physiciens, qui veuillent se donner la peine de répéter ces expériences, parce qu'ils sont convaincus que des

expériences sont faites mal-adroitement, qui amènent les idées de lignes de force et surtout les idées que l'électricité n'est qu'un arrangement et qu'un mouvement particulier des molécules. Pour surcroît de malheur Mr. Reiss a publié en 1854 aussi quelques expériences de ce même genre, mais d'une manière à déranger parfaitement les résultats⁴; en conséquence il a assuré que ces observations sont tellement compliqués qu'elles ne méritent pas d'être continuées.

Excusez-moi, Monsieur, je Vous prie, d'avoir abusé si long-temps de Votre patience, et agréez l'assurance de la haute estime, avec laquelle j'ai l'honneur d'être

Monsieur | Votre très dévoué et très obéissant serviteur |
K.W. Knochenhauer

TRANSLATION

Meiningen (Duchy of Saxony) | 17 Apr 1856

Sir,

In thanking you for your letter, with which you have had the goodness to honour me, I dare to present you with a translation of a paper published in 1854 in Poggendorff's *Annalen*¹. It is not that I hope to communicate anything new, for my experiments are but a repetition of yours, the sole point to which I ask you to lend some attention, is the observation that a plate of an isolating body has but a weak influence on induction, when it is separated by two capacitor plates, both from one and from the other, whilst its influence increases when it touches the plate which contains the induced electricity, and even more, when it is in contact with the plate that receives the free electricity. (See your Series XI – 1271²). Whatever the cause of this result, be it that the formation of lines of force, capable of polarisation, weakens by penetrating various mediums like a kind of reflection, as we find with light, be it that the weakening comes about because the molecules of the two bodies, which are touching, are not in perfect contact, perhaps this observation will give you the opportunity to ask Mr. Reiss for a complete explanation of his results according to the old theory, where the isolating plates fill the whole space between the capacitor plates. It would be very important to me, that by your authority you could bring about such an explanation, for as it does not seem possible to me without supposing that electricity penetrates even the isolating body and besides that, bodies of diverse thicknesses up to a dependent depth of thickness, one could examine this hypothesis very easily by using capacitor plates, of which the isolating bodies can be removed completely, also those which must be filled with electricity according to the old theory. Such a complete explanation would be very important to me not just for the experiments which I published several years ago on the current of a secondary battery but above all for those of the kind which were communicated in reports of

the meetings of the Viennese Academy in November 1855³. Although these experiments could not be explained without assuming a certain arrangement of molecules or, as I call it, a chain formed by the molecules of a wire, which the current of the first battery runs through, furthermore without supposing that the lines of force transfer this arrangement onto the conducting wire of the second battery, and that this new chain causes the charge in this battery and by that the current which it excites, it is impossible for me at present to find in Germany any physicists who want to give themselves the trouble of repeating these experiments, because they are convinced that the experiments have been done clumsily, who bring along ideas of lines of force and above all ideas that electricity is nothing but an arrangement and a particular movement of molecules. To increase my misfortune, Mr. Reiss also published in 1854 some experiments of a similar nature, but in a manner to mix up the results completely⁴; as a consequence of which he has ensured that these observations were so complicated that they were not worthy of being continued.

Please excuse me, Sir, I beg you, to have abused your patience so long, and accept the assurance of the high esteem with which I have the honour of being,

Sir | Your very devoted and very obedient servant |
K.W. Knochenhauer

1. Knochenhauer (1854a). The translation, into French, is in IET MS SC 2.

2. Faraday (1838a), ERE11, 1271.

3. Knochenhauer (1855).

4. Riess (1854).

Letter 3125

Jean-Baptiste-André Dumas to Faraday

c.22 April 1856

From the original in IET MS SC 2

Mon cher et illustre ami,

Dès que M. Deville¹ m'a fait savoir à son retour que Vous n'auriez pas reçu les insignes de commandeur de l'ordre de la légion d'honneur Je me suis empressé de m'informer de la raison qui motivait cet étrange retard.

M. Varcollier² chargé de ce service m'avoit assuré d'abord que les insignes Vous avoient été envoyés. La lettre que je reçois de lui et que je m'empresse de Vous envoyer vous prouvera que rien ne vous a été envoyé en effet. Peut être y a-t-il un peu de négligence de la part de Votre ambassadeur³?

Quoiqu'il en soit, veuillez me faire parvenir une lettre adressée à M. Le grand Chancelier de la légion d'honneur⁴ pour réclamer de lui les insignes de commandeur que votre absence ne vous a pas permis de recevoir des mains de l'Empereur. Le reste me regardera et Je serai très heureux de Vous épargner les démarches qui resteraient à effectuer.

Ne m'en veuillez pas si je vous écris un peu pressé, mais je ne veux pas retarder ma lettre.

Mille Compliments bien affectueux | J. Dumas

TRANSLATION

My dear and illustrious friend,

When Mr. Deville¹ made me aware that you had not received the insignia of the Commander of the Legion of Honour, I quickly tried to find out for myself the reason for this extraordinary delay.

Mr. Varcollier² who is in charge of this department had at first assured me that the insignia had been sent to you. The letter, which I have just received from him and which I hastily send on to you will prove that nothing was in fact sent to you. Perhaps there was a little negligence on the part of your ambassador³?

Be that as it may, kindly send me a letter addressed to *The Grand Chancellor of the Legion of Honour*⁴ to claim from him the insignia of Commander which your absence did not permit you to receive from the hands of the Emperor. I will take care of the details and I shall be glad to spare you the additional steps which remain to be taken.

Please excuse this rather hurried note but I do not want to delay my letter.

A thousand most affectionate compliments | J. Dumas

Endorsed by Faraday: 24 April 1856

1. Henri Etienne Sainte-Claire Deville (1818–1881, DSB). Professor of Chemistry at Ecole Normale Supérieure, 1851–1880.

2. *Almanach Impérial*, 1855, p. 56 gives him as secrétaire des commandements to Joseph Charles Paul Bonaparte, Prince Napoleon (1822–1891, DBF), cousin of Napoleon III and responsible for the Universal Exhibition.

3. Henry Richard Charles Wellesley, Lord Cowley (1804–1884, ODNB). British ambassador to Paris, 1852–1867.

4. Duc de Plaisance.

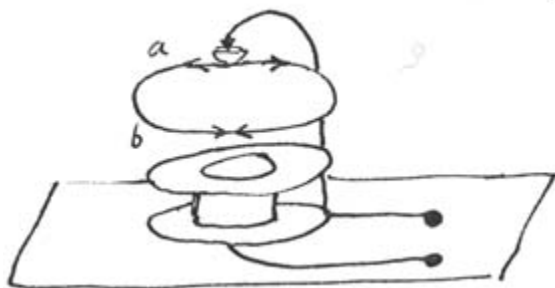
Letter 3126

Faraday to William Henry Adcock¹

25 April 1856

From a typescript in RI MS

R Institution | 25 Apr 1856



Sir,

The above is a copy of your drawing with the addition of two arrow heads shewing the course of the electric current in the upper part of *ab*. Now it is manifest that the current in the upper part *a* is contrary in direction to that in the lower part *b*, and therefore *a* will tend to move in one direction and *b* in the contrary direction. If *a* and *b* be far apart then a concentrated pole near *b* may move the whole in one direction uncertainly upon occasions, but the form of *ab* is unfitted to shew the Electro-magnetic motions – that mere helices can move wires carrying currents has been shown long ago by either Ampère² or Arago or both³.

Your Obedient Servant | (signed) M. Faraday

Address: Mr. W.N. [sic] Adcock, The Mills, Ashby de la Zouch, Leicestershire

1. William Henry Adcock (d.1904, age 73, GRO). Swedenborgian farmer.
2. André-Marie Ampère (1775–1836, DSB). French physicist.
3. See De La Rive (1853–8), 1: 223–32 for a discussion of Ampère's work.

Letter 3127**Robert Mallet to Faraday****26 April 1856****From the original in IET MS SC 2**

Delville, Glasnevin, Co Dublin | 26 Apr 56

My dear Sir,

After consideration – if you will still permit me, I should gladly avail myself of your very kind offer to ascertain, if Dr. Bence Jones, would permit his Rhumkorf's Coil to be used for a few days, in making some preliminary experiments as to its powers & suitability &c. for igniting powder in my Experiments upon the Earthquake wave period, at the Governments Premises Holyhead. – the accountability as to safety, return, and expense of transit resting of course with me. I shall have to obtain a coil for my own special use I therefore would limit my use of that referred to – to those preliminary trials[.]

Faithfully and | with much esteem | yours | Robert Mallet
 Professor Faraday | &c &c &c | Roy. Inst. Gr Brit | Albemarle St. | London

Letter 3128**Christian Friedrich Schoenbein to Faraday****26 April 1856****From the original in UB MS NS 424**

Bâle | 26 Apr 1856

My dear Faraday,

I have to acknowledge the receipt both of your letter and that of Mrs. Faraday's¹ and grateful as they have proved to me I could not help being very sorry for their contents, which have at once annihilated my hopes of seeing you here this summer.

I ardently wish and confidently hope that your excellent wife will before long be restored to her full health.

The third volume of your researches² reached Basle a few days ago and I am very much obliged for that repeated piece of kindness of Your's. I trust I shall live to see a fourth volume coming out containing detailed proofs of the Identity of Magnetism and Gravity. How I would rejoice if such a Glory should fall to your lot! You have however performed scientific exploits enough and if there has been any philosopher who legitimately merited to enjoy the "Otium cum dignitate"³, you are that privileged man.

This letter will be delivered to you by Professor Merian of Bâle a most intimate friend of mine and with whom I am quite sure, you will downright fall in love at the very first sight. To his eminent intellectual powers (he is a profound mathematician), he joins a heart full of the "Milk" of human Kindness such as I know no better one. If possible, make his personal acquaintance

and that of his wife too, who happens to be a pretty good english scholar and a member of the celebrated family “Bernoulli”.

I send you some papers of mine treating of mushrooms and ozoniferous organic substances⁴, subjects of which I wrote you some time ago⁵. If you cannot read them, give the trifles to some of your young chemical friends who may happen to learn a little bit of German by them if nothing else.

I returned last night from a beautiful walking trip made into the Jura Mountains and the valley of the Aar. Nothing could be finer than the landscaping I saw spring being out in its fullest bloom.

From the fact that I walked 24–30 Miles a day you may draw some inferences regarding the constitu<tion> of the legs of your old friend’s.

Pray, present my best compliments to Mrs. Faraday, thank her in my name for her kind letter and believe me my dear Faraday

Your’s | most faithfully | C.F. Schoenbein

Address: Doctor Michael Faraday | &c &c &c | Royal Institution | Albemarle Street | London

1. Letter 3120.
2. Faraday (1855c).
3. ‘ease with dignity’.
4. Including Schoenbein (1855b, 1856a).
5. Letter 3054.

Letter 3129

Faraday to Jean-Baptiste-André Dumas

28 April 1856

From the original in AS MS

Royal Institution | 28 Apr 1856

My dear friend,

You are indeed weighing me down with your kindness in taking so much trouble for me in the midst of your important avocations¹; and I can hardly think it right that the concerns of a worn out being like myself should occupy any part of the activity & intellect of another being who is exerting his powers for the improvement & benefit of his fellow creatures:– but as you desire me so I do, and hope that the pleasure I believe you feel in doing me a kindness will be in some degree your compensation for the trouble. I intended, when I should see you in Paris (for I do mean to be there if only for a couple of days) to tell you about the insignia; but as you direct me to write, so I write²:– Nevertheless, do what you think right in the matter, and, as I fear I am giving you far too much trouble, if I can save you any of that trouble when I come, let me do so_[.]

With sincerest respects to Madame Dumas and the kindest feelings to yourself I am My dear friend

As ever Yours | M. Faraday

Monsieur | Monsieur Dumas | Secretaire | &c &c &c &c

1. See letter 3125.

2. Letter 3130.

Letter 3130

Faraday to Duc de Plaisance

28 April 1856

From the original copy in IET MS SC 3

London | 28 Apr 1856

To His Excellency | The Grand Chancellor of the Legion of Honor | &c &c

Monseignor,

Though feeling quite unworthy of the high distinction done me by the Emperor when he deigned to confer upon me the degree of Commander of the Legion of Honor I am still unwilling to resign any part of that distinction. I was in the country because of ill health and therefore unable to be at Paris at the time when His Majesty distributed the marks of his pleasure; but being encouraged by my scientific friends¹ I venture to apply to Your Excellency for the Insignia of the degree and hope that the estimation in which I hold the honor may be in some measure an excuse for the liberty I am taking.

I have the honor to be | Your Excellency's most humble | & obedient Servant | F

1. See letter 3125.

Letter 3131

Henry Bence Jones to Faraday

29 April 1856

From the original in RI MS Conybeare Album, f. 7

29 Apr 1856

Dear Mr. Faraday,

I have not had a moment in which I could write to you about the coil.

I should like you to do with the coil what you would do if it were Your own¹[.]

I expect to go into the country tomorrow or I should call instead of writing to you[.]

Yours most sincerely | H. Bence Jones

1. See letter 3127.

Letter 3132**Thomas Henry Huxley to Faraday****29 April 1856¹****From the original in RI MS RI CG2/k/3**

My dear Mr. Faraday,

As you are aware I concluded my first Fullerian Course of Lectures yesterday. The experience which I have gained as to the wants of the audience and the possibilities of the subject, has led me to the conviction that there are some impediments in the way of the complete development of the usefulness of the Course which might be very easily removed by the Board of Managers – and I feel therefore, that I should be wanting in my duty as Professor, if I did not beg you to bring these impediments under the notice of the Board, in any way that you may judge most expedient –

“Physiology and Comparative Anatomy” are the subjects of the Course – in other words the whole range of the Natural History Sciences so far as Animals are concerned – How complex these branches of Science are – how particularly desirable it is that, in endeavouring to teach them, the lecturer should be able to appeal to the senses of his auditors – I need not remind you –

But the Royal Institution provides neither Diagrams nor objects of Natural History and the sole object in its whole “repertoire” of which I have been able to make any use during my past course has been the Skeleton of a Bird – not in the best condition[.] So far as diagrams are concerned the want may be supplied by one’s own exertions – and the requisite experimental illustrations of *Physiological* doctrines may be, in like manner, supplied by such rough bits of apparatus as I have been in the habit of extemporizing, but the doctrines of *Natural History* cannot be thought properly without specimens of natural objects – so arranged as to be accessible not only during & after the Lecture but at other times –

There must be something like a Museum before any real knowledge of Natural History can be gained – By “Museum” however, I by no means understand a great heterogeneous collection, a constant source of expence & trouble as most Museums are, but what is known as a “typical Museum” – a series of Typical forms that is, of each great division of Living Beings – The size & expence of such a Museum would depend entirely upon whether the types selected were those of Classes, of Orders, of Families or of Genera; but, large or small, such a Museum would always be complete and harmonious in itself – What I may call an “ordinal Museum” i.e. a collection of the typical Forms of all the *Orders* (of which there are at a rough guess about 100 or 120) would be sufficient for a course of the Length prescribed for the Fullerian Lectures. – It would go into a very small space – and might be so arranged as to be accessible to every member of the Institution so that every one who wished to gain an acquaintance with Natural History might acquire a real & practical knowledge of its doctrines by his own private study –

The increasing demand which the Natural History Sciences are every day making upon the attention of instructed & thoughtful men – and the high place which they must sooner or later take as branches of education – suggest the inquiry whether it would not be to the interest of the Royal Institution to do for them the same good offices which it has already performed for the physical & chemical Sciences? The establishment of such a Museum as that which I have just described would be a most important step in this direction; and should the Board of Managers see fit to adopt my suggestion, I can only say I shall be happy to undertake the superintend care of all details &c &c²

Ever | My dear Mr. Faraday | Very faithfully yours | T.H. Huxley
Jermyn S. | April 29th

1. Dated on the basis of the reference to Huxley's final lecture of twelve on 'Physiology and Comparative Anatomy', RI MS GB 2: 91 and that letter 3133 is the reply.

2. This letter was read at RI MM, 5 May 1856, 11: 146–7, but discussion was postponed until the following meeting. At that meeting, RI MM, 2 June 1856, 11: 149, Huxley's proposal was rejected as 'inexpedient'.

Letter 3133

Faraday to Thomas Henry Huxley

1 May 1856

From the original in IC MS HP16.8

[Royal Institution embossed letterhead] | 1 May 1856

My dear Huxley,

One great difficulty is removed by your kindness in undertaking the direction of the proposed illustrative collection¹:– but before I go to the managers I ought to know what would be the probable full cost involved in carrying out the plan, both with regard to that of the specimens & that of providing for their reception:– otherwise I doubt whether the managers would be able to form any notion of its feasibility or fitness. Can you help me in this matter?

Ever Yours | M. Faraday

1. Offered in letter 3132; see also note 2.

Letter 3134

Faraday to Justus Liebig

1 May 1856

From the original in UU EW

Royal Institution | 1 May 1856

My dear Liebig,

I have long been desirous of writing to you, but laziness or weariness (and I think more of the latter than the former) have until now hindered me.

I want to thank you for your great kindness shewn to my nephew Mr. Frank Barnard¹. I assure you he is very sensible of it, and expresses himself in strong terms to us, upon this matter. He seems to find himself far more in a home atmosphere than he did at Paris. His sister Jane, whom you may remember meeting here and who is of a warm affectionate temperature [sic], feels very strongly your kindness to her brother. I have to thank you, too, for your kindness in sending me a photograph of Mr. Barlow². Your Munich photographs delight me very much; for I am not fond of those which I see about in London, coloured up so highly that all simplicity is taken away from them. I suppose the owners think them fine & elaborate, I think them common place & often vulgar³.

As to science, I have little to say in my own name I could say much in Yours;— but *that* perhaps you would wish me to be silent about. Nevertheless, my deep admiration of your character & your services to the good cause will steal out; so permit these few words – When I set too [sic], to think, I become headachy & giddy and think to no purpose; so am content to wait for what may be suggested or occur. I shall have a common place paper or two to send you at some opportunity⁴, but they are only evidences of the will to work & think, not of the power[.]

Ever My dear Liebig | Yours Most truly | M. Faraday

Address: The | Baron Liebig | &c &c &c &c

1. See Faraday to Liebig, 17 July 1855, letter 3006, volume 4.

2. This is reproduced in volume 3, p. 729.

3. On this see Prescott (1985), 25–7.

4. Faraday (1856c), ERE30.

Letter 3135

Thomas Henry Huxley to Faraday

2 May 1856

From the original in RI MS RI CG2/k/4

Jermyn St. | 2 May 1856

My dear Mr. Faraday,

It is very difficult for me to estimate with any degree of exactness the possible cost of such a Collection as that which I propose¹. I think, however I am speaking well within bounds in taking £100. 0. 0 as the *extreme outside* expenditure which would be necessary[.]

As I purpose to select a purely physiological subject for my next Years course – reserving pure Natural History as a “*bonne bouche*” for the last – one

can take one's time about the collection and it would perhaps be the best way to vote 50. 0. 0 for this years expenditure & see how far it will go –

Ever yours very truly | T.H. Huxley

M. Faraday Esq DCL

1. See letter 3133 and also 3132.

Letter 3136

Thomas Henry Farrer to Faraday

8 May 1856

From the original press copy in TNA MT4/22, p. 2423

8 May 1856

4634

inform you that they have two Lighthouses now in course of erection in the Colonies and it appears to my Lords very desirable to have on the spot some means of distilling or procuring from the Water of the Sea pure Water for drinking and domestic purposes. My Lords desire me to request therefore to know whether you can inform them which of the Inventions for effecting this object is in your opinion the best and whether you are able to give them any information upon the Capabilities of the Marine Arated Fresh Water Apparatus patented by Dr. A. Normandy¹ and referred to in the inclosed Copy of his letter to this Department.

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
M. Faraday Esqre. D.C.L | &c &c &c | 21 Albemarle Street

1. Normandy was granted patent 1856-1252 for his apparatus on 26 May 1856.

Letter 3137

Carlo Matteucci to Faraday

8 May 1856

From the original in BPRE MS Regg E 224/24, doc. 2

Pise | 8 Mai 1856

Mon cher Faraday,

Je vous demande bien pardon d'être obligé de vous écrire si tôt une autre lettre. Ne voyant pas de réponse de M Sabine a propos de la faveur que j'avais demandé d'insérer dans le Phil. Trans. mon dernier travail sur les Phenomènes physiques et chimiques de la contraction musculaire¹ faisant suite aux recherches d'Electro-Physiologie, Je vous prie de vous interesser a cela. Je sais qu'en Allemagne on travaille sur des matières analogues et je voudrais presser la publication de mon memoire. Si le Conseil de la Societe Royale a des raisons qui l'empêchent d'accéder a ma demande je publierai

cela dans les Annales². En tout cas, et si cela n'augmente pas les difficultés vous pourrez donner au Phil. Mag. la lettre que je vous ai écrit dernièrement³.
 Tout a vous | Ch. Matteucci

TRANSLATION

Pisa | 8 May 1856

My dear Faraday,

I sincerely beg your pardon for being obliged to write another letter to you so soon. Seeing no response from Mr. Sabine regarding the favour that I asked of inserting in the *Philosophical Transactions* my last work on the physical and chemical phenomena of muscular contractions¹ which follow on from my electro-physiological research, I ask you to take an interest in this. I know that work of a similar nature is being conducted in Germany and I would like to speed up the publication of my paper. If the Council of the Royal Society has reasons which prevent it from acceding to my request, I will publish it in the *Annals*². In any case, and if this does not increase the difficulties, you can give to the *Philosophical Magazine* the letter which I wrote to you recently³.

All yours | Ch. Matteucci

Address: Professor Faraday F.R.S. | &c &c &c | Royal Institution | Albemarle St. | London

1. Matteucci (1857).

2. Matteucci (1856b).

3. Matteucci (1856a).

Letter 3138

Faraday to Henry Bence Jones

9 May 1856

From the original in ULC Add MS 8546/I/82

[Royal Institution embossed letterhead], 5 Albion Villas,
 Folk[e]stone | 9 May 1856

My dear friend,

I surely have refrained long enough from thanking you for the pleasure & comfort we experience in this house¹; and if I were to hesitate longer I should begin to think myself ungrateful for your kindness in sending us here. We are as comfortable as we can desire to be, & whilst the windy, rainy, dull weather has been going on, have enjoyed the tall rooms & warm walls and double glass windows of the house. My wife has not been out of the house once, but the others pass in and out as the weather permits. We enjoy ourselves in every point, the more because we know that the more agreeable & useful the place proves to us, the more will it be according to your wishes.

As for myself, I am idling thoroughly, and feel fit for nothing else. The head weariness raises, but I am satisfied that indolent patience is the best thing for it. It seems to me strange to be out here on a Friday Evening and I long to be in my duty – I am sorry not to help Mr. Bradbury², & wonder how Mr. Darker³ will get on. I trust all things will come right.

Lady Millicent's kindness & thought for us has been very great. I hope you will offer the sincerest thanks both of my wife & myself

Ever Yours Most truly | M. Faraday

Dr. B. Jones | &c &c &c

1. Bence Jones had lent the Faradays this house. See letter 3142.

2. Henry Riley Bradbury (1831–1860, ODNB). Writer on printing. Bradbury (1856), Friday Evening Discourse of 9 May 1856.

3. William Hill Darker (d.1864, age 53, GRO). Scientific instrument maker of 9 Paradise Street, Lambeth. Clifton (1995), 76.

Letter 3139

John Tyndall to Faraday

9 May 1856

From the typescript in RI MS JT TS Volume 12, pp. 4047–8

9 May 1856

My dear Mr. Faraday,

The following note from the War Department reached me last night, it contains my doom.

War Department Pall Mall | 8 May 1856

Sir,

I am directed by the secretary of State for War¹ to acknowledge the receipt of your letter of the 1st instant, and to inform you that his Lordship considers the explanation you have furnished to be quite satisfactory².

I have the honour &c | H.R Drewry³

J. Tyndall Esqr

I opened this letter with the neutrality of a man who has really made up his mind for the worst, but I confess it gives me great pleasure to think that the matter has thus terminated.

I hope you are improving, and that you will soon return quite restored. This weather is awfully trying – I was at a very low ebb in my lecture of yesterday, but barring the failure of a difficult experiment or two, managed to drag through pretty well. Frankland is in town for a few days and Despretz – who desires me to present his respects to you – is coming. We purposed starting on Tuesday evening⁴, but he has not yet appeared.

I hope Mrs. Faraday is well. I should think that Folk[e]stone cliff [blank in TS] very cold for her at present.

Last Wednesday evening I was at Lord Ashburton's⁵ and met Wheatstone there. We had a conversation about the stereoscope and I was pained to see the desire manifested so strongly to deprive Brewster of what he really merits – or to reduce his merits to zero⁶. Wheatstone says he must give me a lesson on the subject; that is, he must instruct my muddy understanding – the matter appears to me as plain as a proposition of Euclid⁷: but it does not seem as if he had really mastered any theory but his own.

Good bye | Ever Yours most truly | John Tyndall

1. Fox Maule, second Baron Panmure (1801–1874, ODNB). Secretary of State for War, 1855–1858.
2. This relates to a letter Tyndall had written to *The Times* about some examining for commissions in the Royal Artillery and Royal Engineers that he had undertaken. Faraday had advised Tyndall on the best course of action, Tyndall, *Diary*, 1 May 1856, 6a: 312. See Tyndall (1868), 267–8 and Eve and Creasey (1945), 62–3.
3. Henry Runciman Drewry (d.1886, age 84, GRO). Chief clerk of the War Office. *Royal Kalendar*, 1860, p. 279.
4. That is 6 May 1856.
5. William Bingham Baring, 2nd Baron Ashburton (1799–1864, ODNB). Conservative politician.
6. For the controversy over whether Wheatstone or Brewster had invented the stereoscope see Morrison-Low (1984), 62. The controversy was ignited by the publication of Brewster (1856).
7. Euclid (fl. 295 BCE, DSB). Greek mathematician.

Letter 3140

Faraday to John Tyndall

10 May 1856

From the typescript in RI MS JT TS Volume 12, p. 4140

Royal Institution | 10 May 1856

My dear Tyndall,

I am very glad of your letter¹ I was hoping to hear of the end I could not doubt that it would be satisfactory but still wished to *know* that it was so as it is I think you stand in a good independent position and have been very useful in the great matter, whether it progresses now or has to wait a little. That it must progress is I think certain.

I am better: in fact all the matter is that years do their work and why should they not if they have done it in one way i.e. for my comfort and happiness they ought also to do it in another as for instance in wear and tear. Who would wish for an ever enduring life in this world, above all if a better hope appears before him?

I hope we shall be home on Thursday night² and to find you right. I have heard well – *very well* of your last lecture_[.]

Ever Truly Yours | M. Faraday

Dr. Tyndall

1. Letter 3139.
2. That is 15 May 1856.

Letter 3141**Faraday to John Barlow****10 May 1856****From the original in RI MS F1 D29**[Royal Institution embossed letterhead],
Folk[e]stone | 10 May 1856

My dear Barlow,

Just a few words by way of remembrance: for when I am out in this way the thoughts of home & all its matters press upon me & I cannot altogether rest in respect of it if I try ever so hard. But the time goes on & though giddiness hangs about me I trust to return next Thursday¹ & hear that all is well & has been well[.] I was very sorry to lose Mr. Bradburys² evening³ but hope Mr. Darker⁴ did not fail him. Kindest remembrances from all here to Mrs. Barlow. I hope you are both in good health[.] I had a faint surmise that Mrs. Barlow had not been well but I so often make mistakes & associate wrong persons & events together that I am afraid to say much about it.

Ever My dear Barlow | Very Truly Yours | M. Faraday

1. That is 15 May 1856.

2. Henry Riley Bradbury (1831–1860, ODNB). Writer on printing.

3. Bradbury (1856), Friday Evening Discourse of 9 May 1856.

4. William Hill Darker (d.1864, age 53, GRO). Scientific instrument maker of 9 Paradise Street, Lambeth. Clifton (1995), 76.

Letter 3142**Sarah Faraday and Faraday to Harriet Jane Moore¹****10 May 1856****From the original in LU**

5 Albion Villas, Folk[e]stone | 10 May 1856

My dear Miss Moore,

I think you will like a line or two to let you know how we are going on in our unexpected visit to Folkestone – it was not a cold but a long continuous headache which seemed enervating with giddiness which made Dr. Bence Jones send us away & which I knew by experience was the only thing to do my dear husband good; the weather so far has not been propitious – but we are away from excitement & endeavouring to be idly busy[.] Mr. Faraday reading a little, working cross stitch a little, & walking when the weather will allow – (I have not put my head out yet) & we are all enjoying Dr. B. Jones' comfortable roomy house, it is a great thing when shut in by weather in the country, to have room to move about & pleasant companions – we have our two nieces (Miss Reid & Jane) & do not quarrel much & we hope in a few days to return all the better[.]

Do not trouble yourself for a moment about the Davenport a piece came off before, Mr. Huddleston² will set to it rights, in coming home it may have got shaken & it is of not the least consequence[.]

We are sorry to hear still such a sad account of your Brother³ for perhaps all this cold wind may have some influence[.]

With our kindest remembrances to your family circle

Believe me My dear Miss Moore | Yours very sincerely | S. Faraday

Many thanks my dear Miss Moore for your kind note[.] The ready sympathy of my friends often represses me with my own want of feeling. We are getting on better to day is better – it is bright even but my dear wife has not been out of the house since we came into it[.] It is very comfortable[.] With kindest remembrances to all with you

I am Yours Very Truly | M. Faraday

10 May 1856

1. Harriet Jane Moore (1801–1884, James (2001)). Painter and member of the Royal Institution, 1852–1881.

2. Thomas Huddleston (d.1881, age 56, GRO). Furniture maker who undertook work for the Royal Institution.

3. John Carrick Moore (1805–1898, *Proc. Roy. Soc.*, 1898, **63**: xxix–xxxii). Secretary of the Geological Society, 1855 to 1856.

Letter 3143

Thomas Henry Farrer to Faraday

13 May 1856

From the original press copy in TNA MT4/22, p. 2512

13 May 1856

5020

... transmit to you the accompanying Copy of a Letter from Dr. A. Normandy and to acquaint you that my Lords have requested that Gentleman to forward to you the Bottle of the Water distilled by his Marine Aerated Fresh Water Apparatus¹. In order to enable you the better to report upon his apparatus, Dr. Normandy has also been told to forward a Certificate from the War Department to the effect that the Bottle of Water was received there and sealed.

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
Professor Faraday F.R.S. | 21 Albemarle Street

1. See letter 3136.

Letter 3144

Arthur-Auguste De La Rive to Faraday

14 May 1856

From the original in IET MS SC 2

Genève | le 14 Mai 1856

Monsieur & très cher & excellent ami,

Je n'étais pas à Genève quand votre lettre y est arrivée¹; elle m'a été expédiée à Paris où je me trouvais alors en séjour avec ma famille ayant été passer quelque temps dans cette ville auprès de mon fils second² qu'y achève ses études. Je viens vous remercier de votre bon & aimable souvenir. Si vous saviez combien je tiens à tout ce qui vient de vous & combien je suis touché de votre excellente amitié pour moi. C'est de toutes les distinctions ou plutôt de tous les services que m'a rendus la Science celui de beaucoup que j'apprécie le plus. Car qu'est-ce que le reste à côté de ce qui concerne le coeur? *Vanité des vanités*³, le plus souvent.

J'ai appris avec chagrin que vous aviez été un peu souffrant & que vous aviez été au bord de la mer vous faire un peu de bien. J'espère que ce remède vous aura réussi; mais il vous faut du repos; il faut que cette tête qui travaille toujours, sache s'habituer à rester quelquefois un peu oisive. Il n'y a que le coeur auquel il soit permis de ne jamais se reposer; mais chez vous tout est actif la tête & le coeur. Je sais bien que ce n'est pas sur cette terre que nous pouvons & que nous devons espérer trouver le repos; le vrai repos, le le but réel et salutaire, nous ne pourrions le trouver que dans le Sien de Dieu; mais il ne nous est pas interdit cependant d'en chercher un peu l'avant goût dès ici bas.

J'ai profité de mon séjour à Paris pour voir bien des choses intéressantes & en particulier les nouvelles expériences de Foucault⁴, & celles de Mr. Bernard⁵ le physiologiste, qui sont très curieuses. – J'ai moi-même entrepris quelques nouvelles recherches sur les phénomènes d'induction qui ont lieu avec de très grandes vitesses, & j'ai trouvé pour les faire, de grandes facilités dans le Conservatoire des Arts & Métiers où l'on a mis avec beaucoup d'obligeance à ma disposition les instruments & les aides dont j'avais besoin. Je ne tarderai pas à publier le résultat de ces recherches que mon fils qui m'a aidé à les faire, continue sans moi depuis que j'ai quitté Paris.

Vous avez peut-être vu dans le dernier no. de la *Bibl. Univ.* que M Soret⁶ & moi nous avons réussi à montrer que l'eau est décomposée même dans la propagation de l'électricité qui résulte de l'induction statique⁷. C'est en nous servant d'eau [word illegible] a [word illegible] d'une bouteille de Leyde que nous avons obtenu ce résultat.



Prof. de la Rive

Plate 5. Arthur-Auguste De La Rive. From RI MS F1 I165.

Mon frère⁸ qui est à Londres dans ce moment serait bien heureux s'il pouvait avoir le plaisir de vous voir avant son départ; j'espère qu'il me rapportera de bonnes nouvelles de vous.— Ma femme me charge expressément de la rapeller à votre bon souvenir & à celui de Madame Faraday. Veuillez aussi présenter mes respects à Madame Faraday.

Votre tout dévoué & affectionné ami | A. de la Rive

TRANSLATION

Geneva | 14 May 1856

Sir and very dear and excellent friend,

I was not in Geneva when your letter arrived there¹; it was forwarded to Paris where I was staying with my family, having gone there to spend some time with my second son² who is finishing his studies there. I would like to thank you for your good and kind remembrances. If you only knew how much I value everything that comes from you and how much I am touched by your excellent friendship for me. It is of all distinctions or rather of all favours that Science has endowed on me the one that I appreciate the most. For what is everything else beside that which concerns the heart? *Vanity of vanities*³, most often.

I learned with sorrow that you had been somewhat unwell and that you had gone to the seaside to recuperate a little. I hope that this remedy will have succeeded; but you need rest; that head, which is always at work, must learn to be a little more idle occasionally. It is only the heart which is allowed never to rest; but with you, everything is active, the head and the heart. I know well that it is not on this earth that we can and that we may hope to find rest; true rest, and the real and salutary goal, can only be found in the bosom of God; but it is not forbidden in the meantime to seek a little foretaste here below.

I have taken advantage of my stay in Paris to see a great many interesting things and in particular, the new experiments of Foucault⁴ and of Mr. Bernard⁵, the physiologist, which are very curious. I have myself undertaken some new research on the phenomena of induction which takes place at great speeds and I have found excellent facilities to carry this out at the Conservatoire des Arts and Métiers where they have most kindly placed at my disposal the instruments and assistance which I needed. I shall not be slow in publishing the result of this research which my son, who helped me, has continued without me since I left Paris.

You saw perhaps in the last edition of the *Bibliothèque Universelle* that Mr. Soret⁶ and I succeeded in showing that water is decomposed even in the

propagation of electricity that results from static induction⁷. It is by using water [words illegible] of a Leyden jar that we obtained this result.

My brother⁸ who is currently in London would be extremely happy if he could have the pleasure of seeing you before his departure; I hope that he will bring me good news of you. – My wife has asked expressly to be remembered to your good self and to Mrs. Faraday. Please kindly pay also my respects to Mrs. Faraday.

Your all devoted and affectionate friend | A. de la Rive

1. Letter 3106.
2. Charles-Lucien De La Rive (1834–1924, Choisy (1947), 51). Swiss physician and writer.
3. Ecclesiastes 1: 2.
4. Jean Bernard Léon Foucault (1819–1868, DSB). French physicist. See Foucault (1856a, b).
5. Claude Bernard (1813–1878, DSB). French physiologist. See Bernard (1856).
6. Jacques Louis Soret (1827–1890, P2, 3). Swiss physicist.
7. Soret (1856).
8. Eugène De La Rive (1804–1872, Choisy (1947), 52). Swiss lawyer and politician.

Letter 3145

Karl Wilhelm Knochenhauer to Faraday

15 May 1856

From the original in IET MS SC 2

Meiningen | le 15 Mai 1856

Monsieur,

Vous trouverez sûrement très importun que je vous incommode encore une fois, surtout où Vous êtes hors d'état d'examiner Vous-même les faits, ce que je regrette beaucoup. Cependant Votre lettre m'ayant convaincu, que l'induction doit être étudiée encore plus profondément pour dérober tout appui à l'ancienne théorie, je me suis aussitôt résolu à entreprendre quelques nouvelles recherches. Et voilà, comme je le crois, que Vos lignes de polarisation se laissent démontrer tout clairement par les expériences; j'espère donc pouvoir me promettre Votre indulgence, si j'ose Vous communiquer mes observations. – Du condensateur, dont les plateaux ont environ deux pouces de diamètre, je joignis le plateau inférieur au sol, et je communiquai au plateau supérieur de l'électricité positive, en le touchant avec la boule d'une bouteille de Leyden chargée plus ou moins fortement. L'intervalle des plateaux était rempli tantôt d'une ou de plusieurs plaques de verre (c'étaient les plaques BCD d'auparavant), tantôt les plaques étaient séparées l'une de l'autre par une couche d'air (quelques petits morceaux de verre interposés aux

bords servaient à ce but), tantôt la plaque ne touchait que l'un des plateaux du condensateur, tantôt elle était éloignée de l'un et de l'autre. Toutes les fois après avoir retiré le plateau supérieur je trouvais la face supérieure de chaque plaque (c'est à dire celle qui avait été dirigée vers le plateau supérieur) chargée d'électricité positive et la face inférieure chargée d'électricité négative. Pour examiner cette charge j'avais muni l'électromètre à feuilles d'or d'un plateau, et après y avoir mis l'une des faces je couvris l'autre d'un autre plateau, afin d'en affaiblir l'influence, tout à fait de la manière, que Vous avez indiquée.— En voulant étendre un peu plus ces expériences, je me procurai encore 8 tables carrées de verre de vitre munies d'une mince couche de laque, de même 4 tables de soufre et une faible plaque de laque. Le tout exécuté comme auparavant, je fus étonné de trouver presque toujours sur les deux faces la même électricité, tantôt positive, tantôt négative, je répétai les observations avec les anciennes plaques et toujours les deux électricités sur les deux faces. Pour ôter l'électricité aux plaques, je plaçai les tables de vitre dans un fourneau, où elles s'échauffèrent un peu; mais ce moyen n'étant pas applicable à la plaque de laque, je fis usage d'une flamme d'alcool, cependant sans atteindre mon but; même deux flammes ensemble des deux côtés de la plaque ne détruisirent point l'électricité. Aussi les tables de vitre ne voulaient pas perdre leur électricité, quand elles étaient exposées pendant quelques temps aux deux flammes. En examinant souvent les tables je trouvai qu'après l'usage de la flamme les deux faces étaient chargées d'électricité contraire, l'une d'électricité positive l'autre d'électricité négative. Dès ce moment tout était éclairci. Les plaques, qui se sont trouvées entre les plateaux du condensateur, sont chargées de deux espèces d'électricité, l'une recouvre légèrement les surfaces et peut être détruite tout à fait promptement quand on approche la plaque d'une flamme, l'autre a sa source dans la polarisation, n'est détruite qu'avec beaucoup de peine, et met la face supérieure en état positif, l'inférieure en état négatif. Les anciennes plaques, dont les surfaces étaient déjà un peu altérées par l'usage, perdirent l'électricité fugitive de la surface pendant le temps, qu'on les retirait du condensateur; aussi l'état de polarisation disparut après un quart d'heure environ, tandis que les nouvelles tables le conservèrent très long-temps, surtout les tables de soufre et de laque, où il resta même pendant quelques jours.— Maintenant la manière d'expérimenter fut la suivante. Après avoir mis les tables sur le plateau inférieur du condensateur et les avoir couvertes du plateau supérieur, celui-ci était chargé plus ou moins fortement selon le nombre des plaques et aussitôt enlevé; alors j'examinais chaque plaque l'une après l'autre sur l'électricité libre en l'approchant de l'électromètre et sans faire usage du plateau affaiblissant, ensuite je promenais quelques moments chaque plaque devant la flamme, et enfin j'en examinai l'état électrique des deux faces, qui avait sa source dans

la polarisation. Voilà quelques séries. Les nombres donnent les plaques de haut en bas et le mot: régulièrement signifie que la face supérieure était en état positif, l'inférieure en état négatif.

Les tables de l'air	avant l'exp. à la flamme	après l'exp. à la flamme	Les tables de l'air (dans l'air)	avant l'exp. à la flamme	après l'exp. à la flamme	Les tables de l'air	avant l'exp. à la flamme	après l'exp. à la flamme
1	pos.	régul.	1	pos.	régul.	1	pos.	régul.
2	neg.	"	2	neg.	"	2	neg.	"
3	pos.	"	3	neg.	"	3	pos. (pos.)	"
4	neg.	"	4	pos.	"	4	pos.	"
						5	neg. (pos.)	"
						6	neg.	"
						7	pos.	"
						8	pos.	"

Les tables de l'air	avant l'exp. à la flamme	après l'exp. à la flamme	Les tables de l'air à l'exp.	avant l'exp. à la flamme	après l'exp. à la flamme
D	pos.	régul.	1	pos. (pos.)	"
1	pos.	"	2	pos. (pos.)	"
2	neg.	"	3	neg. (pos.)	"
3	pos.	"	4	neg. (pos.)	"
4	neg.	"			
5	"	"			

Les tables de l'air	avant l'exp. à la flamme	après l'exp. à la flamme	Les tables de l'air à l'exp.	avant l'exp. à la flamme	après l'exp. à la flamme
D	pos.	régul.	1	pos. (pos.)	"
1	pos.	"	2	pos. (pos.)	"
2	neg.	"	3	neg. (pos.)	"
3	pos.	"	4	neg. (pos.)	"
4	neg.	"			
5	pos.	"			
6	neg.	"			

Les tables de l'air sont en état de polarisation au moment de l'exp.

Quelle que soit la cause de l'électricité libre, qui recouvre les tables, soit qu'elle vienne de l'air, qui se trouve entre les surfaces et qui reçoit aussi la polarisation, soit qu'elle ait une autre source, sûrement elle n'exerce aucune influence appréciable sur la charge du condensateur, et la distribution, qui paraît être modifiée par le degré de la charge, et de telle manière, que l'ancienne théorie n'en tirera aucun secours; aussi est-elle si faible que la flamme l'enlève aussitôt. La polarisation des tables, qui est retenue avec une si grande force, s'étend sans doute par toute la masse des tables, d'ailleurs elle serait aussi détruite par la flamme, et chacune des tables ne pourrait la montrer. Pour moi, je ne peux douter, que ces observations ne prouvent ce que Vous avez nommé les lignes de polarisation, et si Vous n'avez pas encore perdu la patience de répondre j'écouterai avec grand plaisir ce que Vous en jugez.

Agréez, Monsieur, l'assurance de la haute estime, avec laquelle j'ai l'honneur d'être | Votre très dévoué et très obéissant serviteur | Knochenhauer

TRANSLATION

Meiningen | 15 May 1856

Sir,

You will surely find it most inconvenient that I trouble you once again, especially as you are in no state to examine the facts for yourself, which I greatly regret. However, persuaded by your letter, that induction should be studied in more detail to take away all support from the old theory, I immediately resolved to undertake some new research. And there you are, I believe: your lines of polarization clearly demonstrated by the experiments; I hope therefore that I can rely on your indulgence, as I dare to communicate to you my observations. – In a capacitor, of which the plates are about two inches in diameter, I joined the lower plate to the ground, and I charged the top plate with some positive electricity, by touching it with a more or less charged ball of a Leyden jar. The space between the plates was filled sometimes with one and sometimes with several sheets of glass (plates *BCD* from before), sometimes the sheets were separated one from another by a layer of air (a few small bits of glass placed at the sides used to achieve this), sometimes a sheet [was placed so that it] touched one of the plates of the capacitor, sometimes it was distant from one and from the other. Each time I removed the top plate, I found the superior surface of every plate (that is to say the one that had been directed toward the top plate) was charged with positive electricity and the inferior surface charged negative electricity. To examine this charge, I attached an electrometer with old leaf to one of the plates, and after having put it on one of the faces, I covered the other with another plate, in order to weaken its influence, just as you had indicated. – Wanting to extend these experiments a little more, I acquired 8 more square pieces of window glass covered with a thin layer of lacquer, and in the same way covered four pieces with sulphur and a weak covering of lacquer. Everything was carried out as before; I was astonished to find almost always on the two faces the same electricity, sometimes positive, sometimes negative; I repeated the observations with the old plates and [found] always the two electricities on the two faces. To remove the electricity from the plates, I placed the glass pieces an oven, where they warmed up a little; but this method not being appropriate for the lacquered plates, I used an alcohol flame, however without success; even two flames together on two sides of the plate did not wipe out the electricity. Neither did the glass pieces want to lose their electricity when exposed to the two flames, even for some time. By examining the pieces often I found that after the use of the flame the two faces were charged with opposite electricity, one with positive electricity the other of negative electricity. From that moment all was clear. The sheets, that were between the plates of the capacitor, are charged with two types of electricity, one lightly covers the surfaces and can be destroyed quite easily by approaching the plate with a flame, the other has its source in polarization and is destroyed only with a lot of trouble, and gives the superior face in a positive charge, the inferior in a negative charge. The old

plates, the surfaces of which were already a little altered through usage, lost the fleeting electricity from the surface during the time they were removed from the capacitor; also the state of polarization disappeared after about a quarter of hour, while the new pieces preserved it much longer, especially the sulphured and lacquered pieces, where it even remained for some days. – Now the manner to experiment was the following. Having put the pieces on the lower plate of the capacitor and having covered them with the top plate, the latter was charged more or less strongly according to the number of the sheets and then removed immediately; then I examined every plate one after the other for free electricity by approaching it with an electrometer and without making use of the weakening plate, then I passed every plate over a flame for some time, and finally I examined the electric state of the two faces, which had its source in polarisation. Here are some series. The numbers give the plates from top to bottom and the word ‘regularly’ means that the superior face was in a positive state, the inferior in a negative state.

Whatever the cause of the free electricity, which covers the pieces, whether it comes from the air which is between the surfaces and which is also polarised, or it has another source; surely it exercises no substantial influence on the charge of the capacitor; and the distribution, which appears to be modified by the degree of the charge, is of such a manner, that the old theory cannot draw any support from it, as it is so weak that a flame removes it immediately. The polarization of the pieces, which is retained with such great force, extends to the whole mass of pieces, besides it would also be destroyed by the flame, and would not be shown by all the pieces. I cannot myself doubt that these observations prove what you have called the lines of polarization, and if you have not yet lost the patience to reply, I would listen with great pleasure to your opinion on this.

Accept, Sir, the assurance of the high esteem, with which I have honour of being,

Your very devoted and very obedient servant | Knochenhauer

Address: To Michael Faraday, Esq. | London | Royal Institution

Letter 3146

Thomas Henry Farrer to Faraday

22 May 1856

From the original press copy in TNA MT4/22, p. 2690

22 May 1856

5269

Referring to the recent communication¹ from this Department on the subject of Dr. Normandy's invention for procuring fresh water from that of the Sea², I am directed by the Lords of the Committee of Privy Council for Trade to transmit to you the accompanying Bottle of purified water which my Lords have received from H.M. Secretary of State for War³, being

a sample of the Water distilled by the Apparatus in question now in use at Heligoland.

I am to acquaint you that any opinions which you may give to the Department will at all times be considered as intended only for the guidance of the office. The object of asking your opinion in this instance is to ascertain whether the Apparatus in question is likely to be serviceable for lighthouses at Sea.

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
Professor Faraday FRS | 21 Albemarle Street

1. Letter 3143.

2. See letter 3136.

3. Fox Maule, second Baron Panmure (1801–1874, ODNB). Secretary of State for War, 1855–1858.

Letter 3147

Faraday to Charles Brooke

28 May 1856

From the original in BL add MS 42240, f. 26

Royal Institution | 28 May 1856

My dear Sir,

I send you a portion of the fluid containing gold particles of which I spoke to you last night – I hope it will arrive in good state¹. Small circumstances cause the aggregation of the particles into floculi & then they settle much more quickly than before such change_[.] Unless glass be exceedingly clean the matter on its surface tend to cause the change.

Ever Truly yours | M. Faraday
Chas. Brook[e] Esqr | &c &c &c

1. Faraday, *Diary*, 28 May 1856, 7: 14763 which noted that the gold solution was sent to Brooke for microscopic examination.

Letter 3148

Faraday to James David Forbes

29 May 1856

From the original in SAU MS JDF 1856/53

[Royal Institution embossed letterhead] | 29 May 1856

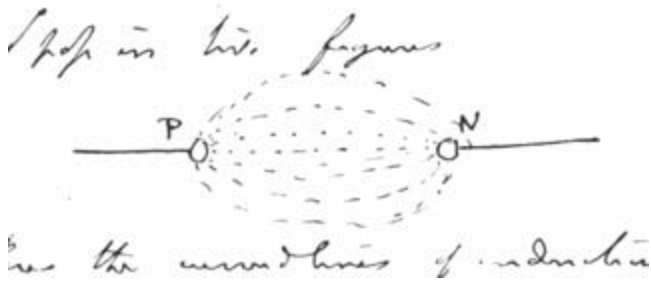
My dear Sir,

I received your letter and proofs¹ yesterday: I return the latter by the post. I thank you very much for a sight of them_[.] I will not say I thank you for the expression of your good opinion of my contribution to science² because I believe you would have performed what you considered a duty independant of any other consideration but I may without offence say how very grateful it

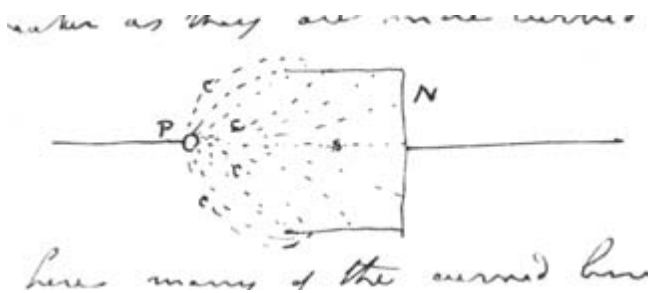
is to me to find that you hold such opinions. I am deeply indebted to scientific men for these very kind feelings towards me[.]

You will see that I have marked nothing (indeed how could I) except one little passage near the top of p 24 "and be weak in proportion"³[.] I do not recollect any expression of an opinion on my part that induction is weak because it occurs or may occur along a curved line and it is easy to arrange a case in which the induction along a curved line shall be stronger than along a straight line the lines being selected from all those concerned. If the words were away that which remained would represent me as I feel but you probably have a meaning which I have not caught[.]

Ever My dear Sir | Yours Very truly | M. Faraday
Professor J.D. Forbes | &c &c &c
t.o.
I pop in two figures



Here the curved lines of induction are weaker than the straight line, and weaker as they are more curved



here many of the curved lines c. c. c. are stronger than the straight lines

1. Of Forbes (1857).

2. *Ibid.*, 977–82.

3. This seems to refer to *Ibid.*, 979 where Forbes discussed induction along curved lines, but did not use the term weak.

Letter 3149¹

Duc de Plaisance to Faraday

30 May 1856

From the printed original in RS MS 241, f. 144

Grande Chancellerie.
ORDRE IMPÉRIAL DE LA LÉGION D'HONNEUR
No. 18293

Nomination de Commandeur.

S.M. l'Empereur, par Décret du quatorze Novembre Mil huit cent cinquante Cinq a promu en grade de Commandeur de l'Ordre Impérial de la Légion d'honneur M

Faraday (à Londres),
Exposant à l'Exposition Universelle
pour prendre rang à dater du même jour.
Paris, le treize Mai 1856.

Le Grand Chancelier | de l'Ordre Impérial de la Légion d'honneur |
Duc de Plaisance

TRANSLATION

Grand Chancery.
IMPERIAL ORDER OF THE LEGION OF HONOUR
no. 18293

Nomination of Commander.

H.M. the Emperor, by Decree of 14 November 1855 has promoted to the rank of Commander of the Imperial Order of the Legion of Honour Mr. Faraday (in London),

Exhibitor at the Universal Exhibition
to take rank as of the same day.
Paris, 13 May 1856

The Grand Chancellor | of the Imperial Order of the Legion of Honour
| Duc de Plaisance

1. This is a reissue of letter 3043. See note 2, letter 3150.

Letter 3150**Jean-Baptiste-André Dumas to Faraday****c.30 May 1856¹****From the original in IET MS SC 2**

Mon cher monsieur et Ami

Je n'ajouterai rien aux informations que vous trouverez dans les lettres et documents qui accompagnent l'envoi que j'ai la satisfaction de vous faire².

Vous verrez que le collier qui vous était destiné a été mis en votre absence par l'Empereur autour du cou de notre grand peintre M. delacroix. J'estime que M. delacroix³ est bien heureux de porter quelque chose qui vous appartenait.

Si vous venez nous voir, vous pourrez juger combien toute notre académie et toute notre jeunesse Scientifique qui vous honorent et qui vous aiment comme le maître à tous ont été heureux de voir votre nom au milieu des noms éminents que l'ordre de la légion d'honneur réunit.

Mes respects et mes hommages à Madame Faraday, de ma part avec tous les bons souvenirs et tous les vœux de la famille

Tout à vous | J. Dumas

TRANSLATION

My dear Sir and friend,

I will not add anything to the information which you will find in the letters and documents which accompany the package that I have the satisfaction of sending².

You will see that the chain, which was destined for you, in your absence, was hung by the Emperor round the neck of our great painter Mr. Delacroix³. I expect that Mr. Delacroix is well pleased to wear something that was due to you.

If you came to see us, you would be able to judge how much our academy and all our scientific youth, who honour you and love you as the master of all, were pleased to see your name amongst the eminent names that the Legion of Honour brings together.

My respects and homage to Mrs. Faraday on my own behalf, with fond remembrances, and best wishes from my family

All yours | J. Dumas

1. Dated on the basis that letter 3152 is the reply.

2. This probably included the insignia of the Commander of the Legion of Honour as well as letter 3149. Because Dumas knew Faraday had not received the insignia (letter 3125), he seems to have assumed that Faraday had not received the official notification (which he had, letter 3043) and presumably asked for it to be reissued.

3. Ferdinand-Victor-Eugène Delacroix (1798–1863, DBF). French painter, appointed a Commander of the Legion of Honour in 1855. Galset de Kulture to Dumas, 13 May 1856, IET MS SC 2 (presumably also included in Dumas's package) explained that after Delacroix had been presented with Faraday's insignia on 15 November 1855 he had taken it away and it would now shortly be recovered.

Letter 3151

Faraday to Elizabeth Reid¹

2 June 1856

From a typescript in RI MS

Royal Institution | 2 Jun 1856

My dear Elizabeth,

I enclose the two tickets of which we spoke:— how useful a remembrance is to bring these things to mind: and yet I fear it is a very poor substitute for memory:— however I have not forgotten this time.

Your Affectionate Uncle | M. Faraday

1. Elizabeth Reid (1830–1902, Reid, C.L. (1914), GRO). A niece of Sarah Faraday. Made confession of faith in London Sandemanian Church on 4 July 1847. Excluded 24 August 1851 but restored.

Letter 3152

Faraday to Jean-Baptiste-André Dumas

2 June 1856

From the original in AS MS

Royal Institution | 2 Jun 1856

My dear friend,

I have received from the Embassy the fruits of all your care and trouble taken for me¹. I feel that I am too much honored; and more by your personal remembrance & exertion than by any thing else; but I am very grateful to the Emperor & to all who have thought me worthy of their consideration in this matter. I hope to see you early in July i.e. before the middle of the month; but trust I shall not be so weary then as I am now, or else I must make my visit to Paris very short[.]

Most respectful remembrance to Madame Dumas in which as well as in those to yourself my wife joins me[.]

Ever | Your faithful friend | M. Faraday

Monsieur | Monsieur Dumas | Secrétaire | &c &c &c &c

1. See letter 3150.

Letter 3153**Thomas Henry Farrer to Faraday****2 June 1856****From the original press copy in TNA MT4/22, p. 2860**

2 Jun 1856

5475 | 5589

acknowledge the receipt of and to thank you for your letters of the 22nd and 26th May¹, reporting on Dr. Normandy's Apparatus for distilling Sea water²; and to inform you that your request with regard to your Reports being considered confidential, shall be strictly adhered to.

Their Lordships desire me to add that attention will be paid to the recommendation contained in your letter of the 26th May, to the effect that water distilled by this Apparatus should not be kept in leaden vessels.

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
Professor Faraday | Royal Institution

1. Not found, but evidently Faraday's responses to letters 3143 and 3146.

2. See letter 3136.

Letter 3154**John Tyndall to Faraday****2 June 1856****From the original in RI MS RI CG3/1/3**

[Royal Institution embossed letterhead] | 2 Jun

Dear Mr. Faraday,

Sir Roderick Murchison I am informed has kindly expressed his readiness to grant us specimens from the Museum of Practical Geology provided an official application be made to him on the part of the managers. Fortunately a meeting occurs today, and I should feel obliged if you would bring the subject forward and obtain for me the application which Sir Roderick considers necessary¹.

Believe me | most truly yours | John Tyndall

Endorsement: "for certain geological specimens illustrative of slaty cleavage"

1. This was for Tyndall (1856a), Friday Evening Discourse of 6 June 1856. For the request see RI MM, 2 June 1856, 11: 152.

Letter 3155**Faraday to Paul Edmund de Strzelecki¹****7 June 1856****From the original in the possession of Kurt Wolfgang Vincentz**

[Royal Institution embossed letterhead],

Royal Institution | 7 Jun 1856

Many thanks my dear Sir for Your publication² – I have been very glad to have my mind freshened up about the matter[.]

Ever Truly Yours | M. Faraday

The Count Strzelecki | &c &c &c

1. Paul Edmund de Strzelecki (1796–1873, ODNB). Polish-born explorer of Australia.

2. Strzelecki (1856).

Letter 3156**Faraday to Cornelia Augusta Hewett Crosse¹****9 June 1856****From the original in Peirpont Morgan Library**

Royal Institution | 9 Jun 1856

My dear Mrs. Cross[e],

I send you the ticket² and hope you will be in some degree interested – as I shall feel much honored by your presence. I do hope to be at Cheltenham³ part of the week & have received some kind invitations there⁴ – but cannot tell how events will turn up[.]

Ever | Your faithful Servant | M. Faraday

1. Cornelia Augusta Hewett Crosse, née Berkeley (d.1895, age 68, GRO). Married the amateur man of science Andrew Crosse (1784–1855, ODNB) in 1850. See under his ODNB entry.

2. Presumably for Faraday (1856b), Friday Evening Discourse of 13 June 1856.

3. For the meeting of the British Association.

4. See letter 3121.

Letter 3157**Faraday to George Thomas Staunton¹****14 June 1856****From Staunton (1857), 54**

14 June 1856

Mr. Faraday presents his compliments and sincere thanks to Sir George Staunton, for the favor he has done him in the presentation of a copy of the Memoirs², and he hopes he may add, of a very valuable life.

1. George Thomas Staunton (1781–1859, ODNB). Diplomat and politician.
2. Staunton (1856).

Letter 3158**George Gabriel Stokes to Faraday****16 June 1856****From the original in IET MS SC 2**

Pembroke College Cambridge | 26 Jun 1856

My dear Sir,

I have looked again for my piece of ruby glass, but without success. But I do not think it of much consequence. I distinctly recollect examining it for fluorescence¹, and finding that it had none, or at least no more than a colourless glass, none which could be in any way connected with the colouring substance. I distinctly recollect noticing that there was more “false dispersion” or scattered light than in colourless glasses, but it did not occur to me that this was connected with the colour by transmitted light. According to the best of an imperfect recollection the dispersing particles were somewhat sparse, not giving an apparently continuous beam like what is seen in the gold mixture from phosphorus. But I am by no means sure of this. It was fluorescence I was seeking after, and finding that this was only a case of “false dispersion” I paid little attention to it. I think it much more probable that the ruby glass I examined was like what you have examined than that my vague supposed recollection of the discontinuity of the dispersed beam was correct.

You have clearly shown that the colouring matter in the phosphorus–gold-mixture is in suspension, but I cannot believe that it is metallic gold. Such a supposition is utterly at variance with my optical experience. I know of *no* instance in which the same substance exhibits two totally different characters as to absorption, such as do the FeO.SO₃ gold and phosphorus–gold mixtures. Many cases occur in which the tint is quite different according to the thickness looked through; but the prism shows that these are among the instances in which the identity of the character of the absorption is most markedly exhibited. Moreover the transmitted colour of the FeO.SO₃–gold mixture agrees, but that of the phosphorus gold mixture does not agree, with what might have been predicted from the reflected colour of gold. I can not help believing in the existence of a purple oxide².

Yours very truly | G.G. Stokes

1. Which Stokes had discovered in 1851. Stokes (1852).
2. This was Stokes’s response to Faraday (1856b), Friday Evening Discourse of 13 June 1856, where he made this point, p. 312.

Letter 3159**Faraday to George Gabriel Stokes****17 June 1856****From the original in ULC Add MS 7656, F21**

Royal Institution | 17 Jun 1856

My dear Sir,

I am much obliged for your letter¹. Though I have a strong impression on the side of the question which admits that finely divided particles of gold may transmit ruby light yet as I said I am by no means certain & mean to work out the point. electric explosion of gold wire seem to present an easy way of settling the question as they may be made to occur in hydrogen, carbonic acid, & other gases, but I want to idle for a time so I shall put your letter with my experimental notes & resume both together[.]

Ever Truly Yours | M. Faraday
G.G. Stokes Esqr | &c &c &c

1. Letter 3158.

Letter 3160**Faraday to John Richardson Major¹****21 June 1856****From the original in FACLM H MS c1**

Royal Institution | 21 Jun 1856

Sir,

I am much obliged by your kind invitation to the Photographic Society but I am constrained to limit both my pleasures & my occupations[.] No one ought to utter an opinion on the subject you mention without especial experience and I on my part possess none. Neither can I try to gain it for I am obliged to reserve what energy remains to me for the pursuit of my own especial trains of thought.

I am Sir | Your Obedient Servant | M. Faraday
J.R. Major Esqr | &c &c &c

1. John Richardson Major (1797–1876, B2). Editor of the *Journal of the Photographic Society*.

Letter 3161**Robert Welbank¹ to Faraday****22 June 1856****From the original in GL MS 30108/2/69**

Trinity House | 22 Jun 1856

My Dear Mr. Faraday,

Our short parley of last night would give you but a faint idea of what I wished to convey to you upon the subject of a light onboard of a vessel

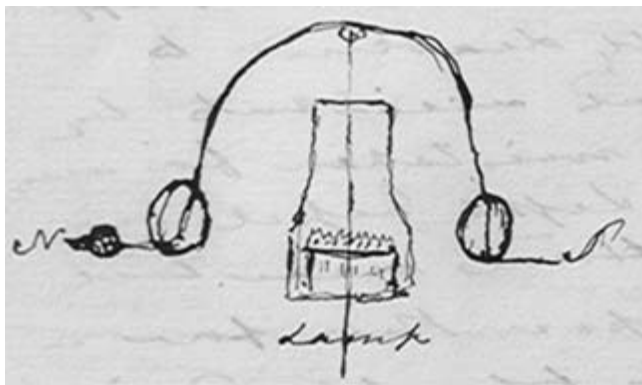
moored in a narrow Channel for the purpose of Lighting only a small portion of the circle instead of the whole circumference—

In consequence of the many distressing accidents to *Sailing* vessels through the carelessness or want of a good look out onboard of *Steamers* – I have proposed at the Trinity House to light a *new Channell* for the express use of *Steamers* by *Night* and to relieve all other Channels in the Neighbourhood from their intrusion by *Night*.

A *light vessel* being under the influence of the *Wind & tide* frequently changing her position & swinging round by either power and never or seldom *quite steady* – It has occurred to me that the *Magnetic Needle* might be made available to keep open at all times as much of the circle and *direction of the Ray* of *light* as might be required and obscuring all not required – as for example – If I want a Channell to be lighted East & West – North & South should be obscured to prevent the Spark or focal ray being sent beyond the narrow ridge up and bounding the Channell[.]

In a Ship the magnetic pole is always the same point – I therefore think if the Needle can be made to traverse upon the Centre point laden at the North & South ends with a shade of any obscuring property – however slight the focal Ray in those directions will be sufficiently deadened to prevent accidents by being mistaken for any other *Light Vessel* or *Light House* near to it[.]

Suppose therefore a Lamp to be placed in the centre of a vessel shewing its focal Ray all round the compass – A pin passing up the Centre of the Lamp within the burners and glass tubes above to a proper *height to prevent being melted by the heat* and then the Magnetic Needle made to embrace the whole one points *North* the other *South* and sufficiently *poised* with the obscuring *weights* at the extremes of the *Needle* – I think my object would be attained –



Have the kindness to give this idea your serious thought & oblige the
Father of the Corporation of Trinity House[.]
 Robert Welbank

1. Robert Welbank (d.1857, age 79, *Gent. Mag.*, 1857, 3: 101). An Elder Brother of Trinity House, 1825–1857, Chaplin (1950), 84.

Letter 3162

Robert Welbank¹ to Faraday

24 June 1856

From the original in GL MS 30108/2/69

Tandridge near, Godstone | 24 Jun 1856

Dear Mr. Faraday,

I returned home yesterday after seeing you & have this morning remembered the subject of my interview and the Letter which you got after I left you or rather which I have no doubt came to your hand by the Post² – The accompanying paper³ contains the substance of my Mornings occupation[.]

Think it over in *due time* and let me know your sentiments upon its *practicability* – & believe me very truly yours

Robert Welbank –

I shall be glad to pay any *charge* –

1. Robert Welbank (d.1857, age 79, *Gent. Mag.*, 1857, 3: 101). An Elder Brother of Trinity House, 1825–1857, Chaplin (1950), 84.

2. Letter 3161.

3. This paper is included in GL MS 30108/2/69.

Letter 3163

Thomas Sopwith¹ to Faraday

30 June 1856

From the original in IET MS SC 2

Allenheads Haydon Bridge | 30 Jun 1856

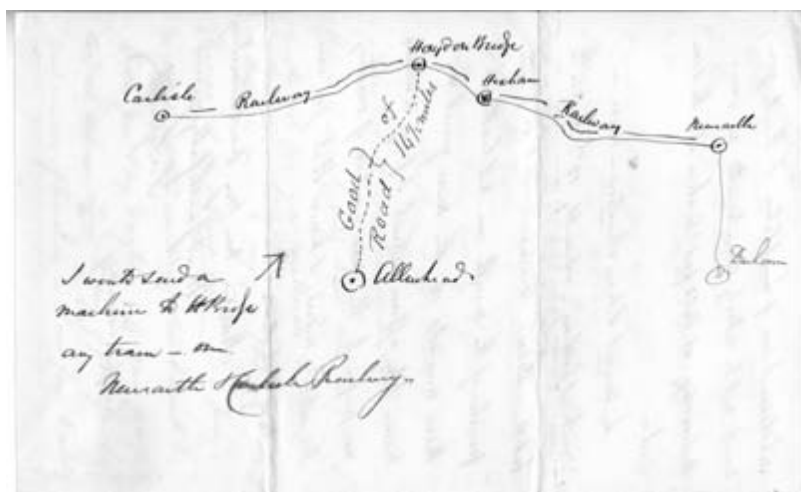
My dear Mr. Faraday,

In the months of *July – September* and *October* this year – (or November though I am *doubtful* whether I may be at home* in that month & it is rather too late for our high moors & mountains) – You cannot come wrong as I hope to be at home & I think *you* would also be *at home* & find much to interest you combined with an abundance of opportunity of rest moderate exercise & agreeable relaxation & if any of yr. family circle can accompany you it would add much to the pleasure[.] It is of no use to offer a philosopher any more sincere welcome than what is implied in the two words “*home & comfort*” for these imply all that can be desired. I am in my own habits a man of

business & make business my main pursuit but I have some small inklings for science & some feeling of admiration for art and what with books & drawings & our fine bold mountain scenery & good schools & Mines & Smelting & de-silvering and a long list of miscellaneous etceteras I think it would *very* hard with us if we could not find you materials to *think* if thoughtful or to enjoy a pleasant leisure "*if on nothing bent*". But pray do not forget how exceedingly glad I would be to see you & my daughter² will give a very cordial welcome indeed to any of your good companions of travel – I am fain to add whatever I can of temptation & think you would not be utterly uninterested in our Meteorology at 1400 feet of Elevation above the Sea level

In August I have *Shooters* – noise – *smoke & feeding* – especially 12th to 24th but the *extreme* commencement or *final* finish of the month are not liable to these inroads on the more quiet & home like affair which I fancy is what you prefer to the mere gaities of visiting for which indeed I know that you care nothing at all³.]

Believe me My Dear Mr. Faraday | Most faithfully yours | T. Sopwith
Professor Faraday | &c &c &c



* aiblins Egypt⁴

1. Thomas Sopwith (1803–1879, ODNB). Mining engineer.
2. Ursula Sopwith (d.1900, age 67, GRO under Chadwick).
3. For an account of Faraday's visit to Sopwith at Allendeheads on 14 and 15 July 1856 see Richardson (1891), 240–3.
4. Sopwith visited Egypt in December 1856 and January 1857. Richardson (1891), 260–1. 'Aiblins' is a Scottish word meaning possibly or perhaps.

Letter 3164**Faraday to Anthony Panizzi****3 July 1856****From the original in BL add MS 54224, f. 225**

[Royal Institution embossed letterhead] | 3 Jul 1856

My dear Sir,

I do not know if I can be of any use to Mr. Smirke but if I can I shall be very happy to see him. I shall be at home tomorrow morning before 12o'clk & probably also on Saturday morning¹[.] On Tuesday² I leave London[.]

Ever Truly Yours | M. Faraday

A. Panizzi Esq | &c &c &c

1. That is 5 July 1856.

2. That is 8 July 1856.

Letter 3165**Thomas Henry Farrer to Faraday****12 July 1856****From the original press copy in TNA MT4/23, p. 3530**

12 Jul 1856

6895

request that you will be good enough to favor my Lords with the results of your experiments with the Reflector on Mr. Gordon's plan, especially as to whether it possesses any practical advantage over the ordinary parabolic reflector.

I am | Sir | Your obedient Servant | T.H. Farrer
Professor Faraday F.R.S. | Royal Institution

Letter 3166**John Tyndall to Faraday****22 July 1856****From the typescript in RI MS JT TS Volume 12, pp. 4049–51**

R.I. | Tuesday, 22 Jul 1856

My dear Mr. Faraday,

I have often thought of writing to you, and should have done so before now had I not been tossed about in body and soul more than I anticipated.

The examinations took me a long time. It is dreary work plodding out these answers, hunted by the consciousness that you may possibly do a candidate wrong. Then this time the examiners had to accompany their returns with a report, which to draw up properly also took time. I have a suspicion that this is one result of *that* correspondence¹. I have put my notations

again strongly, but have done it by arranging facts, rather than by expressing an opinion. This I shall continue to do whenever I have an opportunity, as long as I find natural philosophy, taken together rated equal to French alone, Latin alone, and at 250 marks less than the number awarded to English literature. Such an absurdity I think cannot live long in the world, if there be only somebody to hold it up to the attention of men.

I have made a number of experiments connected with the passage of a current across the place of junction of two metals – Peltier² generalised³ – Here there is a conversion of heat into electricity; I fear the knowledge is a long way off still one does long to know something more of the intimate nature of their conversion. As yet I am in perfect darkness.

Monday I was swept suddenly down to Hampshire to see my young friend⁴ who, in accordance with the advice of Dr. Stokes⁵ of Dublin, goes off to the South of France immediately with his wife⁶. She manifestly has no idea of her danger which makes it all the sadder. She talks about coming back as a matter of course, when it is the opinion of some that she hardly ever will return.

I have had rather a strong letter⁷ from Mr. Sorby⁸ the geologist complaining of my misapprehension of his views, in my late lecture at the R.I.⁹ He has also sent a paper¹⁰ on the subject to Francis. In his eagerness to make out a strong case against me, he has left one little more to do than to quote his own words. This I have done¹¹ without any show of feeling – indeed before I wrote I got all feeling under, for the longer I live the more I feel that these personal discussions are a perfect nuisance to science. There may be an avarice of the intellect as well as of the pocket, and one, in quality is not very much superior to the other. Snow Harris has sent up a violent response to Reiss's last short communication¹², which also might have been far milder and more courteous. Francis has written to him advising him not to print it – I do not know what the result will be¹³.

One morning soon after you left I received a note from Stokes informing me that the grant committee had acceded to my application for £100¹⁴. I wrote to him requesting him to return my thanks to the committee for this proof of their confidence; I said however that having been led to conclude that my application had been considered ineligible I had resorted to private sources and obtained from them all the funds I needed. If therefore the grant committee thought the money voted to me could be otherwise usefully employed, I should be glad if they would consider my application as extinct, and the £100 again at their disposal. I concluded by saying that at some future time, if permitted, I should probably be glad to avail myself of the assistance which the grant offers.* Stokes has written to me again¹⁵, the upshot of his note is that he does not think it likely that it will be recalled, so the matter rests at present.

The morning on which Sorby's letter reached me I met Professor Haughton¹⁶ of Trinity Coll. Dublin. He is an eminent mathematician and geologist; Professor indeed of Geology in Dublin, and he told me that his interpretation of Sorby was precisely the same as mine.

Now a truce to science. I hope you are well, I hope Mrs. Faraday and Miss Barnard are well. This is glorious weather in the country – but at Newcastle with its artificial atmosphere it may be the reverse. I associate with the name visions of foundries, smoke, and grimy miners – perhaps the reality is not quite as bad as my picture of it.

I am half inclined to write this letter again, for it is very confusedly written but I remember that you are accustomed to Matteucci, and after him you will not find me difficult to decipher.

Believe me ever | faithfully yours | John Tyndall

*Bence Jones agreed with this view of the subject.

1. See letter 3139.
2. Jean Charles Athanase Peltier (1785–1845, DSB). French physicist.
3. Peltier (1834).
4. Thomas Archer Hirst.
5. William Stokes (1804–1878, ODNB). Dublin physician.
6. Anna Hirst, née Martin (d.1857 see ODNB under T.A. Hirst). Married Hirst in 1854.
7. Not found.
8. Henry Clifton Sorby (1826–1908, ODNB). Pioneer of microscopic geology.
9. Tyndall (1856a), Friday Evening Discourse of 6 June 1856.
10. Sorby (1856).
11. Tyndall (1856b).
12. Riess (1856).
13. It was published as Harris (1856b).
14. The Royal Society awarded Tyndall a grant of £100 to continue his work on magnetism. See RS CM, 11 July 1856, 2: 363–4.
15. Stokes to Tyndall, 19 July 1856, RI MS JT/1/S/218.
16. Samuel Haughton (1821–1897, ODNB). Irish geologist.

Letter 3167

Faraday to Jane Barnard

26 July 1856

From the original in Russian Academy of Sciences Archives 336/1/57

[Royal Institution embossed letterhead] | 26 Jul 1856

My dear Jenny,

I thank you very truly for your note of yesterday – it was as good & better than a sight of you. I have got home all right but you must hear how from your Aunt – I have put up the *Valisneria*¹ – have thrown away the dead beetle, and wondered where the great number of large snails on the vessel could get their food & their houses from. But I shall leave all as they are[.]

Dr. Bence Jones has been here & tells me he has another son² – his wife & the child being quite prosperous_[.] He seems very happy – & asked after you particularly – I gave him your message – &c_[.]

I must finish give my love to those with you and think of me as your
Very Affectionate Uncle | M. Faraday

1. An aquatic plant.

2. Archibald Bence Jones (1856–1937, *The Times*, 25 February 1937, p. 1, col. a). Later a barrister.

Letter 3168

Faraday to Sydney Smirke

26 July 1856

From the original in BL add MS 60745, f. 109

Royal Institution | 26 Jul 1856

Dear Sir,

I arrived in town late last night & leave town for Paris early on Tuesday morning¹. I am so occupied that I cannot see Mr. De la Rue before my return from Paris – and so am unable to answer your letter at present for I could give no opinion before talking with him_[.]

Ever Truly Yours | M. Faraday
Sydney Smirke Esq | &c &c &c

1. That is 29 July 1856.

Letter 3169

Faraday to Thomas Andrews

27 July 1856

From the original in SM MS 350/36

Royal Institution | 27 Jul 1856

My dear Andrews,

It gave me great pleasure to receive your kind letter and I shall make myself present to you as soon as I can – We start either tomorrow or Tuesday Morning for Paris & Paris only for I long to see my kind friend Dumas. Very sincere thanks to you for your offer of help & I dare say I shall use some of it:– but the fact is that my memory fails so fast that I dare not trust myself alone so I avail myself of the kind aid & care of my brother in law Mr. George Barnard who accompanies me expressly that he may relieve me in all matter that he can – in fact I am in his hands. He settles when we start and has arranged where we shall go:– he takes all the care of money passports hours, routes – Hotels – calls &c and I trust you will let me present him to you – he is an artist – and knowing Paris pretty well I commit all things into his hands_[.]

My kindest remembrances if they may be called so to Mrs. Andrews¹ – I want here [sic] to have some of the feeling towards me that her husband has i.e. I want her to think of me kindly & favourable – it is very pleasant[.]

Ever My dear Andrews | Most truly Yours | M. Faraday

Address: Dr. Thomas Andrews | &c &c &c | Hotel de Luxembourg | 54 Rue de Vaugirard | à Paris

1. Jane Hardie Andrews, née Walker. Married Thomas Andrews in 1842. See his ODNB entry.

Letter 3170

Justus Liebig to Faraday

27 July 1856

From the original in IET MS SC 2

Münich | 27 Jul 1856

My dear Faraday,

I beg you to excuse myself for having so longtime delayed to answer your letter of the 1 May¹ for which I beg to accept my best thanks.

Since last year I find myself engaged in a very stupid controversy with Mr. Lawes of Rothamsted about Scientific principles in Agriculture. Having never read or understood my book² he pretended to demonstrate by experiments that the Science of Chemistry could do nothing for practical Agriculture and that the knowledge of Laws of nature could not be of any use in practical farming! Mr. Lawes is, I believe, a manufacturer of manure and by my disputing his scientific position and showing that his conclusions are erroneous he thinks to loose his customers; This is, I fear, the reason that he went so far as to attack my good faith in an Article (No 36) of the Journal of the Roy. Agric. Soc. Of England³. I was obliged to write an article for the Same journal, which will appear in the No. of July⁴ and in which I hope to have succeeded in uprooting his errors.

You have had always the good fortune, to find for all your works and investigations a well disciplined public which acknowledged grateful[ly] and accepted with thanks the immense services you rendered to Science and to mankind; but in Chemistry and its applications to Agriculture and Physiology I have to deal with a set of people without any Scientific education and who know, or believe to know, all these things better than the natural philosopher himself. I despair sometimes to be able to convince them of the most simple truth. It is that sort of people who believe on walking tables and all kind of nonsensical theory's. Being occupied during ten years with other researches I did not care about the opposition of the so called practical men, but last year I became aware that they fought with a Shadow and that they failed to discern the truth which my theory contained. I should never have thought to answer any articles, if the questions which are involved in that controversy

were not of such great importance to mankind. We are advanced far enough to decide the question about the right way to produce more corn and more meat from the same surface of land. The fortune and income of most people depend on them.

I should think that you could do a great deal of good by a lecture "*on the methods to apply Scientific principles to practical purposes*" next winter in the royal institution! Perhaps you have the goodness to read my article in the Journal of the Roy. Agr. Soc. And it is possible you find matter in it for such a lecture. If my conclusions and inferences have your assent, I am sure all this opposition will cease and a truly scientific agriculture will commence⁵.

Mr. Fr. Barnard is an excellent young man we like him very much and are always glad to see him in our house; he is not coming so often as we wish it and it requires mostly a formal invitation to see him; he is to[o] strict an Englishman. The german custom is to go to take thea [sic] with his friends without these ceremonies. Mr. Barnard is very assiduous and I hear that his professors are much satisfied by his progress. We talk very often of you, his aunt and his aimable Sister. Pray express my kindest regards to them!

Dear Faraday I am with all my heart | Yours very Sincerely | Justus Liebig
(Excuse my horrible English!)

1. Letter 3134.
2. Liebig (1855).
3. Lawes and Gilbert (1856).
4. Liebig (1856).
5. On this episode see Brock (1997), 173–6.

Letter 3171

Sarah Faraday to Noëlie Dumas¹

29 July 1856

From the original in Sidney M. Edelstein Library Jerusalem MS 3/1–4

Hotel Maurice | 29 Jul 1856

Will you My dear Miss Dumas allow me the pleasure of leaving this trifling remembrance of our most pleasant visit to your family & to Paris – it is a little work by my favourite author Miss Edgeworth², and has in my younger days been a source of great pleasure to me – it may perhaps tempt you and your young friends the Miss Edwards³ to pursue your study of the English language[.]

With grateful thanks to your Father & Mother for all their kind attention believe me to remain

My dear Miss Dumas | Yours most truly | S. Faraday

Endorsement: Faraday à Mlle Noëlie Dumas (Mme Herve Mangon⁴)



Plate 6. Sarah and Michael Faraday.

1. A daughter of Jean-Baptiste-André and Herménie Dumas.
2. Maria Edgeworth (1767–1849, ODNB). Writer.
3. Possibly the daughters of the French zoologist Henri Milne-Edwards, (1800–1885, DSB).
4. Charles-François-Hervé Mangon (1824–1888, LGE). Later Minister of Agriculture.

Letter 3172

Faraday to Jean-Baptiste-André Dumas

1 August 1856

From the original in AS MS

Hotel Maurice | Friday

Dear & honored friend,

I fear to disturb you too much by calling yet am anxious to know how I ought properly to present myself at the doors of the Academy des Sciences next Monday and at what hour?¹ If you could tell me this by a line it would set me at rest.

On Saturday i.e. tomorrow we go to Fontainebleau for rest on the Dimanche & return Monday morning[.]

Your Very devoted | M. Faraday

Monsieur | Monsieur Dumas | &c &c &c

1. The meeting was on 4 August 1856. See *Comptes Rendus*, 1856, **43**: 271.

Letter 3173

Luigi Palmieri¹ to Faraday

4 August 1856

From the original in IET MS SC 2

Napoli | 4 Agosto 1856

Chiarissimo Signore

Il professore Scacchi² ed io vi abbiamo in qualche tempo inviato un volume col titolo: Eruzioni del Vesuvio del 1850 e 55³; in esso trovate come io abbia tentato di ridurre le tensioni elettriche dell'atmosfera a misura assoluta; desidero sopra questo punto il vostro autorevole giudizio. Se poi il libro non vi fosse pervenuto, vi prego indicarmi qualche mezzo sicuro per potervene spedire un altro esemplare. Vi fo tenere intanto la figura del mio elettrometro per uso della elettricità atmosferica.

Sono con sentimenti di alta stima &c | Vro Dmo Obbmo Serre | Luigi Palmieri

TRANSLATION

Naples | 4 Aug 1856

Dearest Sir,

Professor Scacchi² and I sent you some time ago a volume entitled: The Eruptions of Vesuvius in 1850 and 1855³; in it you will find how I have tried to reduce the electric tensions of the atmosphere to an absolute measure; I would like your authoritative judgement on this subject. If therefore the book did not arrive, please let me know of some secure means of sending you another copy. In the meantime please find enclosed the figure of my electrometer to measure atmospheric electricity.

I am with sentiments of high esteem &c | Your most devoted and most obliged servant | Luigi Palmieri

Endorsement: Eruption of Vesuvius

Address: A Monsieur | M. M. Faraday | R. Institution | of | London

1. Luigi Palmieri (1807–1896, LUI). Neapolitan vulcanologist.
2. Arcangelo Scacchi (1810–1893, LUI). Neapolitan mineralogist.
3. Guarini *et al.* (1855).

Letter 3174

Joseph Antione Ferdinand Plateau to Faraday

5 August 1856

From the original in IET MS SC 2

Gand | 5 Août 1856

Mon Cher Monsieur Faraday,

vous recevez sans doute en même temps que cette lettre, trois exemplaires de la *troisième Série* de mes recherches sur les figures d'équilibre des liquides¹; veuillez accepter l'un d'eux et offrir le second, de ma part, à la Société Royale et le troisième à l'Institution Royale. Cette troisième Série, jointe à la dernière partie de la *deuxième*² forme un ensemble contenant une théorie complète de la constitution des veines liquides lancées par des orifices circulaires et soumises ou non à l'influence de mouvements vibratoires. Ne lisez pas cette troisième série, car elle ne peut être comprise si l'on ne s'est bien pénétré de celle qui la précède, et le genre de vos propres recherches vous éloigne trop de ce qui concerne les théories capillaires pour que je désire que vous preniez cette peine.

D'ailleurs, j'ai à vous prier de vouloir bien perdre pour moi, d'une autre manière, une petite portion de votre temps, et j'espère que vous ne

me refuserez pas; voici ce dont il s'agit; je m'occupe en ce moment à mettre en ordre, pour la publication, un ouvrage que j'avais commencé il y a un grand nombre d'années, et qui est sur le point d'être terminé: c'est une bibliographie en partie analytique des phénomènes subjectifs de la vision tels que la *persistance des impressions*, les *couleurs accidentelles*, &c. Or, je trouve dans mes notes, que l'invention du *Thaumatrope* est due au docteur Paris³, et qu'il l'a décrite dans un ouvrage intitulé: *Philosophy in sport made science in earnest*⁴; mais j'ignore le lieu et la date de la publication de cet ouvrage, qui paraît n'exister dans aucune bibliothèque de Belgique, et cependant je dois nécessairement l'insérer dans ma bibliographie⁵. Maintenant l'ouvrage étant anglais, ou plutôt, je pense, américain, je me suis dit qu'il devait exister dans les bibliothèques de Londres. Vous me rendriez donc un grand service, si vous vouliez bien prendre la peine de chercher ce même ouvrage, et, si vous le trouvez, de me faire connaître le lieu et la date de la publication, le volume dans lequel se trouve la description du *Thaumatrope*, et enfin la page où commence cette description.

Si l'ouvrage du Dr. Paris est postérieur à l'année 1800, ces renseignements me suffisent, mais s'il a paru dans le siècle précédent, j'aurais besoin de quelque chose de plus, et je ne vous le demande qu'en tremblant, car je sens que je deviens indiscret: Si le passage où il est question du *Thaumatrope* n'est pas long, vous compléteriez le service que j'attends de votre obligeance en le faisant copier pour le joindre aux renseignements précédents; Seulement veuillez ne pas oublier que cela est inutile si l'ouvrage est de notre siècle, et que, si le passage est étendu, je ne veux dans aucun cas que vous vous donniez cet embarras.

Vous me pardonnerez, j'en suis certain d'abuser ainsi de vos bontés pour moi, quand vous réfléchirez qu'il m'est absolument impossible d'aller par moi-même consulter les bibliothèques étrangères.

J'ai reçu les différents exemplaires de vos travaux, que vous m'avez fait l'honneur de m'envoyer; les trois derniers⁶ serviront à étendre et à compléter Vos magnifiques recherches sur le dia-magnétisme; mais permettez-moi de vous exprimer ici toute mon admiration pour l'une de vos découvertes antérieures: Je veux parler de l'induction statique latérale exercée par un courant électrique dans un fil métallique recouvert de gutta percha et plongé dans l'eau⁷. Qui aurait pu s'attendre à pareille chose? Qui aurait pu s'imaginer que la vitesse de transmission de l'électricité éprouverait dans ce cas, un retard considérable? Du reste on est habitué à vous voir enfanter des merveilles.

Encore une fois, mon Cher Monsieur Faraday, veuillez me pardonner mon importunité, et avoir d'avance mes remerciements.

Agrez l'assurance de tous mes sentiments de respectueuse affection |
Jh Plateau

TRANSLATION

Ghent | 5 Aug 1856

My Dear Mr. Faraday,

You have no doubt received at the same time as this letter, three copies of the *third series* of my research on the figures of the equilibrium of liquids¹; please accept one yourself and offer the second, on my behalf, to the Royal Society and the third, to the Royal Institution. This third set, joined to the last part of the *second*² forms a whole containing a complete theory on the constitution of the liquid veins dropped by circular openings and submitted, or not, to the influence of vibratory movements. Do not read this third set, because it cannot be understood unless one has absorbed that which precedes it, and the field of your own research is too far removed from that which concerns capillary theories for me to wish you to take that trouble.

Besides, I do wish to ask you to lose, in another manner, a small portion of your time for me, and I hope that you will not refuse; this is what it concerns; I am at this moment putting in order, for publication, a work that I began a great many years ago, and which is on the point of completion: it is a bibliography, partly analytic, on the subjective phenomena of vision, such as *persistence of impressions, accidental colours, &c.* However, I find in my notes, that the invention of the *Thaumatrope* is due to Dr. Paris³, and that he described it in a work entitled: *Philosophy in sport made science in earnest*⁴; but I am ignorant of the place and date of the publication of this work, which appears not to exist in any library in Belgium, and I must, however, necessarily insert it into my bibliography⁵. Now the work being English, or rather, I think, American, I thought that it had to exist in the libraries of London. You would therefore do me a great service, if you would take the trouble to look for this same work, and, if you find it, to make known to me the place and date of publication, the volume in which the description of the *Thaumatrope* is found, and finally the page on which the description begins.

If the work of Dr. Paris postdates the year 1800, this information will suffice, but if it appeared in the previous century, I would need something more, and I ask this of you with unease, as I feel that I am becoming indiscreet: If the passage concerning the *Thaumatrope* is not long, you would complete the service that I ask of your kindness by making a copy to enclose with the previous information; only please do not forget that that is not necessary if the work is from our century, and, if the passage is long, I do not want under any circumstances for you to give yourself this trouble.

You will forgive me, I am certain, this abuse of your goodness towards me, when you reflect how absolutely impossible it is for me to consult foreign libraries by myself.

I have received the various copies of your works, that you honoured me by sending; the last three⁶ will serve to extend and to complete your magnificent research on diamagnetism; but permit me to express here all my admiration for one of your previous discoveries: I wish to speak of the lateral static induction exerted by an electric current in a metallic wire covered with gutta percha and plunged into water⁷. Who could have thought of anything similar? Who could have imagined that the transmission speed of electricity would experience, in this case, a considerable delay? But one has become accustomed to seeing you give birth to marvels.

Once again, my Dear Mr. Faraday, please forgive my troubling you, and accept in advance my thanks.

Please accept the assurance of all my sentiments of respectful affection | Jh Plateau

1. Plateau (1856a).
2. Plateau (1849).
3. John Ayrton Paris (1785–1856, ODNB). Physician. First biographer of Humphry Davy.
4. [Paris] (1827), 3: 1–27.
5. Plateau (1877), 28 which cites [Paris] (1827) with the incorrect date. See letter 3176.
6. Probably including Faraday (1856c), ERE30 and (1855b), ERE[29b].
7. Faraday (1854a), Friday Evening Discourse of 20 January 1854.

Letter 3175

César Mansuète Despretz to Faraday

6 August 1856

From the original in IET MS SC 2

Le 6 Août 1856

Mon cher Confrère,

Il serait peut être agréable à Monsieur Votre frère¹ ou à Mons. Andrews de voir une Séance publique d'une des academies. Je vous envoie dans cette Supposition les deux billets de centre, que j'ai recus en ma qualité de viceprésident. recevez l'assurance des sentiments les plus distingués avec lesquels Je suis

Votre devoué | confrère | C M Despretz

TRANSLATION

6 Aug 1856

My dear Colleague,

It would perhaps give pleasure to your brother¹ or to Mr. Andrews to see a public sitting of one of the academies. I send you in this supposition the two centre tickets, which I received in my position as vice president. Receive the assurance of the most distinguished sentiments with which I am

Your devoted | colleague | C.M. Despretz

1. That is George Barnard.

Letter 3176**John Ayrton Paris¹ to Faraday****c.9 August 1856²****From the original in IET MS SC 2**

Dear Mr. Faraday,

The first edition of my 'Philosophy in Sport' was published in 1825³; in which appeared an account of the Thaumatrope; but previous to that, I cannot say how long it had appeared in the toymarket. I was first induced to publish it, at the earnest desire of my late friend Wm Phillips⁴, who was the first person to whom I shewed it; & he undertook to get the subjects lithographed, & to arrange the sale of it. It was, of course, soon pirated, & spurious copies distributed. A large number was exported to India, but the Captain turned out a rogue, deserted his cargo, & has never since been heard of – so much for my first & last merchantile speculation. I however gained about £150 from the sale in this country, which was expended for educational objects in my family; so here then you have 'a true and particular account of that wonderful phenomenon termed the Thaumatrope', as the showman would say. I may add that I never put my name to it.

Yours very faithfully | J.A. Paris

The *eighth* Edition of Philosophy in Sport⁵ is now in the press.

1. John Ayrton Paris (1785–1856, ODNB). Physician. First biographer of Humphry Davy.

2. Dated on the basis that this letter gives the information requested by Plateau in letter 3174 which he had clearly received by the time he wrote letter 3180.

3. Paris incorrectly recollected the date of the publication of [Paris] (1827).

4. William Phillips (1775–1828, ODNB). Printer and geologist.

5. Paris (1857), dedicated to Faraday.

Letter 3177**Faraday to Thomas Andrews****12 August 1856****From the original in SM MS 350/36**

Royal Institution | 12 Aug 1856

My dear Andrews,

Your note shocked me very much and my thoughts continually turn to poor Regnault. Mr. Barnard and I were with him at Sevres for several hours together on the previous Friday¹ and saw all his apparatus there and how he mounted himself up in his chair to read his manometer which I think 30 or 40 feet high. I cannot help supposing it was in some of these places he was engaged when he fell & yet cannot call to mind any chimney like that you speak of. When I read the account to Dr. Bence Jones he seemed to hope that the case was not so bad as your note supposed: he thought that though the concussion must have been very great yet that the symptoms were not

desperate. I hope it is so and that by this time there is some degree of recovery. I shall be very anxious until I know. Mr. Barnard will be passing through Paris in a day or two perhaps he may bring me good accounts[.]

I reached home in safety the same day that I left Paris & with a strong remembrance of all your kindness. It was time that I should return for I found when I reached home that I wanted 2 or 3 days rest before I could get out of the exhaustion into which I fell. Now I am quite well again[.]

With kindest regards to Mrs. Andrews² & remembrances to all friends

I am My dear Andrews | Ever Truly Yours | M. Faraday

Address: Dr. T. Andrews | &c &c &c | 54 Rue de Vaugirard | à Paris

1. That is 1 August 1856.

2. Jane Hardie Andrews, née Walker. Married Thomas Andrews in 1842. See his ODNB entry.

Letter 3178

Francesco Rossi¹ and Giovanni Veladini² to Faraday

12 August 1856

From the original in RS MS 241, f. 148

No 437

I.R. Istituto Lombardo di Scienze Lettere ed Arti,
Milano | 12 Agosto 1856

Chiarissimo Signore,

Nelle adunanze ordinarie die giorni 6 e 20 Dicembre 1855 il Corpo Accademico desideroso di accrescere lustro all'I.R. Istituto coll'aggiungere Persone celebri per i loro lavori scientifici, deliberò di nominarla a proprio Socio Corrispondente non residente in Lombardia.

In conseguenza di ciò la scrivente Presidenza si pregia di trasmetterle il Diploma relativo all'antidetta nomina pregandola che voglia prendere in cura speciale gli interessi scientifici di quest I.R. Istituto.

Il Presidente | Rossi

Il Segretario | Prof Gio Veladini

Al Chiarissimo Sign. Professor Michele Faraday, | Membro della Società Reale di Londra | Socio Corrispondente dell'I.R. Istituto Lombardo di Scienze | Lettere ed Arti | Londra

TRANSLATION

No 437

The Imperial Royal Lombard Institute of Science,
Letters and Arts, Milan | 12 Aug 1856

Dearest Sir,

At the ordinary meetings of 6 and 20 December 1855, the Academic Body, desirous of increase the prestige of the Imperial Royal Institute by adding to its

number persons celebrated for their scientific work, has resolved to nominate you herewith Corresponding Member, not resident in Lombardy.

In consequence of this, the current Presidency has the honour of sending the Diploma relative to the foresaid nomination, asking that you would keep in your particular care the scientific interests of this Imperial Royal Institute.

The President | Rossi

The Secretary | Prof Gio Veladini

To Dear Mr. Professor Michael Faraday, | Member of the Royal Society of London, | Corresponding Member of the Imperial Royal Lombard Institute of Science, | Letters and Arts | London

1. Unidentified.

2. Unidentified.

Letter 3179

Sarah Faraday to Benjamin Vincent

13 August 1856

From the original in RI MS F1 G27

[Royal Institution embossed letterhead] | 13 Aug 56

Very dear Friend,

Many thanks for your kind remembrance in writing to me, it seems we narrowly escaped meeting, it would have been pleasant to have done so, but it was better in some respects that you should have my room than my company.

I am happy to hear that I left a pleasant impressions among friends who were so kind to me¹, for I often have a misgiving after leaving, that I have been tempted to speak too freely, forgetting myself & my own character & how far it is from the meekness & gentleness enjoined in the scriptures but I often have occasion to remark the forbearance of my friends & brethren & to rejoice in it.

The weather is beautiful again[,] I seemed to leave it behind me when I came south but I suppose from reports from Newcastle that there was a change there as well as here. Mr. Faraday desires his love to you & says his proposal still holds good to meet you at Old Buckenham² for we feel that expense must be considered & that I shall not be able therefore to accompany him. I regret this the less as Mrs. Vincent is cheering them with her visit & my sister Mrs. William Barnard³ is likely to spend a Sabbath⁴ there on her way from Cromer for I do long to see them all & feel that I am so much improved by our visit to the North that I should have enjoyed it much.

Will you thank Mary [Barnard] & Jane for their letters received yesterday – & with love to my Brother Sister⁵ and family believe me to remain

Yours very affectionately | S. Faraday
Mr. Vincent

1. That is the Sandemanian community in Newcastle which Vincent was visiting. DUA Acc M/409/5/3, p. 142.
2. Faraday did visit Old Buckenham from 23 to 25 August 1856. DUA Acc M/409/5/3, p. 142–3.
3. Martha Barnard, née Lyon (d.1871, age 72, GRO). Widow of William Barnard. Cantor (1991), 299. Made confession of faith in Sandemanian Church, 8 March 1829.
4. She visited Old Buckenham from 30 August to 4 September 1856. DUA Acc M/409/5/3, p. 143.
5. Elizabeth Reid née Barnard (1794–1870, GRO), who married the Newcastle silversmith David Reid (1792–1868, Reid, C.L. (1914)) in 1815.

Letter 3180

Joseph Antione Ferdinand Plateau to Faraday

14 August 1856

From the original in IET MS SC 2

Gand | 14 août 1856

Mon Cher Monsieur Faraday,

Je viens vous témoigner toute ma reconnaissance pour l'obligeance extrême avec laquelle vous avez bien voulu m'envoyer les renseignements relatifs au thaumatrope¹; ils sont précis, et me suffisent complètement; il ne me reste qu'à vous prier de nouveau de me pardonner l'embarras que je vous ai causé.

J'ai cependant à me plaindre de vous: je vois, par votre lettre, que vous avez été récemment à Paris; comment la bonne pensée ne vous est-elle pas venue de retourner en Angleterre par la Belgique, et de me faire une visite à Gand? J'aurais eu tant de plaisir à vous recevoir. Mais Hélas, quand les savants se décident à pénétrer en Belgique, c'est à Bruxelles qu'ils vont, et il est bien rare qu'ils passent par ici.

Veuillez accepter encore l'hommage d'une note que je viens de publier sur les théories récentes de la constitution des veines liquides², et dont j'ai l'honneur de vous adresser par la poste, trois exemplaires ayant les mêmes destinations que ceux de mon mémoire: Je vous ai engagé à ne pas lire ce dernier; mais je désirerois beaucoup que vous voulussiez bien lire ma note; elle n'est pas longue et cela vous prendra peu de temps. Vous vous rappelez sans doute que, par vos bons offices, la première Série de mes recherches sur les figures d'équilibre a été reproduite dans les *Scientific Memoirs*³ de M. Taylor⁴, mais qu'il n'en a pas été de même de la deuxième Série⁵, quoique vous ayez également bien voulu engager Mr. Taylor à la faire traduire. Il résulte de là que mes premières expériences sont aujourd'hui bien connues dans votre pays, tandis que celles de ma deuxième Série y sont probablement à peu-près ignorées, et qu'ainsi, par exemple, les physiciens anglais en sont toujours aux anciennes idées sur la théorie de la constitution des veines liquides; j'en ai acquis la preuve en lisant, dans le philosophical magazine de 1854⁶, le compte-rendu d'une leçon de Mr. Tyndall, ayant en partie les veines liquides pour objet et dans laquelle il n'a pas mentionné mes théories: or, si vous voulez

bien prendre connaissance de ma note actuelle, Si, après l'avoir lue, vous êtes convaincu, comme je l'espère, et si dans vos conversations avec les savants l'occasion se présente de parler de ce sujet, vous pourrez me rendre un grand service en contribuant efficacement à répandre mes idées et à dissiper ainsi des erreurs accréditées.

Ne répondez pas à cette lettre, je vous ai déjà fait perdre assez de temps, et je ne veux pas que vous en perdiez davantage à cause de moi.

Agréez, Mon Cher Monsieur Faraday, l'assurance de tous mes sentiments de respectueuse affection

Jh Plateau

P.S. J'apprends qu'il y a du mieux dans la situation de Mr. Regnault⁷, et tous les savants s'en féliciteront comme moi; vous êtes du reste, sans doute, au courant de son état.

TRANSLATION

Ghent | 14 Aug 1856

My Dear Mr. Faraday,

I come to show you all my gratitude for the extremely obliging way in which you kindly sent me all the information regarding the thaumatrope¹; it is precise, and completely sufficient; it only remains for me to beg you once again to forgive the trouble that I have caused you.

I have, however, a small complaint. I see, from your letter, that you were lately in Paris; why did it not occur to you to return to England via Belgium, and to visit me in Ghent? I would have had so much pleasure in receiving you. But alas, when savants decide to come to Belgium, it is to Brussels that they go, and it is rare for them to pass this way.

Please accept once more the homage of a note that I have just published on recent theories on the constitution of liquid veins², and of which I have the honour of sending you three copies for the same destinations as my paper: I advised you not to read the latter; but am most desirous that you kindly read my note; it is not long and it will take you little time. You probably remember that, by your good offices, the first Series of my research on the figures of equilibrium was reproduced in the *Scientific Memoirs*³ of Mr. Taylor⁴, but that that was not the case with the second series⁵, although did try to encourage Mr. Taylor to translate it. The result is that my first experiments are today well known in your country, whilst those of my second Series are probably almost unknown, and thus, for example, English physicists cling to old ideas on the theory of the constitution of liquid veins; I had proof of this while reading, in the *Philosophical Magazine* of 1854⁶, the report of Mr. Tyndall's lecture which dealt partly with liquid veins and in which he

did not mention my theories: now, if you would acquaint yourself with my present note, [and] if, after having read it, you are convinced, as I hope, and if in your conversations with other savants the opportunity should present itself to speak on this subject, you would do me a great service by contributing most effectively to disseminating my ideas and dissipating in this way recognised mistakes.

Do not answer this letter, I have already wasted enough of your time, and I do not want you to lose any more because of me.

Accept, My Dear Mr. Faraday, the assurance of all my sentiments of respectful affection.

Jh Plateau

P.S. I hear that there is an improvement in Mr. Regnault's situation⁷, and all savants will be as pleased as I am about this; but you are, no doubt, well informed of his state.

1. See letters 3174 and 3176.

2. Plateau (1856b).

3. Plateau (1844).

4. Richard Taylor (1781–1858, ODNB). Publisher and one of the editors of the *Philosophical Magazine*.

5. Plateau misremembered here as it was published as Plateau (1852).

6. Tyndall (1854a), Friday Evening Discourse of 19 May 1854.

7. See letter 3177.

Letter 3181

Faraday to Henry Bence Jones

19 August 1856

From the original in RI MS F1 D31

[Royal Institution embossed letterhead] | 19 Aug 1856

My dear Bence Jones,

Thank you heartily for a sight of Cahours note¹. I had heard just about as much of Regnault² & I suppose as the time increases so may our hopes. I half incline to hope you are off nevertheless I send the note back at once as you may want it. We are going to Hornsey next week: As the weather is, I am glad it is not this week for I think town is best. But I trust you will have or are having a prosperous journey and are away from under this cloud & rain. Health & happiness be with you[.]

Ever Yours | M. Faraday

1. Auguste André Thomas Cahours (1813–1891, DSB). French chemist.

2. See letter 3177.

Letter 3182**Faraday to Benjamin Vincent****20 August 1856****From the original in RI MS F1 D32**

Royal Institution | 20 Aug 1856

My dear friend,

I have delayed thus long that I might make as good a judgment as possible about the probability of my meeting you at Old Buckingham [sic]_[.] I think I can leave here on Friday by what train I cannot as yet say but hope to find out the Green by tea time if I can – & I think I must return on the Monday¹, how far these views may be modified I can<not> say for I have my [MS torn] & on next Wedn<esday> [MS torn] little house at <Hornsey>² [MS torn] <Jane>³ as you are aware is at Newcastle & therefore I am on extra duty. All friends here are pretty well and I hope to find you with Mrs. Vincent & the family as you would wish to be. Here things look out of order but all matters are progressing⁴. You have the news as I hear so often that I cannot tell you any thing – though I dare say when we come to talking there will be enough to say_[.]

Kindest remembrances to our friends from | Yours Ever Affectionately |
M. Faraday

Address: Mr. Vincent | care of Mr. W. Fisher | Old Buckenham | Attleboro |
Norfolk

1. Faraday visited Old Buckenham from Saturday 23 to Monday 25 August 1856. DUA Acc M/409/5/3, pp. 142–3.

2. This reading is suggested by the reference to Hornsey in letter 3181.

3. This reading is suggested on the grounds that Jane Barnard was in Newcastle at this time, see letter 3179.

4. This refers to various repairs to the building of the Royal Institution. See letter 3183.

Letter 3183**Faraday to John Barlow****20 August 1856****From the original in RI MS F1 D24**

Royal Institution | 20 Aug 1856

My dear Barlow,

I have just received your kind remembrance and intend if I can to catch you whilst you remain at Heidelberg by this letter. I hardly knew whereabouts you were & so was delighted with your letter as all was fresh to me_[.] We both hope that Mrs. Barlow and yourself for you wanted it have enjoyed & benefited by your journey thus far. By the bye are you on your way to Vienna to the Congress of Scientific Men there?¹ Tyndall set off some days ago for the Glaciers & Vienna & Dr. Bence Jones started this morning intending also

to reach the Austrian Capital. As you are at Heidelberg give my kindest remembrances to Professor Bunsen_[.] I have sent him a paper now & then though I had not the pleasure of his acquaintance but the Chevallier Bunsen² & I used often to talk of him and if you come across the former I hope you will convey the expression of my respect & remembrance to him. He has that of all men here.

There is a philosopher in Bunsens laboratory Matthiessen³ who sent us some specimens of Lithium Calcium &c I dare say you remember them – will you make kindest remembrances to him from me – I hope he goes on working – I cannot doubt it under such a master having himself such a mind. It is very pleasant to hear of Plucker & the rest you speak of. I was in Paris for 7 days going out of it to Fontainebleau from Saturday to Monday_[.] I saw so many kind friends there that I cannot remember them_[.] Dr. Andrews was there & we had the rare luck of seeing the Catacombs with a Prince Gortschakoff⁴ – there was not much in them except associations & a fine large stock of phosphate of lime in the form of 3,000,000 skulls skeletons &c_[.] A more interesting thing was the sight of an Aluminium operation in a reverberatory furnace – the Sodium & the Chloride of Aluminium &c which was used in the one operation being enough to fill more than a couple of Buckets – when the slag & the Aluminium was drawn off the latter appeared in portions among the formed [sic] like round flattened globules 2 inches in diameter. They were also preparing Sodium *three* retorts were sending forth their streams at once.

Dumas was very well and very kind so was also Biot Regnault Despretz, Le Verrier⁵ & all_[.] Poor Regnault have you heard of his accident – I was with him for 3 or 4 hours at Sevres & also at the Academy. Two days after he fell a height of not more than 9 feet – alighted on his feet but with such a concussion on the brain that when found he knew nobody & could see nothing he remained insensible for a long while & was despaired of_[.] However time has gone on & he is a little better & though far from being out of danger there is more hope of him than there was at first.

My wife is pretty well. Jane is at Newcastle. We go to a little house at Hornsey next Wednesday⁶. All the repairs⁷ & other matters go on well & steadily. The weather which had been very hot whilst I was in Paris is now rainy and we have even had heavy floods in the midland counties_[.] Good bye to you both for the present may you come back to us strong & rejoicing.

Ever Affectionately Yours | M. Faraday

1. That is the Gesellschaft Deutscher Naturforscher.

2. Christian Karl Josias Bunsen (1791–1860, ODNB). Prussian ambassador in London, 1841–1854.

3. Augustus Matthiessen (1831–1870, ODNB). Studied chemistry at Heidelberg and Royal College of Chemistry.

4. Alexander Mikhailovich, Prince Gorchakov (1798–1883, EB). Russian statesman, who was attending the Paris peace conference.

5. Urbain Jean Joseph Leverrier (1811–1877, DSB). Director of the Paris Observatory, 1854–1870.

6. That is 27 August 1856.

7. See RI MM, 7 July 1856, 11: 157.

Letter 3184

Faraday to Benjamin Vincent

26 August 1856

From the original in RI MS F1 D33

Royal Institution | 26 Aug 1856

My dear friend,

Though I had the longer journey¹ I am afraid you had the worst of it – for the rain came on[.] I hope you & Mr. Fisher have not caught cold through your kindness to me. I reached home at the expected time & found my wife pretty well but lonely & wanting me[.] I was very glad to get home to her though sorry to see so little of our friends with you. Pray express my kindest thanks for their hospitality & affection when the occasion serves. This morning we have letters from Mrs. William Barnard² – she still holds her intention of being with her children at Old Buckingham [sic] next Sabbath³[.] I dare say you will hear from her but cannot say from her letters what her time will be[.]

You will be much grieved to hear of a very sudden death at Newcastle of one whom I have no doubt you met Mr. William Reid⁴ – he with his brother Mr. Sandeman Reid⁵ & also Mr. Christian Reid⁶ were at the Whalley Sands beyond Cullercoats & he went into the sea to bathe he got out of his depth & was lost the others seeing it[.] Neither of them could swim & though Mr. Christian ran in up to his shoulders he could not reach him[.] His body was not found until 2 hours after⁷. This is another sad remembrance to add to that which you have already of the dangers of the coast there to bathers. It has a great & sudden gloom over many houses at Newcastle.

With my wifes love & remembrance to You & Mrs. Vincent.

I am My dear friend | Yours Very Truly | M. Faraday

Mr. Vincent

Address: Mr. Vincent | care of Mr. W. Fisher | Old Buckenham Green |
Attleboro | Norfolk

1. From Old Buckenham which he had visited from Saturday 23 to Monday 25 August 1856. DUA Acc M/409/5/3, pp. 142–3.

2. Martha Barnard, née Lyon (d.1871, age 72, GRO). Widow of William Barnard. Cantor (1991), 299. Made confession of faith in Sandemanian Church, 8 March 1829.

3. That is 31 August 1856. She visited Old Buckenham from 30 August to 4 September 1856. DUA Acc M/409/5/3, p. 143.

4. William Reid (1825–1856, Reid, C.L. (1914)). Bookkeeper to the Reid silversmith business. He drowned on 24 August 1856.

5. Patrick Sandeman Reid (1819–1897, Reid, C.L. (1914)). Civil and mining engineer, TNA RG12/1049, f. 84, p. 16.
6. Christian John Reid (1816–1891, Reid, C.L. (1914)). Newcastle silversmith.
7. For an account of the accident see *Newcastle Guardian*, 30 August 1856, p. 5, col. e.

Letter 3185

John Tyndall to Faraday

27 and 28 August 1856

From the typescript in RI MS JT TS Volume 12, pp. 4052–9

27 Aug 1856

My dear Mr. Faraday,

I have bidden Huxley goodbye and watched him descending the slopes of the Furca till his gray coat was lost amongst the gray rocks. I am now all alone in a snug little pen, which may perhaps be justly degnified by the name of bedroom; and my thoughts find refuge in writing to you. I left London on the 12th, and had pleasant intelligent companions to Dieppe. I was not sick in the passage, but came in sight at least of sickness. I reached Paris a little after midnight, had my luggage examined, the act being facilitated by promptness and civility on my part towards the officials. Took a voiture, and while I rumbled through the forsaken streets amused myself by watching the meteors vaulting through the sky. Had three hours bed, but no sleep at the hotel de Strasbourg, and next morning at 6 o'clock was on the road again. Part of the way an intelligent Parisian was my companion, and from him I learned that Regnault was better, but that great anxiety was still felt regarding him. Since then I have heard nothing regarding Regnault. Put up at the Maison Rouge in Strasbourg. I dreamt that I was in a besieged fortress, with the cannon rattling about my ears. As my consciousness became clarified I found I had transformed the knocking of the "boots" into the explosion of cannon. The man had already become irascible on account of my want of attention. this was at half past 3 o'clock in the morning and a little after 4 I started for Basle, hoping almost against hope to catch Huxley and Hooker there. I arrived just in time to see Huxley in a corner of the diligence, to apprise him, by a thump on the shoulder of my arrival, to take my seat in an extra omnibus and set off to the railway station. At Berne we were joined by Dr. Hooker and a party, and then we went on to Thun. From Thun by steamer to Interlaken, the weather being magnificent. On the day of our arrival, we took donkeys, one for the luggage, and the other to carry Mrs. Huxley¹, and reached the Jungfrau hotel upon the Wengern Alp. Here we spent the night and next day we examined a glacier formed by the avelanches which thunder down the sides of the Jungfrau. We derived much instruction regarding both glaciers and avelanches. Though many prophets of evil told us we should have bad weather we ventured forward to Grindelwald, and as far as the weather is concerned nothing could be more glorious_[.] Drifting cloud masses smote the

sides of the Jungfrau. Along the slopes of the Alp the black pine crowded sometimes into forests, and again dispersed in clusters, was interleaved with patches of the most lively green; the sun poured his glory over all. Right before us a rainbow planted one end of its curve in the valley and leaning over the mountain summits to the right, seemed to clasp the savage crags of the Wetterhorn in its embrace. We reached Grindelwald just in time to secure bedrooms at the Bear Hotel.

Next day we engaged a guide and ascended the slope which bounds the glacier. It was most beautiful, and most instructive; the snowy minarets shining in the sun and relieved against the black slope of the opposite mountain, were undescribably beautiful. But there was a scientific pleasure superadded. We examined the conformation of the glacier, the nature and direction of the fissions, the varying inclinations of the surface, and after a little time a mechanical problem of great beauty revealed itself. The structure and modifications of the entire mass were such as might be predicted *a priori* from mechanical laws. A certain relationship was thus established between our own minds and the mass before us, which rendered our subsequent journey upon it pleasanter. I will not bother you with this further now, as I hope to be able to make a Friday evening out of the subject². We ascended to what is called the *Eis mer* "mer de glace" in some places the breaking up of the mass into chasms, rendered our progress difficult and not quite without danger. In one place we had to cross an edge 10 inches wide, with a chasm 60 feet deep yawning at each side of us: but our progress since has caused us to think little of this. In some places deep shafts are formed into which the surface water of the glacier tumble with the noise of thunder. One of these shafts we found by letting a stone fall to be at least 400 feet deep. High above us on the mountain slope was a flat mass of brown crag, surmounted by a crown of ice cliff. Summer or winter on this rock no snow rests; and it is thence called the *Heisseplatte*. (the hot slab). Down this while we were on the glacier at least a dozen avalanches darted downwards: no wonder that the noise of these equal the sound of thunder. They are composed in part of solid blocks of ice: one of these blocks which we measured, and which was cast to a distance of 1000 yards weighed at least 5 tons. We derived great instruction from this day's journey, and I trust we shall be able to put certain glacial phenomena in an entirely new point of view.

Next day Mrs. Huxley was ill, and Huxley had to return with her to Interlaken. I was joined by Hooker and we again ascended the glacier together. We followed one great tributary of the Grindelwald glacier as far as our guide (a powerful dark looking Swiss) dared to take us. When further progress on the ice was impossible we ascended the steep slope of a mountain side to a height sufficient to give us a view of the entire glacier. The dirt bands of Prof Forbes³ were here strikingly manifest; but it did not appear quite so manifest that he had solved them. I will reserve all further expression of

opinion until my thoughts are riper on the subject. While on the mountain the rain came down in torrents, and we returned to Grindelwald well drenched. I went to bed until my trousers (for I had not a second pair) were dried.

Next morning I left Grindelwald, and had a glorious excursion over the Sheideck to Meyringen, on the following day I walked to the Grimsel, and saw the Handeck falls to great perfection. The Aar had a bountiful supply of water; and the Aerlen, a river which darts down a precipice to the left, was also well supplied. The Aar plunges into a chasm 200 feet deep. The smaller river first falls upon a rock ledge and rebounding from this darts at the Aar, and both plunge together like a pair of fighting demons to the bottom of the gorge.^[1] The spray produced a vivid rainbow; which was beaten about hither and thither, as the water smoke shifted its position.

A day has flown by since the last word was written, and I have left 12 hours of hard walking, during seven of which I carried my heavy pack, behind me, I am now in a little town in the Canton of Graubünden, surrounded by Roman catholics. As I write the bells of a large abbey are summoning the Benedictine monks to their duties; but I must quit the present and recur the past. I reached the Grimsel and found Huxley there before me. His wife became better, and he came straight from Interlaken to Meyringen, and thence to the Grimsel. I had read previous to leaving London that a certain M. Dollfus⁴ of Mielhausen⁵ had built a hut by the side of the glacier of the Aar, for the purpose of making observations upon the motion of the mass. All furniture had been removed from the hut, but there was still a litter of old hay there, and this Huxley and I proposed to make our bed, and thus enable ourselves to go to the very summit of the glacier, which would be impossible in one day from the Grimsel. We had some blankets packed together; a couple of stout fellows carried up some food and firewood, and on the evening of the 22nd, after a hard day's work, during which I visited the mass of rock, which in former years sheltered Agassiz, we returned to our hut. After cold mutton, sour bread and a glass of beer we betook ourselves to our hay and spent the night, if not comfortably, at least philosophically. Next morning at day dawn we found the snow falling heavily. We waited until noon, and finding it still persistant, and deriving no comfort from the predictions of our guide, we resolved to return – Made on our way as many observations as the fresh fallen snow permitted us to make. As we descended the snow flakes became smaller and finally merged into a heavy rain which drenched both of us. Notwithstanding this defeat we found our visit to the glacier extremely instructive, the structure of the ice, the formation of the moraines, the great blocks of white granite which came slowly sailing down the glacier – all were objects of the highest interest. To form the lower glacier two others combine, and their confluence reminds me strikingly of the union of two rivers. Indeed the idea of viscosity, or plasticity, or semi fluidity, is that which must first suggest itself to any reflective mind. Forbes' comparison with an overturned

pail of mortar is in many cases admirable⁶; but whether the comparison will stand philosophic analysis is quite another question. I believe myself that your old experiments will help us greatly here⁷.

Next morning we paid our bill in good time at the Grimsel and sallied forth towards the glacier of the Rhone. I sought for a cloud in heaven as we started, but could not find one. Clouds and vapours had all swept themselves away during the preceding night, and we had the unspotted firmament above us. Two hours brought us to a point from which we had a capital view of the lower portion of the glacier of the Rhone. Our view upwards was bounded by ice cliffs, and beyond these it was our purpose to penetrate towards the origin of the glacier. Having procured some food at the Auberge and a bottle of wine we set out, conducted by our athletic guide. Having examined the lower portion of the glacier, and observed some remarkable and beautiful phenomena, we crossed, and ascending a mountain at the opposite side, we came right over the upper portion of the glacier: Here on the crags we had our frugal dinner, in a manner which made the pomp of emperors poor. A scene of indescribable magnificence opened before us. Right in front was the mighty mass of the Finster Aar horn, further towards the horizon was the grand peak of the Weisshorn, more to the left we had the snowy summits of Monte Rosa by the side of which the cone of the Matterhorn (Mont Cervin) rose like a black savage tattooed with streaks of snow, Still further to the left the chain of the Furca, with shoulders of snow as smooth as chiselled carrara marble, completed the picture; and over all this the glorious sun poured his undimmed radiance. It was a scene calculated to stir the heart of man, and to carve for itself an everlasting resting place in his memory. We went upon the glacier and tramped for hours upwards over the newly fallen snow. We learned much, and our bump of caution was in perpetual action, for the crevasses were terrific. As we ascended, the blue of the sky became deeper: and at the highest point there was something awful in the blueblackness it assumed. Once the sun got above a thin veil of frozen particles, and we had a splendid ring like a circular rainbow round him. The veil gradually melted away to the westward, and we had simply the eastern semicircle: by degrees this also melted away and the circle wholly disappeared. As we returned the shadow of the Finster Aar horn caused the vapours to curdle up, and to flow with great velocity into the valley of the Rhone. Here however the sun still shone, and the vapours were licked up as fast as they came: hence though the supply from behind was incessant, the cloud river made no progress. Huxley accompanied me to the Furca, at the summit of which stands a little inn. The valley was full of vapours when we arrived, and standing on a mountain ridge, with the sun behind us each of us suddenly observed his head surrounded by a coloured halo, and his shadow projected on the vapour mass in front. We raised our hands, the gigantic spectre before us did the same, and imitated all our other actions – we had in fact “the spirit of the

Brocken" in all its splendour. We closed this day of wonders over a chop and a bottle of wine in the Furca Hotel, whence I descended to see Huxley on his way to the Rhone Auberge. The marmots were piping on the rocks as we bade each other good bye, and I was soon on my way back to my lonely lodging.

One's pleasure is sometimes marred by the cupidity of the Swiss guides: but on this I will not dwell at present, I bought a salad spoon and fork at Grindelwald for Mrs. Faraday but cannot undertake to deliver it to her safely. To her and to Miss Barnard pray present my kind remembrances. I am now at Feldkirch having just crossed the Austrian frontier. Tomorrow morning at 4 o'clock. I start for Landeck. Mean while I will post this. Would you have the kindness to say to Anderson that I received my letters safely at Chur. Would you add to this kindness by handing him the enclosed letters and asking him to have them prepaid and posted to the gentlemen to whom they are addressed? Two of them require envelopes and postage stamps, I had nearly forgotten an important point. Knowing that Mr. Addams⁸ formerly possessed some acoustic apparatus and thinking that he might be willing to dispose of it cheaply, I called upon him previous to leaving London but he was absent. I wrote to him but his people did not forward my letter. Last night at Chur I had a note from him saying that he had not disposed of his ap[pl]aratus. Would it not be worth while to see it, and ascertain whether it, or part of it could not on account of being second hand be more cheaply purchased than new instruments at Paris. I should be greatly obliged by your advice upon this point. If you would write to me *Poste Restante Vienna*, I should know how to act there and in Paris on my return. I shall not reach Vienna until the 16th of September. I would thank you to ask Anderson to direct my letters there up to the 15th of September. The meeting⁹ will continue 6 days and that will afford sufficient time for all letters posted in London up to the 15th to reach me. And now I must beg of you to excuse all the trouble which I so impudently lay upon your shoulders, to wish you patience through this tedious ill written letter and to believe me

ever most faithfully Yours | John Tyndall

1. Henrietta Anne Huxley, née Heathorn (1825–1914, ODNB under T.H. Huxley). Married T.H. Huxley in 1855.

2. Tyndall (1857), Friday Evening Discourse of 23 January 1857.

3. Forbes (1842), 348–52.

4. Daniel Dollfus (1797–1870, DHBS). Geologist who worked on glaciers.

5. That is Mulhouse.

6. See Forbes (1843), 378–82.

7. See *Athenaeum*, 15 June 1850, pp. 640–1 for an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'.

8. Robert Addams (d.1875, age 85, GRO). Scientific instrument maker and lecturer.

9. Of the Gesellschaft Deutscher Naturforscher.

Letter 3186**John Tyndall to Faraday****11 September 1856****From the typescript in RI MS JT TS Volume 12, pp. 4060–6**

Innsbruck | 11 Sep 1856

My dear Mr. Faraday,

I believe my last letter¹ was posted to you at Feldkirch. Yesterday I reached this place and had *one* letter from the post office. It was from Magnus, dated August 17th, and had reference to a plan of a journey into which I had already plunged. He wanted to arrange matters so that we might meet somewhere but this will now be impossible. On crossing the frontier I was happy to find better people and better German. From Feldkirch I proceeded to Landeck: the first instance of the piety of the Tyrolese struck me here, on passing the kitchen of the hotel I heard a confused murmur of voices and looking in saw the whole household upon their knees praying aloud before going to bed. From a conversation with some guests I learned that the nearest glaciers were in Kaimserthal and up this valley I wandered next day. In the valley is a place of pilgrimage called Kaltebrunnen which is much resorted to by the devout. Near the chapel is a figure of Christ with a tube in his side from which is projected a stream of limpid water. The walls of the chapel are covered with offerings to the virgin, for here it is said she loves especially to dwell, and to shower her benefits upon her adorers. Tablets containing rude drawings of men and women in all possible positions of peril are hung along the walls; some half crushed under fallen rocks, others falling into rivers, women with carts overturned tumbling down precipices; most however were of men on mountain slopes lying under fallen pine trees. At the bottom of these tablets it was sometimes coolly stated that the person represented was saved by the manifest and immediate interposition of the Mother of God at Kaltebrunnen. I like the piety of the people, but cannot say that this form of it pleased me. Onward to the highest inhabited part of the valley and here I engaged a chamois hunter to be my guide on the glaciers for two days. About 10 miles higher up was the Gebatsch Alp to which the dwellers in the lower part of the valley send their herds in summer; and in a cowhouse on the Alp I proposed to take up my nights quarters, There were two huts, one of which was occupied by 4 herds, who milked the cows and made cheese and butter. A fine tall fellow with a wild countenance, his stockings, without feet, being drawn upon his legs, leaving a black zone of dirty skin between the upper rim of his stockings and the lower extremity of his breeches, asked me what he could cook for me. I asked for some milk and attacked the fare of the place, viz brown bread a month old, with the relish of hunger. The milk was rich and sweet, and I fared sumptuously. Went with my guide upon the glacier afterwards and remained upon it for several hours. The mass of ice was all in gray shadow, and a fringe of dying sunshine lay all along the opposite mountain summits as we finished our day's work. On coming again to the chalet, my

cook asked me what I wished to eat: my reply was “boiled eggs”. The four herds were there, all brownly sunburnt, the brown deepening into black with beard and dirt. The grouping of these men round the pine fire, which gave us our only light, was sometimes extremely picturesque. All of them smoked, and now and then one of them took a blazing torch from the fire to light his pipe, thereby casting a ruddy glare upon his wild face. A quantity of flour was placed in a wooden dish; milk was gradually added and the mass stirred up, all being done by the gleam of the pine logs. A saucepan was taken and some eggs were broken into it, sometimes the yolk clung to the shell and on such occasions the cook, who was the chief of the establishment, scooped out the contents with his finger, shaking dexterously the portions which attached themselves to the latter into the general mass. I had seen his fingers before and only dreaded that through a misunderstanding of my request the eggs were intended for me. A small broom of stiff twigs was taken from two pegs which supported it, and with this the eggs were pricked and whipped into a liquid, which, to my unspeakable satisfaction, was poured into the wooden dish already referred to. Meanwhile my eggs were selected and boiled – two of them were rotten – I could not help admiring at times the fine straight figure of the cook as he stood erect in the firelight and talked in the intervals of his vocation with those around him. The fire was made on a platform of stone elevated three feet above the floor, there was no chimney but the smoke found ample egress through the fissions in the sides and roof. At one end of the chalet was a hook which turned a small wheel, which imparted an up and down motion to a churn dash, which in its turn again made the butter. The beams and rafters were covered cheeses drying in the warm wood smoke. The men had their supper and were the images of tranquil content afterwards. They all gathered round the fire, lighted their pipes and talked, with the gravity of philosophers, about those things which concerned them. Finding my interest alive as to his manner of making cheese the cook took me into his stores and explained the process to me. In one room were three gigantic masses of butter, and I amused my instructor very much by calling them *butter glaciers*[.]

As I have said there was a cowhouse near the chalet and above the ground floor a kind of cockloft was supported by pillars of pine, on the loft hay was strewn, and we reached it from the outside by means of a broken ladder. My guide shook up the hay and broke its nodules, piling up an eminence for my head. I lay down in my clothes with my scotch plaid as a blanket: my valet said I should find my feet cold before the morning, and to prevent this piled hay upon me up to the hips. He suggested the propriety of continuing the process till my *head* was covered, but this I declined. Before morning however the biting coldness of the air which, sometimes blew in upon us proved that there was some wisdom in the suggestion[.] Having set me right my chamois hunter prepared a place for himself, and soon his heavy breathing told me that he was in a state of blessedness that I could only envy. During the long

hours of the night I found a melancholy amusement in watching the keen gleaming stars travelling across the apertures in the roof above me. Once as I opened my eye the Pleiades were there, twinkling in beauty: I *strove* to admire them, but an hour's sleep would have been worth a score of constellations. Sometimes I did approach a doze, which, when on the verge of deepening into slumber, was rudely broken by the clamour of an unamiable group of pigs which occupied the ground floor of our dwelling. The commotion of these animals usually commenced by a small grunt; this, like the first rattle of a pebble which announces the incipient motions of an avalanche, ascended to a grumble, and broke out at length in vociferous and angry expostulation. Rose at 5 o'clock greatly unrefreshed by my night's attempt at sleeping, still strong enough to bear the fatigues of the coming day.

Spent five hours upon the glaciers of Gebatsch and learned a good deal: saw some marmots scampering over the rocks: returned to our hut, fortified ourselves, as far as it was possible to do with bread and milk, and faced the mountain, the summit of which we had to cross to reach Launtaufer's Thal. After a rough ascent through the mossy pine clad Alp we came to the bare rocks. The weather has made havoc with the mountains broken them up into ruinous masses and showered them down the slopes. Among these we picked our way. Reached the end of a steep glacier whose surface was coated with sloppy snow; had this been frozen it would have been impossible to ascend. Once indeed I found my footing insecure and knowing by experience that if I fell so as to sit upon the ice I should be shot like an avalanche to the bottom, I took hold of the hand of my guide, to check this motion in case I slipped. He was a fearless man, far more reckless than the Swiss guides and not at all accustomed to act the cautious part of such. Whenever I made a proposal regarding the attainment of a perilous point, the word "impossible", which I often heard in Switzerland, appeared not to be in his vocabulary. "I don't know" was his usual reply "it will certainly be dangerous but we can try it". Near the summit of the slope up which we now toiled was an islet of stones and debris upon the glacier. Here we sat down to rest; right in front of us, and far above all vegetable life, surrounded only by bare sharp crags and dazzling snow, stood a beautiful chamois watching all our motions. The association of animal life and beauty with dead savagery around was exceedingly striking. Onward again for a time over a gentle ascent, till we reached the base of an ice slope steeper than any we had yet encountered. The chamois hunter faced it with confidence, though I should have regarded the ascent as impossible. I plodded after him through the snow slush fitting my feet into the tracks which he left behind him, almost afraid to look upwards or downwards. After a desperate struggle, which was felt as much by my guide as by myself, we reached the narrow row of crags which formed the summit and looked into the world of mountains beyond. The keen breeze here smote us but it put new vigour in our muscles and by jumping springing and sliding we descended through the mass of debris which here forms the mountain side. We reached

the glacial ice once more. The mass had melted away from the mountain side which rose perpendicular to our right; and to our left an ice wall rose to a height of 50 or 60 feet_[.] Through the gorge these formed we walked. From what I had seen of other glaciers I inferred that the mass on which we trod merely formed the roof of a cavern underneath; and I soon found that this conjection was right; for on turning an angle of rock my guide muttered with an expression of surprise and concern upon his countenance – “I did not expect this.” The ice had wholly melted in one place thus revealing a green tarn of unknown depth between the ice and mountain side. My guide inspected the place and desiring me to stand still, crept like one of his own marmots along the ice slope above the tarn. I endeavoured to round it by clambering over the rocks, but my guide warned me back. Returning to me he said “we must try it together” “good” I replied “only do you take hold of this” handing him my plaid. Thus prepared for the cold plunge bath I expected I took his hand, and his glacier stick. Striking the latter into the ice I disposed of a portion of my weight and following cautiously reached the land safely_[.] The sketch² will give you some idea of the gorge in section we first walked along the roof a until we reached a place where the ice from a to b had melted away, and the cavern c was exposed. We had to edge our way along the slope above a and this, contrary to my expectations, we accomplished in safety. I met Frankland next day. He is now beside me and desires his kind remembrances. I think I must tell you the rest of my adventures by word of mouth. Kind remembrances to Mrs. Faraday and Miss Barnard. good bye –

As ever Yours | John Tyndall

1. Letter 3185.

2. Not in typescript.

Letter 3187

Thomas Henry Farrer to Faraday

12 September 1856

From the original press copy in TNA MT4/24, p. 4607

12 Sep 1856

8145

11th August¹, stating the results of your experiments with Mr. Gordon's Reflector; and to give you the thanks of their Lordships for the trouble you have taken; and for the valuable Report you have made_[.]

I have the honor to be | Sir, Your obedient Servant | T.H. Farrer
Professor Faraday F.R.S. | Royal Institution

1. Not found, but evidently Faraday's response to letter 3165.

Letter 3188**Thomas Henry Farrer to Faraday****13 September 1856****From the original press copy in TNA MT4/24, p. 4624**

13 Sep 1856

6920

With reference to your Report dated 26th May last¹, upon Dr. Normandy's Patent Marine Aerated Fresh Water Apparatus², I am directed by the Lords of the Committee of Privy Council for Trade to transmit to you the accompanying Copy of a Letter from Mr. Alexander Gordon, Civil Engineer, on the subject of this Invention, and of another Apparatus for distilling Sea Water by means of the Sun's rays through the Agency of a Brass Reflector or "Thermheliostat".

My Lords desire me to express their hope that you will be able to favor them with you opinion as to the reflective merits of these two Inventions, more especially with reference to their Applicability to Lighthouses.

Any further information on the subject of Mr. Gordon's Apparatus shall be procured and forwarded, if desired_[.]

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
M. Faraday Esqre. F.R.S. | &c &c &c | 21 Albemarle Street

1. Not found, but evidently Faraday's response to letter 3146.

2. See letter 3136.

Letter 3189**Faraday to John Tyndall****15 September 1856****From the typescript in RI MS JT TS Volume 12, p. 4141**

Hornsey | 15 Sep 1856

My dear Tyndall,

It is too bad of me to think of putting you off with a short letter, and yet I think I must do so, to save the post, for the 15th has come most unawares on me¹. Being here in the country for my wife's benefit, the dates are beguiled away from me. Your letter was most welcome, and many have enjoyed it. I return you our most sincere thanks for the great favour. On our part we have no events to describe; we are rustivating. I try to experiment and write, but I forget so fast that I have the pleasure of doing both over two or three times. Never mind. A time for all things².

I called and saw Mr. Addams³ the other day. I found him disinclined to separate his acoustical apparatus one part from another. He has told you, I believe, that the price of the whole would be £80. I have not seen them, and indeed if I had, I could not have formed any judgment, for I do not know

the present state of acoustical apparatus and should be unable to make any comparison with that which exists elsewhere, or in the collection of [blank in TS.]⁴ He said he would send me a bit of them, but I have not received it.

I suppose before you receive this you will meet with Dr. Bence Jones. If so, do not let him forget me. Very kindest thoughts to both him and you from all of us here. My wife is really better for the country, and we have had very fine weather. As to the house in Albemarle St., Anderson is well, Mr. Vincent is well, Miss Savage⁵ is somewhere in France, and has sent a letter, but no address. I do not know whether you expect to meet with any of our German friends at Vienna⁶. Schrotter⁷ remember me to, and the others if you see them. I think you told me something about Magnus and the transparency of gold [blank in TS]⁸. If so, tell him it works on pretty well – very curiously in my mind – but as yet with no broad distinct result which will strike scientific men, though I hope it may help the working of some.

Your account of your wanderings in the Oberland and also by Strasburg Bale, &c &c &c was most delightful to us, having been over the same ground. We also were stopped at the Grimsel glacier, by illness⁹. We had agreed to meet Agassiz, and could not go up to him, and so it goes in intentions and acts: not always well accordant, but let us hope well[.]

I must not [sic] stop | Ever, dear Tyndall | Yours very truly |
M. Faraday

Address: Dr. Tyndall | Post Restante | Vienna | Austria

1. The latest date, given by Tyndall in letter 3185, by which letters posted to him would reach him in Vienna.

2. William Shakespeare, *The Comedy of Errors*, II, 2, 65.

3. Robert Addams (d.1875, age 85, GRO). Scientific instrument maker and lecturer.

4. A pencil annotation in the margin here says 'lecturers?'.

5. Sarah Savage (d.1865, age 57, GRO). Housekeeper of the Royal Institution, 1835–1865. (RI MM, 19 July 1835, 8: 363–4 and 6 March 1865, 12: 97).

6. At the meeting of the Gesellschaft Deutscher Naturforscher.

7. Anton Schrötter (1802–1875, ADB). Secretary of the Imperial Academy of Sciences in Vienna from 1850.

8. A pencil annotation in the margin here says 'paint?'.

9. In August 1841. See Bence Jones (1870a), 2: 156.

Letter 3190

Justus Liebig to Faraday

17 September 1856

From the original in IET MS SC 2

Munich | 17 Sep 1856

My dear Faraday,

It is so long ago that I received your kind letter of the 1 Mai¹ that I am quite ashamed to answer it². I always thought that I could communicate to

you some news of interest, but I was unfortunate enough to have nothing worthy of Faraday! I occupied myself in the last 5 months with agriculture and what is worse with a controversy in agriculture of the most absurd kind. The truth of certain natural laws which some have done me the unmerited honour to call my theory, has been attacked by so called practical men and the questions involved in this controversy seemed to me of such importance for the material progress of nations and for the welfare and prosperity of millions that I determined to enter the lists in defense of them. If you take the trouble to read my paper in the Journal of the Roy. agric. Society No xxxvii Vol xvii Pt. 1³, you will understand all what I have to say. My dear friend, my disappointment is very great of being obliged to defend things so simple! By that controversy I have lost all my confidence of the possibility of improving agriculture by teaching scientific principles. What I taught during 15 years has had no effect whatever, it has taken no root. What a singular being is the mind of man! The walking tables, mesmerism and similar nonsense attracts the attention of thousands and the most simple and important truths find their enemy's and opponents and always successfull in opposing them! There was not in England and not in Germany a single man who did take the trouble to signalise the open errors, mistakes and misrepresentations of Mr. Lawes and Gilbert⁴ about my views!⁵ the unknown force acting in the moving tables found hundreds of zealous defensors! I can not find the key for that! You are in this respect in a much better situation; no person believes in your department to understand the questions better than yourself but there is no ignorant medical man or no farmer who does not believe to understand medical or agricultural questions better than the natural philosopher who has thoroughly investigated them! I am dominated by the desire to establish a school of practical farming for the education of teachers of practical Agriculture. It seems to me that there is no other way of showing the application of Scientific principles. It must be done in a large Scale and I am confident to Succeed. I think I could do for Agriculture what I have done 30 years ago for the practical education of experimental chemists. All my friends tell me that it is a folly to give up the most brilliant position which a man of Science has ever held, but I am tired of lecturing; there are so many others which would do it quite as well; I am sick of my Schoolmastership and all my happiness depends to get rid of it. I have to regret that my friends in England hastened 3 years ago to[o] much to give me in that testimonial a signe of acknowledgment⁶. If this matter would have been brought before Parliament by the Duke of Argyll⁷ – perhaps the english nation would have voted for me a pension which might have given me full liberty to resign my professorship. By this supply I should be in position to spend 3–4 months in Scotland or England and to devote all my powers to agricultural questions. The royal agric. Society has to dispose of large sums annually and a great deal of good could be done by it. But all that is to[o] late!

Mr. Barnard tells me that he is returning next month to England and to remain there; I am sorry that he is leaving us. We like him all very much; he is such a good natured, honest young man, openhearted and true! he devoted every minute of the day to his studies and I am glad to hear him say that the Stay at Munich was really useful to him. I wish very much that he might direct his attention to introduce in England the Painting with soluble Glas, of which Mr. Barlow has given an interesting lecture⁸. Mr. Barnard has all the facility of learning the little secrets of our celebrated Kaulbach⁹ to which I introduced him and I think it would give him a start in London. It is something new and most analogous to his painting in water colours or aquarell.

My wife¹⁰ begs to unite with me in kind regards to Mrs. Faraday and your nieces. It is my warmest wish to see you and your Lady in Munich and to show you our beautiful mountains. You could repose yourself and enjoy the most solitary life.

Believe me dear Faraday | yours very truly | Justus Liebig

1. Letter 3134.
2. Clearly Liebig had forgotten that he had already replied in letter 3170.
3. Liebig (1856).
4. Joseph Henry Gilbert (1817–1901, ODNB). Experimentalist at Rothampstead agricultural station.
5. Lawes and Gilbert (1856). On this see Brock (1997), 173–6.
6. On this see Brock (1997), 142–3.
7. George Douglas Campbell, 8th Duke of Argyll (1823–1900, ODNB). Postmaster General, 1855–1858.
8. Barlow (1854), Friday Evening Discourse of 7 April 1854.
9. Wilhelm Kaulbach (1805–1874, NDB) German historical painter.
10. Henriette Liebig, née Moldenhauer (1807–1881, Brock (1997), 44). Married Justus von Liebig in 1826.

Letter 3191

Christian Friedrich Schoenbein to Faraday

20 September 1856

From the original in UB MS NS 425

Bâle | 20 Sep 1856

My dear Faraday,

Are you still alive or have you entirely forgotten your friend on the Rhine? It is indeed an age since I have seen a line from you¹ and I think it is time to break your long silence. To induce you to do so I send you this letter conjointly with a paper of mine, which I desire very much that you should acquaint yourself with its contents. It treats of a matter being, as I believe, full of interest i.e. of the connexion that, to my opinion, exists between allotropic and catalytic phenomena².

During our midsummer vacations I took a trip into the north of Germany, to me a "Terra incognita", rambled about in Holstein, visited Hamburg and Berlin, saw many scientific and other friends, made new ones, paid my respects to the Senior of the European philosophers at Potsdam in the Royal Castle, had a very interesting and long conversation with that eminent old man³, touched a little the Thuringian Forest, mounted the Wartburg, where the great Reformer Luther⁴ fought against the Devil, passed a couple of agreeable days at Frankfurt, returned home highly satisfied with what I call my "North-pole expedition" and met my family in good health. Before I set out to my journey I had worked a good deal and have done so ever since my return, not quite for nothing I trust for I have succeeded in finding out a number of novel "phenomena of contact"⁵ which I hope will add if not much, at least something to our stock of knowledge regarding the Chemistry of Oxygen.

I have already drawn up a voluminous memoir, in which the results of my experiments are described and knowing that you take some interest in this kind of researches I am very sorry to be prevented (by the smallness of the space allowed to a letter) from entering into details about my late doings; but to give you at least a slight Idea of the nature of those researches let me tell you that they refer to what they call catalytic actions so far as these concern oxidation. One of the principal results obtained is the fact that in a number of cases two substances "toto coelo"⁶ differing from each other as to their chemical nature: Platinum and the red globules of the blood – produce the same effects i.e. determining oxidizing actions, which either would not take place at all or but very slowly without the presence of the substances named and some others⁷. I need not point out to you the probable importance of such a remarkable fact to physiology⁸.

Another fact not quite void of scientific interest is this, that in some instances I can show, as it were, steps which the oxidation of certain matters passes: first ozonisation of inactive oxygen, then a sort of loose combination of that ozonised oxygen with the oxidable substance and finally actual oxidation of the latter. I have reason to believe that on looking a little closer into that matter, we shall discover a great number of similar cases and it is not impossible that any oxidation is a sort of chemical drama, consisting of different acts, the last of which is real oxidation. Shakespeare says that there are many things, between heaven and earth which the philosophers do even not dream of⁹ and Schoenbein maintaining that between the moment on which two isolated elementary bodies meet and that of their chemical associating being finished there lies a whole world of phenomena and is very much of which the Chemists of the present day have as yet not the slightest notion. There is even within inorganic Chemistry something which I might call Physiology and the most interesting and truly scientific object of chemical research lies to my opinion within the

short interval of time alluded to and hence the great difficulty of such an investigation.

Less interesting but pretty enough is a third fact which I must mention to you, namely that out of free ozonised oxygen and olefiant gas formic acid is readily and directly formed, a result easily accountable by the chemical equation $C_4H_4 + 8O = 2C_2H_2O_4$. But now enough of Chemistry and Oxygen.

If you should happen to have a friend in the country being blessed with girls and desirous to receive for a time in his family a grown up girl pretty well versed in the german, french and english litterature, being a tolerably good musician, carefully educated and of an excellent moral character, I know one, whom I should venture to recommend. I must however not omit to tell you that the girl in question is very far from wishing to become a paid governess, she desires to be considered as a friend and member of the family and make herself at the same time as useful as possible in the education of the children. That girl is my own eldest daughter¹⁰ who is very anxious to pass six or twelve months in an English family. I do not much relish those wishes of her's for I love her too tenderly as readily to allow her going to a foreign country, but if it be possible to place her in a good family I shall not prevent her from crossing the Channel. Pray let me know, what you think about the plan of my adventurous, silly sweet girl.

Mr. Wiedemann charges me to present to you his best compliments, he is at this present moment actively engaged in magnetic researches, which seem to lead to interesting results¹¹.

My friend Mr. Merian and his wife were highly gratified with the friendly reception they met with at the Royal Institution and send the kindest remembrances to its amiable Master and Mistress.

In closing my letter I ask you the favor to remember me most friendly to Mrs. Faraday and tell her that Mr. Schoenbein had not yet entirely given up his hopes of seeing once more her Ladyship and her Lord on this side of the water.

Believe me my dear Faraday | Your's | most truly | C.F. Schoenbein

Pray be kind enough as to send the inclosed paper to Dr. Whewell as soon as you can.

1. See letter 3120.
2. Schoenbein (1856c).
3. That is Humboldt.
4. Martin Luther (1483–1546, NDB). Theological reformer.
5. Schoenbein (1857a).
6. 'Entirely'.
7. See Schoenbein (1857b).
8. Schoenbein (1856d).
9. See William Shakespeare, *Hamlet*, I, 5, 166.
10. Emilie Schoenbein.
11. See Wiedemann (1857).

Letter 3192**Faraday to Justus Liebig****3 October 1856****From the original in BS MS Liebigiana II.B. Faraday, M.**

Royal Institution, London | 3 Oct 1856

My dear Liebig,

Your letter¹, received a few days ago; grieves me, since it shews that you feel very much, the opposition set up by some to your views, and the principles you so effectually advocate. – I say effectually, for though present obstruction arises the truth must ultimately prevail. But we know, as a matter of universal experience, that it never makes way at once; but has to fight through a long course of resistance, arising from invested interest, pride of knowledge falsely so called², retention of old habits, prejudice, &c and all these your great truths must meet with, just as every other advancing part of science has and is still meeting with, as I think I, for one, perceive in my own department. Moreover I think you are hardly aware of the strong hold your name, and principles, have upon our general population; and of those here who speak for them. Daubeny³ I believe is one of the latter⁴; – and Playfair, on one of the last Friday Evenings (May 30th)⁵. in our Institution in referring to the chemistry of Agriculture (which was his subject) to Mr. Lawes and to you, spoke up most earnestly for your views, as he had a right to do; and was, as I can testify, well responded to by the feeling of our audience: – as far as I could judge true Agricultural chemistry is making its way as well as could be expected, considering the enormous mass of persons in the country requiring instruction and the very unprepared state for it in which they were not many years ago. But as I said before how can a man expect, in his own life time, to be truly, recognised; it requires more than one generation to give currency to his highest truths.

Your expressions with regard to my Nephew⁶ are most kind and acceptable; and give great comfort to his parents and family. He speaks of you in a manner shewing his deep sense of gratitude for all your kindness, and his respect and veneration for you as a commanding intellect amongst those by which you are surrounded. We hope to see him soon, & to perceive the influence of the minds amongst which he has been sojourning. – His sister⁷ thanks you gratefully for him and desires to be remembered. Tell the Baroness⁸, that my wife and I are deeply indebted to her for thinking of us, and, scarcely hoping for any personal opportunity of expressing our feelings, desire *now* to thank her most heartily, with you, for your wishes in our favour – I am becoming old dear Liebig, and I am losing my memory, & with it the means of enjoying many pleasures; but no loss of memory can make me altogether forget the abundance of mercies I have received & am in possession of, amongst which I hope & ask for the continuance of a contented & cheerful spirit for the short remainder of days.

Ever My dear friend | Yours Most truly | M. Faraday

1. Letter 3190.
2. 1 Timothy 6: 20.
3. Charles Giles Bridle Daubeny (1795–1867, ODNB). Professor of Chemistry at Oxford University, 1822–1855.
4. Daubeny (1856), lvi.
5. Playfair, L. (1856), Friday Evening Discourse of 30 May 1856.
6. Frank Barnard.
7. Jane Barnard.
8. Henriette Liebig, née Moldenhauer (1807–1881, Brock (1997), 44). Married Justus von Liebig in 1826.

Letter 3193

Faraday to Robert Brudenell Carter

4 October 1856¹

From the original in WIHM MS FALF

[Royal Institution embossed letterhead] | 4 Oct 1856

Dear Sir,

I enclose the papers as you desire & beg to return my thanks to you for the copy of your work²[.]

Yours Very faithfully | M. Faraday

R.B. Carter Esqr | &c &c &c

1. Robert Brudenell Carter (1828–1918, *Ann. Reg.*, 1918, pp. 197–8). Physician.
2. Carter (1855).

Letter 3194

Maurice Hess¹ to Faraday

5 October 1856

From the original in IET MS SC 2

33, Rue de l'Est, Paris le | 5 Octobre 1856

Mon Vénérable Maitre!

J'ai l'insigne honneur de vous remettre ci-jointe une feuille qui peut-être vous interessera, Monsieur, à cause de la matière qu'elle traite, plutôt qu'à cause de la manière tout à fait insuffisante avec laquelle j'ai essayé de la traiter dans une ébauche plus que superficielle.

La feuille que j'ai l'honneur de vous remettre, est un extrait d'une introduction que j'ai publié dans une revue française. Elle a déjà paru l'année

passée 1855, dans le même mois où Mr. *Foucault*² a fait l'expérience que je cite à la fin de la brochure. Il est assez heureux pour moi qu'un an plus tard, c'est à dire dans l'année courante 1856, Mr. *De la Rive*, qui n'avait certainement pas connaissance de mes petits travaux, pas plus que je ne pouvais avoir connaissance de son deuxième volume qui a paru il y a seulement quelque mois³, ait terminé ce second volume par la même citation de l'expérience de M. *Foucault* à l'appui de sa théorie qui a une grande analogie avec celle émise dans ma petite brochure. Bien que ma théorie ne coïncide pas complètement avec celle de M. De la Rive, cette dernière arrive pourtant à supposer aussi une *rotation d'atomes*, supposition qui est le point principale sur lequel j'appuis une nouvelle théorie de la *gravitation*.

Ce n'est que depuis les dernières commotions politiques qui m'ont conduit forcement de l'Allemagne, ma patrie, en France, où je demeure comme exilé, que j'ai commencé à m'occuper sérieusement de physique et de chimie, attiré principalement par les ingénieuses recherches et expérimentations, par lesquelles vous avez démontré la liaison qui existe entre l'électricité et l'action chimique⁴. Conduit par votre opinion sur l'insuffisance de la théorie des atomes pour expliquer les phénomènes électro-chimiques, et désiré de trouver les rapports pressentis par vous entre la gravitation et ces phénomènes⁵, j'ai essayé de me rendre compte de ces rapports par une théorie qui n'est, en effet, qu'une hypothèse, tant qu'elle ne sera constatée par l'expérimentation. Mais, quoique je ne désespère pas à parvenir un jour de constater ma théorie par des expériences scientifiques, il est pourtant de mon devoir à vous communiquer, mon vénérable maître, mes tentatives théoriques.

Comme vous verrez, Monsieur, je ne suis pas adversaire absolu de la théorie des atomes. Mais si je crois qu'elle rend bien compte des phénomènes de la chaleur et de la lumière, de même que des actions chimiques, je ne suis pourtant pas de l'avis de M. *De la Rive* quant à sa théorie générale des phénomènes électriques. Pour que l'électricité puisse se propager, M. De la Rive suppose toujours un milieu contenant des atomes chimiques, et il explique p. ex. l'arc voltaïque dans le vide par une décharge d'atomes chimiques qui s'en vont d'un bout du conducteur à l'autre. Il y a une objection capitale qui s'oppose à cette théorie. L'électricité s'échappe de la surface des corps conducteurs isolés par l'air, et cela d'autant plus que l'air est rarifié davantage; ce qui a conduit *Mateucci* à supposer qu'elle s'en échapperait entièrement, dans le vide parfait. Il faut en conclure que l'électricité peut exister indépendante d'atomes chimiques – ce qui n'exclut pas que l'éther soit précisément de l'électricité combinée, accumulée, et plus ou moins condensée. Mais cette condensation qui peut produire aussi des atomes, ou de centres, formant le milieu par lequel se propage la chaleur et la lumière, doit produire originairement des atomes beaucoup plus dilatés que les atomes chimiques ou le milieu par lequel se propage p. ex. le son qui impressionne l'oreille, parce que le son se propage infiniment moins vite que la chaleur et la lumière.

Je suis donc porté à croire que l'arc voltaïque est produit par l'accumulation de l'électricité et la formation d'atomes étheriens qui réagissent en se condensant et produisent la lumière et la chaleur – et je crois que si l'on pouvait déterminer une condensation, ou une accumulation plus forte de l'électricité dans le vide, il pourrait en résulter la formation d'atomes chimiques, si toutefois les difficultés expérimentales ne seraient insurmontables. Mais beaucoup de difficultés qui paraissent insurmontables, ne le sont pas pour vous, Monsieur; et si vous ne trouvez pas mon hypothèse trop folle, peut-être vous vous occuperez de cette expérimentation difficile. – On arriverait par une telle expérimentation à expliquer la formation des nébuleuses dans l'espace, où je suppose de l'électricité accumulée de plus en plus par les déperditions que les corps célestes, et notamment les soleils, éprouvent pendant toute leur existence.

Je vous demande mille fois pardon, Monsieur, de vous entretenir de théories qui ne sont que des hypothèses dans l'état actuel des sciences. Mais je crois que c'est précisément à un savant si éminent, comme vous, Monsieur, de constater par des expériences ingénieuses les liens déjà pressentis par vous, Monsieur, qui existent entre la gravitation et tous les autres mouvements dynamiques de la matière.

Si vous le permettez, Monsieur, je vous enverrai mes travaux à mesure qu'ils seront publiés, en commençant par une nouvelle théorie du soleil que je publie maintenant dans une revue naturaliste allemande.

Vous me feriez infiniment heureux, Monsieur, si vous vouliez me daigner de votre avis sur mes tendances théoriques. – Agréez, en attendant, vénérable Maître, l'hommage qu'un de vos plus grands admirateurs se permet de vous offrir en comptant sur votre indulgence.

Maurice Hess

TRANSLATION

33, Rue de l'Est, Paris | 5 Oct 1856

My Venerable Master!

I have the distinguished honour of sending you the enclosed paper which may interest you, Sir, because of its subject matter, rather than because of the wholly inadequate manner with which I have tried to treat it in this extremely superficial draft.

The paper that I have the honour of sending you is an extract from an introduction that I published in a French magazine. It appeared last year 1855, in the same month as Mr. *Foucault*² conducted the experiment which I cite at the end of this brochure. It is most fortuitous for me than a year later, that is to say this year 1856, Mr. *De La Rive*, who certainly did not know of my little work, any more than I could have known of his second volume which appeared only a few months ago³, ended this second volume by citing the same experiment of Mr. *Foucault* in support of his theory that has a strong

analogy with the one expounded in my small brochure. Although my theory does not coincide completely with the one of Mr. De La Rive, his theory also comes to suppose a *rotation of atoms*, a supposition which is the principal point on which I support a new theory of *gravitation*.

It is only since the last political troubles which forced me to leave Germany, my homeland, and come to France, where I live as an exile, that I have begun to occupy myself with physics and chemistry in earnest, attracted principally by the ingenious research and experiments, by which you have demonstrated the link which exists between electricity and chemical action. Guided by your opinion on the insufficiency of the theory of atoms to explain electro-chemical phenomena⁴, and desirous of finding the relationship sensed by you between gravitation and these phenomena⁵, I have tried to explain this relationship with a theory which is nothing more, in fact, than a hypothesis, until it is proved by experimentation. But, although I do not despair that one day I shall prove my theory with scientific experiments, it is however, my duty, to communicate to you, my venerable master, my tentative theories.

As you will see, sir, I am not an absolute opponent of the theory of atoms. But if I believe that it gives a good account of the phenomena of heat and light, as well as of chemical actions, I do not share Mr. *De La Rive's* opinion as to his general theory of electric phenomena. In order for electricity to be propagated, Mr. De La Rive still supposes there to be a milieu containing chemical atoms, and he explains for example the voltaic arc in a vacuum by a discharge of chemical atoms which go from one end of the conductor to the other. There is a fundamental objection to this theory. Electricity escapes from the surface of conducting bodies isolated by air, and the more rarified the air, the more it escapes; which led *Matteucci* to propose that it escaped entirely in a perfect void. This leads one to conclude that electricity can exist independently of chemical atoms – which does not exclude that ether is precisely combined, accumulated, and more or less condensed electricity. But this condensation which can also produce atoms, or centres, forming the milieu through which heat and light are propagated, must originally produce much more dilated atoms than chemical atoms or the milieu through which, for example, sound which hits the ear is propagated, because sound is propagated infinitely less quickly than heat and light. I am therefore led to believe that the voltaic arc is produced by the accumulation of electricity and the formation of ethereal atoms which react while condensing and produce light and heat – and I believe that if one could determine a condensation, or a stronger accumulation of electricity in a vacuum, the formation of chemical atoms could result, always assuming that the experimental difficulties were not insurmountable. But many difficulties which appear insurmountable are not insurmountable to you, Sir; and if you do not find my hypothesis too wild, perhaps you could undertake these difficult experiments. – Such experimentation would

explain the formation of nebulas in space, where I suppose that more and more electricity has accumulated from the loss experienced by celestial bodies, most notably suns, during the whole of their existence.

I beg your pardon a thousand times, Sir, for speaking of theories which are but hypotheses in the current state of science. But I believe that it is precisely up to a savant as eminent as you, Sir, to determine the links already sensed by you, Sir, which exist between gravitation and all the other dynamic movements of matter.

If you will permit it, sir, I will send you my works as they are published, starting with a new theory on the sun which is now being published in a German naturalist magazine.

You would make me infinitely happy, sir, if you wished to dignify me with your opinion on my theoretical tendencies. – Please accept, in the meantime, venerable Master, the homage which one of your greatest admirers takes the liberty of offering you, while counting on your indulgence.

Maurice Hess

1. Maurice Hess (1812–1875, NDB). Prussian socialist and Zionist who lived in Paris.
2. Jean Bernard Léon Foucault (1819–1868, DSB). French physicist.
3. De La Rive (1854–8), 2.
4. Faraday (1834), ERE7, 869.
5. Faraday (1851a), ERE24.

Letter 3195

Warren De La Rue to Faraday

11 October 1856

From the original in IET MS SC 2

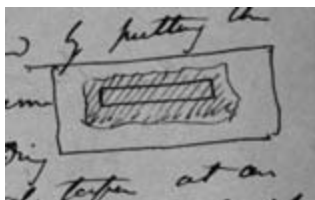
London, 110 Bunhill Row | 11 Oct 1856

My dear Mr. Faraday,

I returned only yesterday from Paris through which I passed on my way homeward from Caen in Normandy. I met Graham there and we called together on Pelouze¹ who informed us that Mitscherlich² was staying at the Hotel des Princes:– we called but he was unfortunately out. The Paris Mint is overwhelmed with gold which it fails to manufacture fast enough to supply the place of the place of the absent silver five-franc pieces, and much uneasiness exists in Paris as to the result of this monetary disturbance³. The processes of manufacture in the Paris mint appear to me to [be] very rough more especially in the founding and rolling of the metals as more than 20% of “blanks” are rejected for errors of weight or defects, and upwards of 10% of the coined pieces & I believe in the London Mint the total rejections do not exceed 5% in both stages.

Gold leaf

The depolarizing effects which I noticed in gold leaf were obtained by putting the gold leaf on a copper frame



with a long slit in it, and holding one or two of these inclined before a lighted taper at an angle of 45° & viewing the transmitted light through a Nichol's prism which was revolved in the hand. I have not repeated the experiment but I will do so⁴.

Yours very truly | Warren De la Rue

1. Théophile-Jules Pelouze (1807–1867, DSB). French chemist.
2. Eilhard Mitscherlich (1794–1863, DSB). Professor of Chemistry at Berlin University, 1825–1863.
3. See *The Times*, 9 October 1856, p. 7, col. a for an account of this.
4. See Faraday, *Diary*, 11 October 1856, 7: 15074.

Letter 3196

Warren De La Rue to Faraday

13 October 1856

From the original in IET MS SC 2

London, 110 Bunhill Row | 13 Oct 1856

My dear Mr. Faraday,

I have repeated the experiment with the gold leaf:–

I find that light passing through a gold leaf held at an angle say of 45° is polarized, and hence that an obscuration and lightening are produced alternately if the light is viewed through a Nicholl's prism made to rotate.

If the light is first polarized and the analyzer arranged to produce obscuration then the interposition of the gold leaf renders the flaw visible, as you state in your note.

I believe that your explanation is the true one for both cases, and hence that there is no polarization produced by the gold leaf *in consequence* of the compression by hammering¹.

Yours Very truly | Warren De la Rue

1. Faraday, *Diary*, 13 October 1856, 7: 15085 and 16 October 1856, 15089–113.

Letter 3197

Faraday to Christian Friedrich Schoenbein

14 October 1856

From the original in UB MS NS 426

Royal Institution | 14 Oct 1856

My dear friend,

Hearty and healthy and occupied and happy as you are let me congratulate you, for every letter of yours brings me evidence of the existence of a healthy mind in a sound body¹. How you have been running about? & you go home as if you were refreshed rather than tired by it. I do not feel so any longer; even if I go away for a little general health, I am glad to return home again for rest in the company of my dear wife & niece but as the wise man hath said there is a time for all things², & my time is to be quiet & look on, which I am able to do with great content & satisfaction – I expect one of my nieces³ here very soon who will let me into the knowledge of your last paper⁴; in the mean time I have sent the other copy & your letter to Dr. Whewell. What you tell me of your paper makes me long to hear the whole of it; though the very pleasure of getting knowledge is now mingled with some thoughts of regret at the consciousness that I very quickly lose it again;– well – a time for all things. I have been occupying myself with gold this summer; I did not feel head-strong enough for stronger things – The work has been of the Mountain & mouse fashion⁵; & if I ever publish it⁶ & it comes to your sight, I dare say you will think so:– the transparency of gold – its division – its action on light: &c &c &c.

Now with regard to Miss Schoenbein's desires:– I am sorry that my unsocial habits have left me unacquainted with any such family as that which I think would suit your view. Not one name occurs to me but Grove and to Mrs. Grove⁷ I shall show your letter as soon as they come to town. It so happened that two or three years ago Tyndall shewed us a letter very much to the same purport regarding a daughter of one of his German friends⁸; that letter we shewed to a lady (Miss Hornblower) & it led to Tyndall's friend coming to London & being with Miss Hornblower for I think two years & it is not very long ago since she went back, very happy in the thoughts of her residence her. I have shown *your* letter to Miss Hornblower in hopes she might know of some family: and her note to me in reply, is such, that I send it on to you. Miss Hornblower is a very dear friend of ours, & in her character & all that is about her, all we could wish;– but then she keeps a school. it is an excellent establishment, with many masters, & the pupils who have been with her all love & respect her. If what she says induces you to write to her, do so directly & without hesitation. For your private thought I may say she is about 50 years of age, very active, though not very strong, & has sustained her establishment of 15 or 20 pupils at Stamford Hill for full 30 years.

Pray remember me to Wiedemann;– and us most kindly to Madam & Miss Schoenbein & also to the Merians and above all to Yourself.

Ever My dear Schoenbein | Yours Most truly | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | Switzerland

1. See letter 3191.
2. William Shakespeare, *The Comedy of Errors*, II, 2, 65.
3. Probably Margery Ann Reid.
4. Schoenbein (1856c).
5. Aesop fable 26.
6. Faraday (1857c).
7. Emma Maria Grove, née Powles (d.1879, age 68, GRO). Married Grove on 27 May 1837, see his ODNB entry.
8. See Ballin to Tyndall, 2 January 1855, RI MS JT/1/B/11.

Letter 3198

Faraday to Frederick Field

21 October 1856

From Gladstone (1874), 95

Royal Institution | 21 Oct 1856

My dear Sir,

Your paper looks so well, that though I am of course unable to become security for the facts, I have still thought it my duty to send it to the Royal Society¹. Whether it will appear there or not I cannot say²,– no one can say even for his own papers; but for my part, I think that as facts are the foundation of science, however they may be interpreted, so they are most valuable, and often more so than the interpretations founded upon them. I hope your further researches will confirm those you have obtained: but I would not be too hasty with them,– rather wait a while, and make them quite secure.

I am, Sir, your obliged Servant | M. Faraday

1. See letter 3199.
2. Field (1856). The manuscript is in RS MS AP 39.31.

Letter 3199

George Gabriel Stokes to Faraday

23 October 1856

From the original in IET MS SC 2

69 Albert St. Regent's Park | 23 Oct 1856

My dear Sir,

I have just got and read Mr. Field's paper which seems a nice one and will do very well for the Proceedings¹.

I shall take great interest in reading your paper on Gold², but I shall scrutinize very carefully the evidence for the ruby colour being due to metallic gold³.

Yours very truly | G.G. Stokes

M. Faraday Esqr | &c &c &c turn over

I see you have called me Stokes; I hope you will not consider it too great a liberty if I call you Faraday.

1. Field (1856). The manuscript is in RS MS AP 39.31.
2. Faraday (1857c).
3. See letter 3158.

Letter 3200

George Gabriel Stokes to Faraday

25 October 1856

From the original in IET MS SC 2

69 Albert St. Regent's Park | 25 Oct 1856

My dear Sir,

I had hoped to have the honour of calling you by your name, but in a host of official R.S. letters the "Sir" slipped out unawares¹, and I was inclined to write my letter over again, but thought it was not worth while. But as you think it necessary to apologize I suppose I must keep to the "Sir" for I feel it due to your age and standing in the scientific world that you should make the change if any is to be made.

I know well that you carefully criticize your own papers. Still my optical studies render it so difficult for me to believe that the colour of the ruby glass is due to metallic gold² that I cannot be content without scrutinizing for myself. I shall take great interest in reading your paper when it is ready³.

I am dear Sir | Yours very truly | G.G. Stokes

1. See the end of letter 3199.
2. See letter 3158.
3. Faraday (1857c).

Letter 3201

Peter Henry Berthon to Faraday

6 November 1856

From the original in GL MS 30108/2/71

Trinity House | 6 Nov 1856

My dear Sir,

The Board being desirous of obtaining your opinion on the Effect of a lenticular Apparatus procured in 1852 for the Bishop Rock Light House and

which is still lying pack'd in Cases at Blackwall, I shall be obliged by your calling upon me any day that may be convenient to you in order that I may have an opportunity of explaining the particular points upon which your advice is considered requisite.

Believe me | My dear Sir | Yours very faithfully | P.H. Berthon
M. Faraday Esq | &c &c &c

Letter 3202

Faraday to George Wilson¹

7 November 1856

From the original in LU

[Royal Institution embossed letterhead] | 7 Nov 1856

My dear Sir,

Let me thank you very sincerely for your kindness in sending me a copy of your book on the five Gateways of knowledge². I hold it close at hand for a pleasant reading very shortly[.]

Ever Your Obligated | M. Faraday
Dr. Geo Wilson | &c &c &c

1. George Wilson (1818–1859, ODNB). Chemical lecturer and writer in Edinburgh.

2. Wilson, G. (1856).

Letter 3203

Christian Friedrich Schoenbein to Faraday

10 November 1856

From the original in UB MS NS 427

Bâle | 10 Nov 1856

My dear Faraday,

Many many thanks both for your own letter¹ and that of your Friend's². When you happen to see her again, pray tell her that from many reasons my daughter desires to stay here during the winter. When spring comes and we live then, we shall see, whether a move over the water may be effected. In the mean while your friend will perhaps be kind enough to let you know her views on the subject in question. All I can say is this: Miss Schoenbein knows well and speaks fluently french and german and is considered to be a pretty good musician, in which accomplishments she is very willing to make herself useful in the establishment of your friend. As to the moral Character of my daughter, I am too partial to judge about, but I do not hesitate to qualify her as a modest, good natured and rather high-minded girl, and the purest Swabian blood running in her veins she partakes a little of the poetical spirit said to belong to the native land of her fore-fathers.

Since I wrote last³ to you I have actively continued my researches on the phenomena of contact, and obtained some results which are curious enough. You know perhaps that according to my former experiments ozonised oxygen at the common temperature oxidizes both the elements of Ammonia forming with that compound nitrate of Ammonia, whilst, as you are well aware, common oxygen does under the same circumstances not at all affect either gaseous or aqueous ammonia. The same oxygen however on being put in contact with certain matters acquires the power of engendering with ammonia nitrous acid i.e. nitrite of ammonia. Platinum and copper are such matters. Moisten the former metal (being in that state in which we call it "Ethiops") with a strong solution of Ammonia, leave for a short time those substances exposed to the action either of common oxygen or atmospheric air, then treat the metallic powder with some distilled water and you will easily detect in that liquid the presence of nitrite of Ammonia. The simplest way of doing so it to put to that water some dilute sulphuric acid and paste of starch containing a little pure jodide of potassium (free even from the slightest trace of jodate). Nitrite being present the mixture will become dark blue.

Assisted a little by heat even compact platinum is capable of causing common oxygen to engender a nitrite with Ammonia. Put some drops of a strong solution of Ammonia into an air holding bottle introduce into the vessel the heated coil of a thick platinum wire, hold over that coil a strip of filtering paper to which sticks paste of starch containing some jodide of potassium and being acidulated by dilute sulphuric acid, and you will perceive that paste instantaneously turning dark blue. Whilst the hot platinum coil rests within the bottle, whitish vapours make their appearance which on being taken up by some distilled water give to that fluid all the properties of a nitrite solution. Being acidulated by dilute sulphuric acid it deeply and instantaneously blues the jodide holding paste of starch and such a strong reaction will be obtained though the heated coil may have remained in the bottle but for a few seconds. The platinum coil does not require being red heat to produce that effect, but those of iron wire &c: must have that temperature to enable oxygen to engender a nitrite with Ammonia. I above mentioned copper as another substance being capable of causing ordinary oxygen to oxidize both the elements of Ammonia at the common temperature, and I may add that its action even surpassed that of platinum. To convince yourself of the truth of my statement put about 50 grammes of minutely divided Copper (such as obtained from heated oxide of Copper by the means of hydrogen) into an oxygen or air holding bottle, moisten that metallic powder with a solution of Ammonia, close or cover the bottle and you will soon see filling the vessel with whitish fumes, which are nitrite of Ammonia; for if you introduce into the bottle a strip of paper being covered with acidulated paste of starch that contains some jodide of potassium, it will rapidly be colored blue. Or if suspended but for a short time strips of filtering paper being impregnated with distilled water, they will contain perceptible quantities of nitrite

of Ammonia, as you may easily satisfy yourself by applying the test above mentioned. Even a moistened glassplate or watchglass, by which you cover the vessel of reaction will do to receive within a very few minutes so much of the nitrite formed as to enable you to ascertain its presence by the most striking reactions.

To complete my statements I must not omit to mention that the copper powder soon after having been moistened with liquid ammonia, exhibits a rise of temperature, no doubt resulting from the formation of the nitrate of ammonia. The blue liquid obtained on shaking copper powder with aqueous Ammonia and oxygen or atmospheric air besides oxide of copper also contains nitrite of Ammonia, for if you put some soda to it and boil it up to drive off the ammonia and throw down the black oxide of copper, a solution is obtained which after being evaporated to dryness leaves behind a yellowish salt being principally made up of nitrite of Soda. This substance being mixt up with powdered charcoal and heated burns that combustible matter, yields with sulphuric acid strong fumes of nitrous acid, rapidly discharges the colour of indigo solution being strongly acidulated by oil of Vitriol, colors brownish a solution of vitriol of iron containing free sulphuric acid &c. Common pure or atmospheric oxygen being put in contact with copper powder and aqueous ammonia is so rapidly taken up that I succeeded in depriving completely a whole cubic foot of atmospheric air of its oxygen within a few minutes. Copper and ammonia may therefore be used as eudiometric means and for preparing nitrogen from the common air. The facts above stated appear to me to bear closely upon the important question of nitrifications and proving beyond any doubt that under the influence of the contact of some ponderable matters inactive oxygen is empowered even at the common temperature to oxidize both the constituent parts of ammonia. Before long I hope to be able to give you some more details on nitrification, a chemical phenomenon which at this present moment deeply enjoys my attention.

Pray present my best compliments to Mrs. Faraday and believe me

My dear Faraday | Your's most faithfully | C.F. Schoenbein.

Mrs. Schoenbein and the young ladies charge me to offer to you and Mrs. Faraday their kindest regards.

Address: Dr. Michael Faraday | &c &c &c | Royal Institution | Albemarle Street | London

1. Letter 3197.
2. Jemima Hanbury Hornblower.
3. Letter 3191.

Letter 3204**George Herbert to Faraday****12 November 1856****From the original in GL MS 30108/2/70**

Trinity House | 12 Nov 1856

My dear Sir,

I am directed to forward to you the accompanying Samples (3) of Drinking Water which has been received from two of the Corporation's Light Houses where it is kept in Leaden Cisterns, and to request that you will favor the Elder Brethren by making an examination of the Water, and reporting the result, at your convenience; with a view to ascertain whether in the contents of either of the Samples, there is anything deleterious to the health of the persons using the same.—

I am | My dear Sir | Your very faithful Servant | George Herbert | Assistant Secy

M. Faraday Esq. D.C.L | &c &c &c

Letter 3205**Faraday to Peter Henry Berthon****15 November 1856****From the original copy in GL MS 30108/2/71**

Royal Institution | 15 Nov 1856

My dear Sir,

I have applied to Mr. Wilkins for any drawings or plans of the apparatus referred to¹ but can get no information that will enable me to form an opinion without the aid of a personal inspection. I venture to suggest that Mr. Wilkins be instructed to arrange three of the sections each about 10 inches wide with the upper & lower reflectors on the frame at the Trinity house along side & next to a lenticular pannel already on the frame. The Deputy Master² & Brethren can then see the effect & I think the distance will be quite sufficient for my observation i.e. if the optical work prove to be of the usual character of that produced in France[.]

I am My dear Sir | Very faithfully Yours | M. Faraday
P.H. Berthon Esq | &c — .

1. In letter 3201.

2. John Shepherd.

Letter 3206**Faraday to George Herbert****17 November 1856****From the original copy in GL MS 30108/2/70**

Royal Institution | 17 Nov 1856

My dear Sir,

In regard to the three waters Nos. 1. 2 & 3 accompanying your letter of the 12th instant¹, I may report that No. 1 is a very good water, of considerable softness,—containing no lead in solution, & therefore in itself wholesome: but on examining the slight deposit in the bottle, lead was found there in small quantity. This is often the case in leaden cisterns:—care should be taken that the pumps or pipes are so arranged not to draw off the deposit in the cistern with the water[.]

No. 2 is a good soft water of itself but it contains a little lead *in solution*. I have explained on former occasions that rain water can dissolve lead, & that whilst the lead remains dissolved the water is unwholesome:—such is the case at present with this specimen.—The little sediment which was there also contained a trace of lead.

No. 3 water is soft & good & there is no trace of lead either in the fluid or the deposit.

All these waters contained small portions of sulphates & chlorides of lime & soda: but as there was nothing objectionable in them but rather an advantage, I have not referred particularly to them[.]

The corks of all the bottles were bad – Bottle No. 3 was also dirty and contained an old wine cork – a dirty cork is competent to affect the lead in water & occasionally to remove it. So that, such a state of things should be avoided in cases where the questions to be considered are important & influential[.]

I am | My dear Sir | Very truly Yours | M. Faraday
G. Herbert Esq | &c &c &c

1. Letter 3204.

Letter 3207**Faraday to Henry Bence Jones¹****1 December 1856****From the original in RI MS F1 D34**

51A Kings Road, Brighton | 1 Dec 1856

My dear friend,

I purpose writing out an account of the little philosophic incident which interested you so much the other evening, though it is very probable that the interest may sink when the facts are plainly described. A Lady, in walking about her bedroom, ran a needle into the great toe; on searching the floor a

part of a needle was found, and there was every reason to believe, from the sensation & other circumstances, that the pointed end, being broken off, was left in the toe: yet neither a friend not a surgeon could find it there_[.] About two months after, and when doubts were entertained by some whether any needle was there or not, I proposed to settle *that* question, & if there, to say in what part of the toe it then had its place, & what was its direction_[.] The following were the means employed. A fine sewing needle was well magnetized & a piece about $\frac{1}{8}$ th of an inch long was broken off. Then a filament of cocoon silk, about an inch long, had one end attached to the middle of the little magnet by a touch of soft cement, & the other end to the handle of a camel hair brush. In this way a minute test magnetic needle was obtained, which pointed well under the earths power, & could easily be brought near any part of the toe. The toe being examined by this needle, shewed occasional deflexions of the latter, which, being imperfect as indications, still seemed to imply that a piece of the broken needle was there, but rather deeply situated. Now, the means of increasing these indications and making them tell their story more perfectly was applied, and upon the following principles.—A piece of unmagnetized needle will affect a little magnet at a distance very feebly, compared to the same piece magnetized:—the unmagnetized piece will affect either end of the test needle alike, whereas the magnetized piece will affect the opposite ends in different ways, attracting one & repelling the other;—A piece of unmagnetized needle can be magnetized at a distance by a magnet of sufficient power, & as well, whilst in the toe as out of it:—also its magnetic state can be reversed if the inducing magnet be reversed, and further it can be, by management, unmagnetized, and being without magnetism is not rendered magnetic whilst the lines of force of the inducing magnet are perpendicular to it. Upon these principles I started, employing a single bar horse shoe magnet made by Logeman², with the poles so far apart that I could introduce my thumb between them. This magnet was brought up to the toe, then taken away & the part examined by the indicating needle, the indication was not stronger than before; hence one of two conclusions was probable; either there was no piece of needle there, or its position was at right angles nearly to the line joining the poles of the inducing magnet when it had been applied. So the latter magnet was employed again in a different position, & now the indicating needle told its story beautifully; shewing the little magnet which had been formed within the toe & which by its power, attracted one end of the needle & repelled the other. A few more trials with the inducing magnet shewed in what position it was able to act most strongly on the hidden needle, and then the position of the magnetic axis of the former indicated the position of the latter. Reversing the position of the inducing magnet instantly changed the magnetism of the buried needle, and now that point of the skin which before attracted the marked end of the indicating needle, repelled it strongly & attracted the other end. It was beautiful to see, when the indicating needle was carried round a given spot on the surface of the toe, how one end always

pointed towards that spot; & even moved as if striving to dip down to it. Another indication, which will interest you, was afforded. The direction of the piece of needle and the locality of *one* end was clearly shewn; then came the question where may the other end of the piece be and what is the probable length of the whole? By carrying the indicating needle about the toe, it was found that the opposite end of the piece, though it exhibited an influence over the indicator, did not come near the opposite surface of the toe, but was buried deep in the flesh; and the piece, though manifestly piercing deep, & almost perpendicularly into the flesh, did not come near the other side. I guess it therefore to be about the third of an inch long. Presently we may tell if the needle travels and which way it is going:— all of which will be of great interest to us, who look on philosophically, and likewise to the unlucky possessor of this illustration: who I trust however will soon be able to make the experiments in her hand instead of her feet.

And now my friend here we are very comfortably situated in dry & largish rooms at 51A Kings Road – and though all tender (I am best) yet we all think it will do us good. I think if you could pop in to a cup of tea – you would approve of our locale & arrangements. Winter is very strong on the outside though perhaps he is going but we do not let him come into the house. Hoping that all with you are pretty well in which hopes as well as for those for yourself my wife & Jeannie joins me

I am | Ever truly Yours | M. Faraday

We have no occasion for a Dr. but cannot remember the name of the one you spoke of.

1. The identity of the recipient is indicated in Bence Jones (1870a), 2: 393.
2. Wilhelm Martin Logeman (1821–1894, NNBW). Scientific instrument maker in Haarlem.

Letter 3208

Faraday to Benjamin Vincent

2 December 1856

From the original in RI MS F1 D35

51A Kings Road, Brighton | 2 Dec 1856

My dear friend,

I received your letter a while ago & dull as the news which it contains is thank you very much for it. We had heard from many quarters of the sore affliction which had overtaken Mr. Sandeman¹ & our brother² & sister³ & I may say a great multitude of friends – from Newcastle & all parts we hear of the grief – the event was to us very sudden for we had not heard of Mrs. Sandeman[’s] illness⁴. What a heavy visit to Edinburgh this will be to our dear friend how all things concur to make it sad. I am glad he has taken his

son⁵ with him – and we may be fully persuaded he will rest his griefs in the father of mercies who doth not willingly afflict. How rich is that source of comfort in the times of such trouble.

We are much as usual – the weather very frosty – I hope to see you next Saturday⁶. Remember me to Mr. Anderson & thank him from me.

Ever My dear friend | Yours Affectionately | M. Faraday
Mr. Vincent

1. Robert Sandeman (d.1886, age 60, SRO). Architect.
2. Stephen Leighton (d.1881, age 83, GRO). Printer and an Elder of the London Sandemanian Church.
3. Helen Leighton, née Blair (1796–1870, Sandeman (1950), GRO). Wife of Stephen Leighton.
4. Helen Sandeman, née Leighton (d.1856, age 25, SRO), Daughter of Stephen and Helen Leighton. She married Robert Sandeman in 1852 and died on 28 November 1856. Sandeman (1950), 112.
5. Unidentified.
6. That is 6 December 1856.

Letter 3209

Faraday to George Gabriel Stokes

6 December 1856

From the original in ULC Add MS 7656, F22

Royal Institution | 6 Dec 1856

My dear Stokes,

Though my paper¹ is very long I must add a little more. The accompanying comes in at the account of the effect of heat on gold²_[.] If you are induced to look for the connexion you will easily find it by the pencil references at beginning & end. I am going out of town again immediately but shall return permanently at the end of next week & then will try to see you_[.]

Ever Yours Truly | M. Faraday

1. Faraday (1857c).

2. This insertion is in RS MS PT 57.2, pp. 10a–10c which corresponds to Faraday (1857c), 150–1.

Letter 3210

Faraday to Paolo Volpicelli¹

6 December 1856

From the original in Accademia Nazionale dei Lincei

Royal Institution, London | 6 Dec 1856

My dear Sigr. Volpicelli,

I take the liberty of introducing to your acquaintance a friend of mine Dr. Henry² who will shortly be in Rome_[.] I am quite sure you will be pleased to make his acquaintance_[.] Such papers as I have had for you I have sent

before time by the Royal Society but I hope in the course of a few months to give you proof that I am alive if not by a good paper at the Royal Society at least by a long one³. My kindest respects to Sig Palagi & others of my friends at Rome – I wish I could see you all there & pay my obedience to the Society⁴[.]

I am | My dear Sir | Your faithful Servant | M. Faraday

1. Paolo Volpicelli (1804–1879, P3). Secretary of the Pontifical Academy of Sciences, 1847–1877.

2. William Charles Henry (1804–1892, ODNB). Physician and chemist.

3. Faraday (1857c).

4. Presumably the Pontifical Academy of Sciences to which he had been elected in 1850. Odescalchi and Volpicelli to Faraday, 20 November 1850, letter 2345, volume 4.

Letter 3211

Robert Murray¹ to Faraday

15 December 1856

From the original in RI MS F1 I130

43 Piccadilly | 15 Dec 1856

Dear Sir,

I am about asking a favour & I hope you will not think I am taking too great a liberty. I wanted my son who is in the business with us to hear your Lectures². The favour I want is to ask is if you could kindly give me an admission for him to go in the gallery[.] I shall esteem it a great favour hoping you are quite well & will pardon my troubling you

I remain your Obedient Servant | Rob. Murray

1. Robert Murray (1798–1857, *J. Chem. Soc.*, 1857, 10: 191). An employee of John Newman's.

2. Faraday's Christmas lectures 'On Attractive Forces'. His notes for these are in RI MS F4 J12.

Letter 3212

Faraday to Peter Henry Berthon

19 December 1856

From the original copy in GL MS 30108/2/71

Royal Institution | 19 Dec 1856

My dear Sir,

I am now able to reply to your letter¹ respecting the lenticular apparatus, four of the twenty four lens pannels having been arranged in correct position at the Trinity house, in association with the central four-wicked lamp. Each lens pannel is 9 inches wide & $50\frac{3}{4}$ inches in height, so as to present an area of 457 square inches. The great lens (eight to the circle) is nearly 29 inches wide & $37\frac{1}{2}$ in height, its area being 1088 square inches. The four lenticular pannels threw very good beams of light on to the screen placed at a distance of 43 feet; where their width was nearly 50 inches, and the dark intervals between them

about 95 inches. With a uniform rate of revolution the light at this distance will continue therefore nearly half the time of the following darkness; and so on in succession. It is probable that at greater distances the light will creep a little into the darkness; but I believe that at any distance at which the light will be visible the light & the darkness will be perfectly distinct from each other. Above & below each lens are reflecting prisms^[.] The eleven above give a very good amount of light, but the ray from them has an average horizontal width of 92 inches, the following dark space being about 48 inches; so that under a uniform rate of revolution the light due to this part will endure twice as long as the darkness. The four reflecting prisms below each lens pannel, give very little light by comparison; its duration will be about the same as that of the upper prisms. The effect of the whole, at this distance of 43 feet, under the supposition that one revolution is to occur in 5 minutes, would be to give for each period of 12 seconds, – 4 seconds of bright light,– succeeded by 2 seconds of feeble light,– followed by 4 seconds of darkness, succeeded by 2 seconds of feeble light:– and then again the 4 seconds of bright light & so on^[.]

The glass of the lenses is excellent;– that of the reflecting prisms is green and several of them very green.– The work is excellent.

Though I have no doubt of the perfect separation of the light & darkness at any distance, still the proportion given above for the 43 feet would probably vary for quarter distances. Any variation of that kind might be ascertained by trial between Blackwall & North Woolwich station or between Purfleet & Blackwall if thought necessary.

I need scarcely observe that the light from each lens must not be expected to equal that from the great lens as above twice the number of rays from the lamp fall upon & are sent forward by the latter²^[.]

I am | My dear Sir | Your faithful Servant | M. Faraday
P.H. Berthon Esq | &c &c &c | Trinity House

1. Letter 3201.

2. This letter was read to Trinity House By Board, 23 December 1856, GL MS 30010/40, p. 480. It was referred to the Deputy Master, Wardens and Light Committee.

Letter 3213

George Gabriel Stokes to Faraday

24 December 1856

From the original in IET MS SC 2

Observatory, Armagh, Ireland | 24 Dec 1856

My dear Faraday,

You will see by the heading that I am not in Town otherwise I should have taken great interest in seeing the experiment. I suppose I shall find the specimens on my return to London on the 3d of Jan?

Wishing you the happiness of the season I remain
Yours very truly | G.G. Stokes

Letter 3214**Faraday to Millicent Bence Jones****26 December 1856****From a photocopy in RI MS**

R Institution | 26 Dec 1856

Dear Lady Millicent,

I do not know that any body from your family would care about the lectures¹ here or find it convenient to come to any of them but I know I ought not to forget the kindness I have received from it. If the enclosed is of no use you can burn it if of any I shall be happy I have sent it_[.]

Ever Your faithful Servant | M. Faraday

1. Faraday's Christmas lectures 'On Attractive Forces'. His notes for these are in RI MS F4 J12.

Letter 3215**Edward Brown Fitton¹ to Faraday****27 December 1856****From the original in IET MS SC 2**

St. Ewe, St. Austell, Cornwall | 27 Dec 1856

To Michael Faraday Esqre D.C.L &c &c

Sir,

Although very probably I am about to describe to you an occurrence with which you are perfectly familiar as an Electrical Phenomenon yet as it is possible that you have never witnessed or had related to yourself an occurrence such as I am about to describe to you viz a luminous ball or halo of Electric fluid playing around to ears of *an animal* [similar I suppose to the balls of light which I have read of as frequently seen by sailors, during storms, at the extremities of the Masts & spars of a vessel]² I venture to write to you the details of such an occurrence which I witnessed yesterday December 26th during a severe storm of hail mixed with melted snow, at about 5h. 15m P.M. within $\frac{1}{2}$ a mile of Truro.—

As a guarantee of the truth of my story and of my own existence if the story is incredible to you I may mention that I am a son of Dr. Fitton³ whom I believe you know and who is now residing at 4 Sussex Gardens Hyde Park_[.]

Christmas day hereabouts (i.e. within 12 miles of Truro Cornwall —) was frosty with a good deal of sleet & snow and the ground very hard frozen on the morning of the 26th. I was out of doors almost the whole day from 10 till dusk having to ride on business to Truro on the 26th and I noticed that there

was scarcely any actual Thaw altho frequently there were severe showers of melting or only half frozen snow. I left Truro on my way home to St. Ewe at precisely 5h. 5m P.M. being mounted on a tall mare nearly 16 hands high[.]

A steep hill leads out from Truro for about $\frac{1}{2}$ a mile on the Main road along which I was riding to St. Ewe. The top of this hill lies N.W of the Town of Truro & the road is then almost flat for about $\frac{1}{2}$ a mile it being at this part a bleak unsheltered tract of land and almost the highest spot of ground within a quarter of a mile in all directions[.]

Just as I reached this flat part of the road we (I & the mare) were overtaken – (i.e. as I rode it came sideways but more in our faces than on our *left* side) – by a very violent storm of sleet which did not lie hard & *white* upon the ground but appeared to thaw as it fell. This lasted for about 4 or 5 minutes & came from the N.W. or more North than west and was accompanied by dim flashes of lightning and sharp gusts of wind[.]

My mare almost refused to face the storm & made very slow progress as against the drifts of sleet which were beating violently in her & my face. In a few minutes, perhaps 2 or 3, after the commencement of this storm I noticed a bright appearance on the *top & edges of my mares ears*. I felt certain at once that this was occasioned by Electricity in the Atmosphere but as it might be possible that I was mistaken I stooped forward in the saddle & wiped each ear with my fingers to make sure that the brightness I saw was not *white* sleet frozen on the edges of her ears[.]

The Ears like all the rest of the mares body were dripping wet but no snow at all remained unthawed on her body or on my garments and I was now quite positive that the brightness on the tips of each of her ears was caused by Electricity[.]

The light only appeared at the edges & tip perhaps for about $\frac{1}{8}$ th of an inch deep around the quarter of an inch forming the extremity of each ear thus

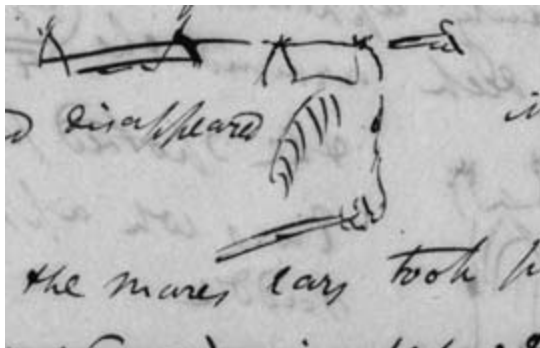


the spotted portions in the sketch being the part of the ears wh appeared luminous.- to me as I sat in the saddle[.]

This bright light like a glowworm's light or Phosphorous on the wall of a dark room only *whiter* I think than Phosphorous but more dim than Electric sparks which I have often seen from a Machine remained steady & unflickering while we walked (for the mare would not go faster in the face of the storm) for a full 6th of a mile I should say for at least 4 or 5 minutes judging by the distance we passed over - During this time there were one or two bright flashes of lightning but I could not hear any thunder and the light on the mares ears continued for a minute or so after the storm of sleet had ceased. then there was a large sheet of lightning but less bright than the flashes which had preceded it and the mares ears lost their light gradually the loss decreasing for a moment or two longer than the lower portion which had been brightened.

During the continuance of the light I repeatedly stroked the mare's ears with my hands on which I wore leather gloves (of course wet through) but the light was not imparted to the glove - nor did I *feel* any sensation at all - in fact the light on the mares ears was entirely unaffected by my handling her ears.-

There were several sharp bursts of similar storms during my journey home & once again at about 5.40 PM I noticed a faint light as before but now only at the extreme tip of the mares ears[.]



This time the light only lasted for about half a minute and disappeared immediately after a faint flash of lightning[.]

This second occurrence of the light on the mares ears took place on Probus Hill (Track Hill about 5 miles out of Truro) just below the 5th Milestone

from Truro. a bleak unsheltered portion of road & high rising ground. I may mention that the *bits* of my bridle and also my stirrups are galvanized to protect them from rust and all the buckles are plated so that there was no steel surface *exposed* to attract Electricity_[.]

I was protected from the cold by a Seal skin coat with the fur outwards but altho I expected to notice some similar luminous appearance, on the extremities of its hairs, to that which had attached itself to the points of my mare's ears I could not distinguish anything of the kind_[.]

It is very probable that the Phenomenon I have detailed may be familiar to you, in which case I hope you will forgive the liberty I have taken in addressing you as a gentleman of scientific repute throughout the world on matter the *exact* similitude of wh I have never heretofore heard or read of & I remain Sir yours obediently Edward Brown Fitton

1. Edward Brown Fitton (d.1910, age 85, GRO). Barrister not in practice.
2. Square brackets in text.
3. William Henry Fitton (1780–1861, ODNB). Geologist.

Letter 3216

Faraday to John Gibson Macvicar¹

29 December 1856

From the original in RCP

Royal Institution, London W | 29 Dec 1856

My dear Sir,

I hasten to thank you for your paper² & letter. You work vigorously; & it is well you do so; for your work is before you. I am free to say you are far in advance of me, for I am only an experimentalist, & cannot enter into the very numerous views of natural bodies & forces which have been put forth; otherwise than as they arise in my own mind from my own results. As it is I am esteemed by many to be very heterodox, but I just work on, forgetting as age comes on, many of my own choices of thought which I would have been very glad to retain, & afraid to embarrass[s] myself with the reasonings of others. Let us hope we are all advancing more or less towards truth though it may be by different roads_[.]

Yours Very Truly | M. Faraday
Rev'd. John MacVicar | &c &c &c

1. John Gibson Macvicar (1800–1884, ODNB). Minister at Moffat from 1853 and scientific writer.
2. Probably Macvicar (1856).

Letter 3217**Faraday to Edward Brown Fitton¹****29 December 1856****From the original in Birmingham University Library MS L Add 1887**

Royal Institution, London W | 29 Dec 1856

Sir,

I am greatly obliged by the account you have sent me² of a natural phenomenon which though I have never witnessed it has often been described to me in one form or another by those who have had that good fortune. Your account quite tallies with the rest & agrees with what we know of electrical action though as far as I remember the season of the year is rather unusual: but who can limit natural phenomena. I do not know that you were in any particular danger but you must have been involved in a very highly charged condition of the atmosphere & earth[.]

I am Sir | Your Very Obligated Servant | M. Faraday
E.B. Fitton Esqr | &c &c &c

1. Edward Brown Fitton (d.1910, age 85, GRO). Barrister not in practice.

2. Letter 3215.

Letter 3218**Faraday to Edward Vivian¹****31 December 1856****From the original in Torquay Natural History Society MS**

Royal Institution, London W | 31 Dec 1856

My dear Sir,

I have just had the pleasure of making out & signing your certificate: it will quickly have the other signatures necessary: and whenever you come to town you will have the right (by courtesy) to use the Institution as a Member even though the day of Election may not have arrived². I send also by this post certain printed papers which will explain themselves. I am glad you have thought of us we like the name of Vivian amongst our Members & I am also glad to hear of the progress of the hygrometer[.]

Ever My dear Sir | Yours Truly | M. Faraday
Edwd. Vivian Esqr | &c &c &c

1. Edward Vivian (1808–1893, B3). Partner in the Torquay Bank.

2. Vivian was elected a member of the Royal Institution at RI MS GM, 2 March 1857, 6: 234.

Letter 3219**Faraday to Angela Georgina Burdett Coutts****1 January 1857****From the original in BL Burdett-Coutts papers**

R Institution, London W | 1 Jan 1857

My dear Miss Coutts,

You are very kind to think of our pleasure & send us entrance to your box for to-morrow night¹. We thank you very sincerely, and I mean to enjoy it for I still have a sympathy with children and all their thoughts & pleasure. Permit me to wish you very sincerely a Happy year and also to Mrs. Brown – With some of us our greatest happiness will be content mingled with patience but there is much happiness in that & the expected end[.]

Every Your Obliged Servant | M. Faraday

Address: Miss Coutts

1. Coutts's box was at the Theatre Royal, Drury Lane where Faraday later recollected that he had seen a pantomime, letter 3595. This would have been 'See Saw, Margery Daw', see *The Times*, 2 January 1857, p. 6, col. b.

Letter 3220**Julius Plücker to Faraday****2 January 1857****From the original in IET MS SC 2**

Bonn | 2 Jan 1857

Dear Sir!

I thank you heartily, Sir, for your last very kind letter¹, I got several months ago, after having exposed to you openly my feelings² with regard to Professor Tyndalls paper³. I did so convinced as I was, that you were above all such personal disputes and I fully adopt all conclusions of your letter. My intention never was to bring before the Royal Society any personal question, nor even any question of priority. I felt myself so much indebted to English men of Science that I would think it "mal placé" to provoke personal discussions, if not forced to do so. When ever I shall be so happy to see you again, I would be much interested to speak to you without any "arrière pensée". But for the moment it will be sufficient to declare to you, that I have no animosity against M T. as I think he has none against myself[.] I will not examine the motives he had, when he suggested to me ideas, which never were mines, and which I think absurd. I ask only to be the interpreter of my own words, if these words, especially when translated, admit a double meaning, or rather I ask only to restate their true meaning, laid down in many papers⁴, in the most explicit manner in a paper, whose date is anterior to Mr. Tyndalls first publication about the subject. The way how that may be done is indifferent to me.

I fully succeeded this last time to sustain the theory of magnecrystallic action by analytical calculus as well as by experiment. I am enabled know to determine by calculus the position of equilibrium of a crystal, when suspended between the two poles along any direction whatever, as well as the relative number of its oscillations in any two suspensions. And, vice versá, having determined by observation any position of the crystal, however suspended, or the relative number of its oscillations, I can calculate the position of its magnetic axes.

I wanted an biaxial crystal to verify the theory. By far the best would have been ferrocyanite of potassium. But I did not succeed to get a perfectly clear crystal of this salt large enough to furnish a sphere (10 mm or 15 m diameter); till now all my labours to get such a crystal were lost. Therefore I recurred to formicate of copper, which I made crystallise myself. Under Professor Beer⁵ valuable cooperation I had a complete success. I got very curious facts deduced by calculus and verified by experiment.

Encouraged by yourself, Sir, I think the results I obtained not unworthy to be laid before the Royal Society⁶ as a “*temoignage de ma reconnaissance*”. (Then I may in a note rectify Professor Tyndals assertions (Phil. Trans. 145 I p. 2⁷).) Only my bad English gives me some trouble, but I prefer to send an original paper incorrectly written, like this specimen then to have a german paper translated into a good English but not exactly rendering my meaning.

My best whishes for your health. With all my heart | Yours | Plücker

1. Letter 3116.
2. Letter 3109.
3. Tyndall (1855).
4. Starting with Plücker (1849), 427–31.
5. August Beer (1825–1863, NDB). Professor of Mathematics at the University of Bonn.
6. Plücker (1858f).
7. Tyndall (1855), 2.

Letter 3221

Faraday to Benjamin Collins Brodie

14 January 1857

From the original in SI D MS 554 A

Royal Institution | 14 Jan 1857

My dear Sir Benjamin,

I have added a few lines at the bottom of page 1 as you desired[.]

Will you look at the X mark of the first paragraph. I have added the words in pencil only to prevent the supposition that there was £150 for each daughter, the sense is right as it is & if you think the enlargement unnecessary, expunge it.

The statement seems to me very simple & good¹.
 Ever Yours Truly | M. Faraday

1. This letter may relate to letter 3293.

Letter 3222

Faraday to George Rennie¹

17 January 1857

From the original in NLS MS 19938/123

Royal Institution, W | 17 Jan 1857

My dear Sir,

Will you have the kindness to tell me whether the enclosed is your writing or a forgery.

Ever Truly Yours | M. Faraday

Geo Rennie Esqr | &c &c &c

1. George Rennie (1791–1866, ODNB). Civil engineer.

Letter 3223

Henry Hart Milman¹ to Faraday

21 January 1857

From the original in RI MS Conybeare Album, f. 26

Deanery St Pauls. E.C. Jany | 21

My dear Mr. Faraday,

I have received a letter from Lord Broughton², Chairman of our commission³, pressing that we should hold our first meeting on the 28th of this month. Not knowing your direction he has requested me to communicate with you. Will [you] be so kind as to write me word whether that day would suit you, or write direct to Lord Broughton –

Erlestoke | Westbury | Wilts.

The government settle our place of meeting. I am most glad to find myself your colleague[.]

Believe me | Most very truly yours | H.H. Milman

Michael Faraday Esq

1. Henry Hart Milman (1791–1868, ODNB). Dean of St Paul's, 1849–1868.
2. John Cam Hobhouse, Lord Broughton (1786–1869, ODNB). Retired Whig politician.
3. Into the site for the National Gallery.

Letter 3224**Faraday to Christian Friedrich Schoenbein****23 January 1857****From the original in UB MS NS 428**

Royal Institution | 23 Jan 1857

My dear Schoenbein,

I wished to write to you and therefore wrote to my friend Miss Hornblower whose former letter you have¹ – After a few days she wrote me enclosing a letter to Miss Schoenbein which I now send – it will explain itself & say more than I can. Your last but one letter² I also laid before Mrs. Grove³ & some others but nothing has come of it so far. Mrs. Grove was anxious to aid the cause but could find no opportunity. You will see that the philosophic part of your last has appeared in the Phil. Mag.⁴ & I trust will aid by degrees in doing the work of science: but the work is slow – Look at Ozone how beautiful it is & yet how its progress has been resisted & how little it was thought of at first – I do not know that I am doing any thing. I forget – I have been subduing gold & other metals but probably told you about that – I cannot say⁵ & I must not say more just now than to wish all happiness to Miss Schoenbein & the rest with you & the kindest thoughts to yourself from

Your friend | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | on the Rhine | Switzerland

1. See letter 3197.

2. Letter 3191.

3. Emma Maria Grove, née Powles (d.1879, age 68, GRO). Married Grove on 27 May 1837, see his ODNB entry.

4. Schoenbein (1856e).

5. Faraday did mention this in letter 3197.

Letter 3225**Faraday to James David Forbes****29 January 1857****From the original in SAU MS JDF 1857/14**

Royal Institution, London W | 29 Jan 1857

My dear Sir,

I have just received your dissertation¹ and thank you heartily for it. I have just looked at the table of contents and wonder at the work you have had to do. I already know of the expression of Your kind feelings toward myself by the proof sheets or slips that you sent me²[.]. For that I thank you & also congratulate myself because I believe that whatever you have said has been not a mere partial feeling towards myself but the execution of a duty to

that which you consider the truth of science[.] Kindness is a dear thing to me
& for all yours I most heartily thank you[.]

Ever Truly Yours | M. Faraday

J.D. Forbes Esqr | &c &c &c

1. Forbes (1857).
2. See letter 3148.

Letter 3226

William Gregory¹ to Faraday

29 January 1857

From the original in RI MS Conybeare Album, f. 15

Edinburgh | 29 Jan 1857

My dear Sir,

Dr. F. de Chaumont², formerly a pupil of mine, has begged me to give him an introduction to you. He proposes to settle in London as a medical man, and takes much interest in chemical and physical science.

You will find him a most excellent young man, and one on whom any kindness will be well bestowed.

Trusting that you are in good health, I am

Yours faithfully | William Gregory

1. William Gregory (1803–1858, ODNB). Professor of Chemistry at Edinburgh University, 1844–1858.
2. Francis Stephen Bennett François De Chaumont (1833–1888, B1). Graduated from University of Edinburgh, 1853, and thereafter an army surgeon.

Letter 3227

James Forrest¹ to Faraday

30 January 1857

From the original press copy in ICE MS letter copy book, p. 254

30 Jan 1857

Dear Sir,

May I trouble you to revise and return to me at your convenience the accompanying shorthand entry notes, of the remarks which fell from you last Tuesday evening².

Yours faithfully | James Forrest | Assistant Secretary

Professor Faraday FRS | Royal Institution | Albemarle Street

1. James Forrest (1825–1917, *Min. Proc. Inst. Civ. Eng.*, 1917, **203**: 417–19). Assistant Secretary of the Institution of Civil Engineers, 1856 to 1859.
2. That is 27 January 1857 when Faraday took part in an extended discussion of submarine telegraph cables at the Institution of Civil Engineers, *Min. Proc. Inst. Civ. Eng.*, 1857, **16**: 220–2.

Letter 3228**Peter Henry Berthon to Faraday****30 January 1857****From the original in GL MS 30108/2/78**

Trinity House, London, E.C. | 30 Jan 1857

Sir,

I am directed to transmit you, the accompanying sample of Red Lead which Messrs. Pilcher propose to provide for the service of this Corporation, and as it is important that the best red Lead should be supplied, I am directed to request you will subject it to such analysis as shall determine the genuineness of the article.

I am | Sir | Your most humble servant | P.H. Berthon
Professor Faraday

Letter 3229**Faraday to Achille Brachet¹****31 January 1857****From the original in Staatsbibliothek Preussischer Kulturbesitz****Dokumentensammlung Darmstaedter F 1 e 1831 (2). Autograph I/1478/23**
Royal Institution, London | 31 Jan 1857

Sir,

I write in haste as you desire in reply to your enquiry – I do not think that there is any manufactory of such heavy flint glass as that I made many years ago² nor do I think that such glass can be obtained commercially[.] That which I had has been gradually dispersed amongst philosophers because of its value in investigations regarding the action of magnetism on light[.] I am very sorry to have so barren an answer to give to your enquiry[.]

I have the honor to be | Sir | Your Very humble Servant | M. Faraday
Monsieur | M. Achille Brachet | &c &c &c

1. Achille Brachet (b.1820, Guyot de Fère (1859), 58–60). French physicist and microscopist.

2. See James (1991).

Letter 3230**R.W. Swinburne & Co¹ to Faraday****31 January 1857****From the printed original in RI MS F3 G253**Red Bull Wharf, 93, Upper Thames Street
London, E.C. | 31 Jan 1857

Sir,

We have the pleasure to inform you that we have completed arrangements with Monsr. Petitjean² for the exclusive use of his Patent Process for Silvering Glass with pure silver³, and that we are now prepared to silver any

Glass that may be ordered of us, or any that may be brought to us, the produce of other Manufactories.

By this process blind frames are not required, and the silvering is not affected by damp, heat, or climate.

The following is the scale of Prices for Silvering

Not above	1 foot	9d	
" "	3 feet	10d	P. foot super.
" "	8 "	1/-	
" "	12 "	1/3	
" "	16 "	1/6	
" "	20 "	1/9	
Above 20 feet, special prices			

Specimens can be seen, and every information obtained at our Address, as above.

We are | Sir | Your obedient Servants | R.W. Swinburne & Co
M. Faraday Esq

- 1. R.W. Swinburne & Co. Plate glass manufacturers of Red Bull Wharf, 93 Upper Thames Street. POD.
- 2. Tony Petitjean (d.1873, age 49, GRO under Petitjeax). Invented process for silvering glass.
- 3. Petitjean was granted patent 1855–1681 for his process for silvering glass on 24 July 1855.

Letter 3231
Faraday to William Whewell
3 February 1857
From the original in TCC MS O.15.49, f. 41

Royal Institution, London W | 3 Feb 1857

My dear Dr. Whewell

The following are references¹.

Thomson – On Poissons² *Magnecrystallic* views – Phil. Mag. March 1851, page 177³. Perhaps Thomson has something more about the matter in 1854, Comptes Rendus xxxviii pp. 632. 637⁴.

As to lines of Magnetic force you will find the *test experiments* described at p 28 of the accompanying paper Par 3351 &c. of Exp Res⁵.

Van Rees⁶ has a mathematical paper in Poggendorfs Annalen 1853, vol xc p 415⁷, – he is opposed to my views but says they give the same mathematical results⁸ as the views of Ampere⁹ or Coulomb¹⁰ or Weber¹¹[.]

Thomson says that the lines represent truly the Magnetic forces & even more simply than the representative idea of Coulomb Phil Mag 1854, viii. p 53¹²

Another paper besides that on *some points* has turned up – I send it because pp. 4.5.6.7 has reference to what we said about Gravity[.]

Ever faithfully Yours | M. Faraday

1. Which Whewell presumably needed while preparing Whewell (1857), especially 3: 521–35.
2. Siméon-Denis Poisson (1781–1840, DSB). French mathematical physicist.
3. Thomson (1851).
4. Thomson (1854b).
5. Faraday (1855b), ERE[29b], 3351–62.
6. Richard van Rees (1797–1875, BNB). Professor of Mathematics and Physics at the University of Utrecht, 1838–1867.
7. Rees (1853).
8. *Ibid.*, 433.
9. André-Marie Ampère (1775–1836, DSB). French physicist.
10. Charles Augustin Coulomb (1736–1806, DSB). French physicist.
11. Wilhelm Eduard Weber (1804–1891, DSB). German physicist.
12. Thomson (1854a), 53.

Letter 3232

Faraday to Peter Henry Berthon

4 February 1857

From the original copy in GL MS 30108/2/78

Royal Institution W | 4 Feb 1857

Sir,

Having examined the red lead which you sent me on the 30th Ultimo¹, I am in a condition to say, that there are no impurities mingled with it as adulteration;– no red oxide or iron, red earth or other of the red substances sometimes added; nor any thing to make weight[.] As a manufactured article red lead varies, so that there is better & worse, independant of the presence of impurities; but of these differences I am not a competent judge: on such points a person is required, well acquainted with the article & accustomed to compare different qualities.

I am Sir | Your Very faithful Servant | M. Faraday
P. Berthon Esqr | &c &c &c

1. Letter 3228.

Letter 3233**Faraday to William Thomas Brande****5 February 1857****From the original in BUL Newell Collection**

Royal Institution W | 5 Feb 1857

My dear Brande,

I congratulate you and Mrs. Brande¹ on the happy event of which certain cards gave me notice to day². Long may the pair live in the enjoyment of health & content and that true companionship which in married life should increase in its worth & consequences from day to day_[.]

Ever Yours Affectionately | M. Faraday

1. Anna Frederica Brande, née Hatchett (1797–1881, ODNB under W.T. Brande). Married William Thomas Brande in 1818.

2. That is the marriage of Brande's eldest son, the Anglican priest William Thomas Charles Brande (d.1891, age 69, GRO, AO) and Fanny James (d.1878, age 41, GRO) on 4 February 1857, GRO.

Letter 3234**Christian Friedrich Schoenbein to Faraday****9 February 1857****From the original in UB MS NS 429**

Bâle | 9 Feb 1857

My dear Faraday,

Not knowing exactly the direction of your friend¹ at Stamford Hill, I take the liberty to inclose a letter addressed to her by my eldest daughter and beg you to forward it to its place of destination. If the plan of Miss Schoenbein should happen to be realized, I am very glad to know her placed with an intimate friend of your's and in your neighbourhood, being sure that in Stamford Hill she will find a second home and in you and Mrs. Faraday a father and mother.

As to the girl herself, being good-natured, cheerful, healthy, active, and I may add well-informed and well-bred too, I trust she will please and suit your friend.

During our late crisis and warlike preparations² I was very busy too, but in a very quiet and harmless way. I worked very hard upon oxygen (for what else should or could I do) and think to have succeeded in ascertaining a series of novel facts such as to my opinion at least, leave no shade of doubt about the correctness of an old notion of mine, according to which common oxygen must be considered as a chemically inert body and any oxydizing action

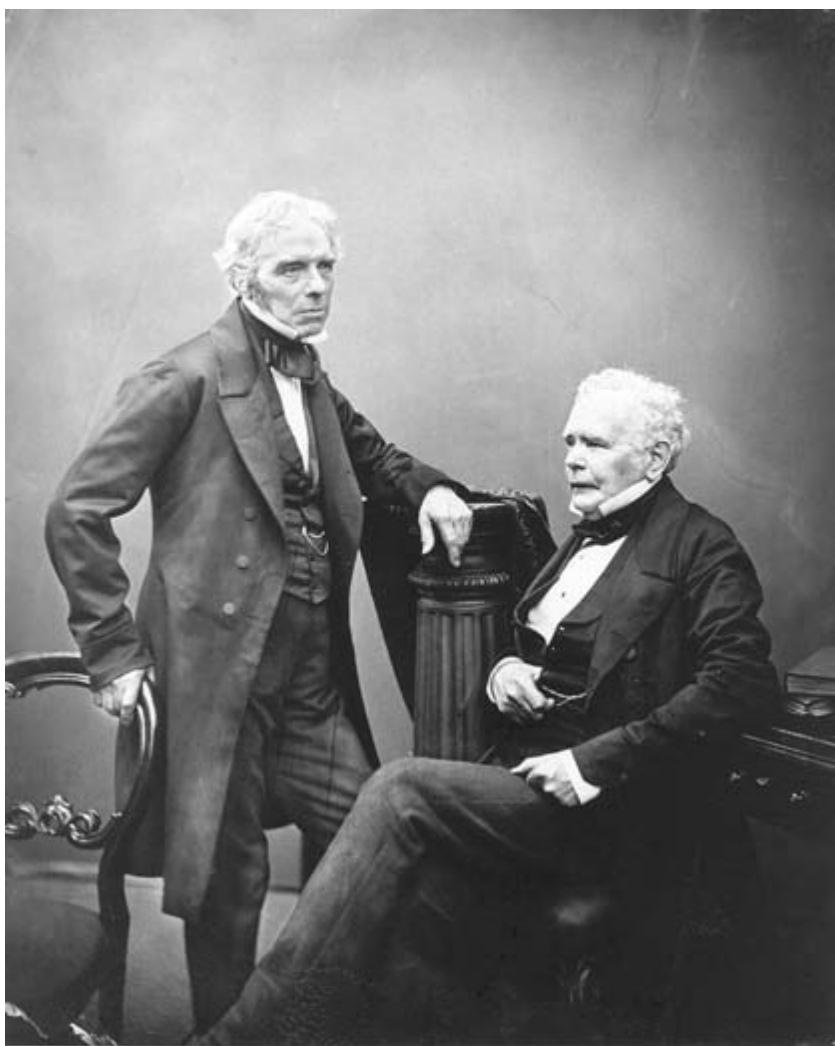


Plate 7. Michael Faraday and William Thomas Brande. From RI MS F1 I43.

apparently being brought about by O is invariably and as a *conditio sine qua non* preceded by an allotropic modification (change of chemical condition) of that elementary substance.

The facts alluded to appeared to me so simple and striking, when I saw them first, that looked for as they were, I felt an infantine joy, to which I could not help giving utterance, although I was quite alone in my laboratory. You shall know the details in my next letter, for at this present moment I have no leisure-time to write an epistolary memoir.

Amongst other little things I have found out that under given circumstances even strong acids may be chemically associated to metallic peroxides, such as PbO_2 and MnO_2 yielding, as you may easily imagine, highly energetic oxidizing solutions such indeed, as act like free ozonized oxygen.

So you see, every day a little step is made onward in my favorite study and I hope progressing still further for some time to come, for in the Ozone business much work is yet left to be done. We have hardly begun the "magisterium".

I don't know, whether you have been told that a great and wholly unexpected honor was bestowed upon Mr. Schoenbein some months ago. A gold medal conjointly with a prize of about 3500 francs has been awarded to him (by the King of Bavaria³) for his investigations on ozonised oxygen. Liebig being quite intimate with his Majesty, I suspect that our friend has not been quite strange to the matter. Be that however as it may, I cannot deny that I was highly gratified by that Royal munificence less on account of the money than of the meaning of the gift. The existence of the little baby, christened "Ozone" has been at last acknowledged even by a Monarch; now the schoolmasters must follow the Royal example.

I intend to spend the easter holidays at Munich a place which from several reasons I am exceedingly fond of and visit more than any other town. In the first place I have got there many friends of a very motley description, artists, poets, philosophers &c. and there is even a Nimrod found amongst them. *Varietas delectate*⁴. And then the Bavarian capital teems with master pieces of the fine arts, which, unartistical as I am, I nevertheless relish very much. It is indeed a great treat to me now and then to shake off from my shoulders the dust of the laboratory and store up my mind with the Images of exquisitely beautiful objects, creations certainly belonging to an order of things infinitely superior to that under which we range physical phenomena and philosophical truths. And I will not conceal it from you, that on returning to the earth from the lofty regions where Imagination reigns and rules, I feel myself a better philosopher and matter-of fact dealer, for even on the prosaic ground of palpable matter, we cannot do without that enchantress who conjures up Ideal worlds.

Now being at the end of my stories and sheet I beg you to pardon the loquacity of

Your | old and affectionate | friend C.F. Schoenbein.
Dr. M. Faraday | &c &c &c

P.S. Pray present my best compliments to Mrs. Faraday.

1. Jemima Hanbury Hornblower.
2. A reference to Prussian interference in Swiss affairs. See *Ann. Reg.*, 1856, 98: 244–7.
3. Maximilian II (1811–1864, NDB). King of Bavaria, 1848–1864.
4. ‘variety pleases’.

Letter 3235

Peter Henry Berthon to Faraday

11 February 1857

From the original in GL MS 30108/2/79

Trinity House, London E.C. | 11 Feb 1857

Sir,

I am directed to transmit to you the accompanying Sample of White Lead from Messrs. Pontifex and Wood, who have sent in a Tender to supply the quantity required for this Corporation’s Service, and to request you will be so good as to analyze the sample, and favor me with the result for the Board’s information¹.—

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday D.C.L. | &c &c &c

1. Faraday’s analysis is in GL MS 30108/2/79 with a note that he replied on 18 February 1857. This was read to Trinity House Wardens Committee, 24 February 1857, GL MS 30025/25, p. 16 which noted that Faraday had found the sample very pure and it was agreed to order white lead from Pontifex and Wood.

Letter 3236

Faraday to James David Forbes

12 February 1857

From the original in SAU MS JDF 1857/19

[Royal Institution embossed letterhead], London W | 12 Feb 1857

My dear Sir,

I published no account of my experiment on water other than that which you will find in the *Athenaeum* June 15th 1850. pages 640–1¹. It was before the publication of our Proceedings was commenced. I had no spare copies—

The librarian Mr. Vincent says that our proceedings are sent regularly to the Royal Society [of Edinburgh] Parts i and iii went through Williams & Norgate²[.] Part v [went] by post [and] they all go now by post. If you cannot find i. iii. & v. he will endeavour to supply them but would be glad if your folks will first enquire about them[.]

Ever Truly Yours | M. Faraday
Professor J.D. Forbes Esqr | &c &c &c

1. *Athenaeum*, 15 June 1850, pp. 640–1 which contains an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'.
2. Booksellers of 14 Henrietta Street. POD.

Letter 3237

William Bird Herapath¹ to Faraday

February 1857²

From the original in RS MS MC 5.311

P.S.

My Dear Sir,

There is a little misunderstanding about the dates I spoke of – 1844 refers to my diploma as a Member of the College of Surgeons[.] 1854 refers to the date of the Fellowship of the Royal Society of Edinburgh neither of these therefore belong really to the certificate itself–

The former date appears invidious to me as it would mark me out a younger man than I am having entered very late into the profession in consequence of my having spent many years in my fathers² Chemical Laboratory as his assistant Lecturer &c – Now I fancied it would be better to leave out that 1844 especially – but if this cannot be done of course there is an end of it – The other is by no means objectionable but I certainly do not see any other medical mans name with the date of his diploma as a physician or Surgeon attached to his Royal Society Certificate – I applied to the Royal Society for admission in 1853 – and I presume that is the date attached to my certificate – to which Mr. Weld alludes as being unable to interfere with⁴ –

I remain | My Dear Sir | Yours very sincerely | obliged | W. Bird
Herapath
Professor Faraday | F.R.S.

1. William Bird Herapath (1820–1868, ODNB). Bristol physician and chemist.
2. Dated on the basis that Faraday wrote letter 3238 shortly after receiving this.
3. William Bird Herapath (1820–1868, ODNB). Bristol physician and chemist.
4. Herapath was elected a Fellow of the Royal Society in 1859. Faraday nominated him from personal knowledge. RS MS Cert 9.424.

Letter 3238**Faraday to Charles Richard Weld****16 February 1857****From the original in RS MS MC 5.311**

[Royal Institution embossed letterhead] | 16 Feb 1857

My dear Sir,

Have the goodness to put up Dr. Herapaths¹ certificate² – the Old one – If any of the changes referred to in the enclosed³ are proper make them – but at all events put up the certificate[.]

Ever Truly Yours | M. Faraday
C.R. Weld Esqr | &c &c &c

1. William Herapath (1796–1868, ODNB). Analytical chemist in Bristol.

2. See letter 3237.

3. Letter 3237.

Letter 3239**George Biddell Airy to Faraday****17 February 1857****From the original press copy in RGO6/344, f. 80**

17 Feb 1857

My dear Sir,

On 1847 May 20 you were so kind as to give me an answer to a question which I had placed before you, relating to the possible preference of a pure metal or an alloy (on the score of durability) for the Standard of Length¹.

Will you permit me to quote your answer, in the Account of Construction of the Standard which I am now drawing up?²

I am, my dear Sir, | Yours very truly, | G.B. Airy
Michael Faraday Esq | &c &c &c

1. Faraday to Airy, 20 May 1847, letter 1991, volume 3.

2. Airy, G.B. (1857), 653–4.

Letter 3240**Faraday to George Biddell Airy****17 February 1857****From the original in RGO6/344, f. 81**

Royal Institution, Albemarle St. W | 17 Feb 1857

My dear Sir,

Use the communication by all means if you desire to do so. I do not remember the matter but cannot doubt that the letter had no reserve about it¹[.]

Ever Truly Yours | M. Faraday
The Astronomer Royal | &c &c &c

1. See letter 3239.

Letter 3241

Peter Henry Berthon to Faraday

19 February 1857

From the original in GL MS 30108/2/72

Trinity House, London, E.C. | 19 Feb 1857

Sir,

The Board has had reference to the inferior quality of the Samples of Oil which have been submitted to them during the past year, and deeming it expedient to review the method that has been heretofore pursued in testing the quality of Oil for their Light Houses, (the Contract for the annual supply of which will be entered into before long) have directed me to request you will be so good as to give this subject your attention, and advise them as to the arrangements you would recommend them to adopt for practically testing the quality of each Cask of Oil supplied by the manufacturer,—together with the standard test you would recommend for their adoption.—

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday D.C.L. | &c &c &c

Letter 3242

Peter Henry Berthon to Faraday

19 February 1857

From the original in GL MS 30108/2/73

Trinity House London | 19 Feb 1857

My dear Sir,

I have been called upon by the Board of Trade for the Results of any Experiments which may from time to time have been made of the relative powers of the Fresnel System of Lights as compared with the Argand Lamps & Reflectors, I do not find it recorded that we have ever had any general reports from you on the Subject, but still such may have been made and not purposely recorded, may I therefore trespass upon you with a request that you will inform me whether your attention has ever been directed specially to the comparative powers of the two systems? and if so whether you have ever reported thereon & when?

I find that amongst the Brethren a general Opinion prevails that when seen in the exact focus the Reflector is as powerful as the lens if not more so but that the latter has the advantage of more general diffusion

of Light.– and is consequently the best for *fixd* Lights, & the former for revolving,–

Believe me | My dear Sir | Yours very faithfully | P.H. Berthon
M. Faraday Esq | &c &c &c

Endorsed by Faraday: Replied with the accompanying list of references¹

1. This list of Faraday's reports is in GL MS 30108/2/73.

Letter 3243

Faraday to Miss Lyon¹

20 February 1857

From the original in FACLM H MS c1

Royal Institution, Albemarle St. W | 20 Feb 1857

My dear Miss Lyon,

I hasten to send you the tickets² with our heartiest remembrances[.]
Jeanie is not here & if she were I do not know that I should let her write:
I hope you have heard or will hear the great bell³ before you come: it is a fine
sight & has a glorious sound[.]

Ever Truly Your | faithful Servant | M. Faraday

1. Unidentified.

2. Presumably for Denison (1857), Friday Evening Discourse of 6 March 1857.

3. A reference to the new great bell (later called 'Big Ben') for the new Houses of Parliament which was then being tested in Palace Yard. See Weinreb and Hibbert (1983), 63–4.

Letter 3244

Faraday to Peter Henry Berthon

21 February 1857

From the original copy in GL MS 30108/2/72

Royal Institution, Albemarle St. W | 21 Feb 1857

Sir,

In reply to your letter of the 19th¹ I have to state that I have never had the qualities of either Spermaceti or Rape oil in regard to the particular fitness of different specimens for combustion in lamps brought before me nor am I aware that there is any ready process for distinguishing their qualities. I have always understood that the ability to distinguish better from worse has depended upon the experience of the parties judging & their

acquaintance with the run of qualities. I have not any judgment in that matter myself²[.]

I am | Sir | Your Very Obedient Servant | M. Faraday
P.H. Berthon Esq | &c &c &c

1. Letter 3241.

2. This letter was read to Trinity House Wardens Committee, 24 February 1857, GL MS 30025/25, pp. 15–16.

Letter 3245

Faraday to Henry Enfield Roscoe

21 February 1857

From Roscoe (1906), 138

Royal Institution, Albemarle St. W | 21 Feb 1857

My dear Sir,

Last night I placed a portrait of Bunsen in the Library¹, and that led me to get a friend to read the inscription beneath; and considering the case I am persuaded that I came to a false conclusion about it. Somehow I imagined that it came from Bunsen himself, but then I do not know him personally and the inscription tells me that it is a valued personal testimony to some one in particular, and I doubt not it is you. Now I should send it back to you at once by the carrier, but am not quite sure that I have your town address, so I use this letter as a means of knowing really where you are and with many thanks for your kind intention of showing it to us here, beg your pardon for my mistake and oblige

Ever, my dear Sir | Yours Very Truly | M. Faraday
Roscoe, Esq | &c &c

1. For the Friday Evening Discourse exhibition. This portrait is now in RI MS F1 H50.

Letter 3246

George Richmond to Faraday

23 February 1857

From the original in IET MS SC 2

10 York Str | 23 Feb 1857

My dear Sir,

On reaching home¹, I found that my good wife² had invited Dr. Ogle³ to tea with us, so I must even give up the pleasure of coming to you this Evn though I send the sheet of colours & vehicles.

I will call on the chance of seeing you some afternoon just to read to you some of the written notes, under the colours which you may have difficulty in making out, as the writing is not very distinct[.]

I remain my dear Sir | Your very faithful Servant | Geo Richmond

1. Following a meeting of the National Gallery Site Commission which Faraday and Richmond had attended in the afternoon. *Parliamentary Papers*, 1857, 2nd session [2261], XXIV, p. ix. The main issue was the cleaning and restoration of paintings, questions 1112–1227.
2. Julia Richmond, née Tatham (d.1881, age 69, GRO). Married Richmond in 1831.
3. John William Lister Ogle (1824–1905, ODNB). London physician.

Letter 3247**Faraday to Henry Enfield Roscoe****25 February 1857****From the original in RSC MS**

[Royal Institution embossed letterhead], Royal Institution,
Albemarle St. W | 25 Feb 1857

My dear Mr. Roscoe,

Your letter is very kind and I would on no account risk offending you¹. But I know the value of a personal remembrance & I think the portrait ought to be in its right place with you.– I think so of those which I have and should never allow a mistake to cause the displacement of the least of them. I would send the portrait back at once but fear to annoy you in some way & so shall wait until I see you face to face & then

Ever Truly Yours | M. Faraday
H.E. Roscoe Esqr | &c &c &c

1. See letter 3245.

Letter 3248**Robert Mallet to Faraday****9 March 1857****From the original in IET MS SC 2**

Delville, Glasnevin, Co Dublin | 9 Mar 57

My dear Sir,

In relation to some professional questions – it is a matter of some importance to me to obtain an *Estimate* of the *average* force of expansion of the fire damp in Coal seams – as it issues from the “Blowers” say in inches of Mercury.

From your connection in time past with the subject of Colliery explosions¹ it has occurred to me that you could probably give me some approximate data[.]

There is neither time nor opportunity for making any experiment.

The probable theoretic maximum pressure of an expanding “Blower”. I take to be that due to the *liquid* gas, at the moment it becomes gaseous, and at the temperature & pressure due to the coal seam or locality?

I think I may count upon your not grudging a crumb of knowledge – though I feel that it demands a very important object to justify any trespass upon *your* time, the value of which is not to be counted in money – against which in truth you have never weighed it_[.]

with much esteem | my dear Sir | faithfully yours | Robert Mallet
Mr. Faraday Esq FRS. | &c &c &c

1. That is the Haswell Colliery disaster in 1844. See James and Ray (1999).

Letter 3249

Faraday to Henry Bence Jones

11 March 1857

From the original in RI MS F1 E1

Wednesday Evening | 11 Mar 1857

Dear friend,

I write this note & bring it not expecting to find you. I returned alone from Brighton today and now come to ask you for something such as you gave me before. Last night my Stomach was thoroughly upset up & down – it has been pretty well during the day though with a feeling of weakness & tonight the sensation of heartburn & sickness returns so far as to bring me to you – for I have forgotten all you told me before. But if you are not at home – take no trouble tonight. A little dry toast & a *little* hot brandy & water with perhaps a little Carb Ammonia after a while if all is not quiet will I have no doubt do very well.

Ever Affectionately Yours | M. Faraday
Dr. B. Jones | &c &c &c

Letter 3250

William Henry Adcock¹ to Faraday

13 March 1857

From the original in IET MS SC 2

The Mills, Ashby de la Zouch | 13 Mar 1857

To Dr. Faraday,

Since I received your last communication, I have been reading in the 3d vol of your exp. Researches² and notwithstanding the indifference you entertain towards Swedenborgs³ theory I must say (though I fear to give offence) that the views you profess of the nature of force &c are essentially the same.

Swedenborg shows *rationaly & geometrically* the relation existing between the forces gravitation and magnetism. He says of the magnetic element that it is “the first in which elementary nature presents herself as visible

to the eye. *Here it is that she begins to emerge out of her hiding place and from darkness to issue forth into light*"⁴.

In fact he makes the magnet a stand point a key with which he unlocks the mysteries of the universe. Shows that the milky way is under the same care, is the axis of a mighty helix.

Reading your papers on lines of force⁵ I am perpetually reminded of something I have met with before and in him and what is singular the figures he has used to illustrate are many of them like yours⁶.

I am sure experiment is leading slowly and surely to his Principia. No doubt at the time it was written it appeared dark and obscure, but every fresh advance in physical knowledge has but served to illustrate and confirm its reasonings. I am astounded at the general negligence of it either it is truth or falsity. It has not been proved erroneous but it is scouted and accepted and acknowledged a truth it cannot be till more attention has been paid to it than hitherto.

I trust you will pardon my again obtruding the subject on your notice after what you have said. But I am so impressed with the way in which your thoughts tend to look for the resolution of forms of force that I cannot but think a perusal of the book would give you pleasure.

Do not let any prejudgment interfere but read it and I am sure you will agree it ought to be more generally known.

It is no dream but *strict rational* reasoning brought down at every fresh step to accord with geometry and known truths.

As an earnest of what I have said I will if you will accept it send you the 1st vol⁷.

I am Your Humble Servant W. Adcock

1. William Henry Adcock (d.1904, age 73, GRO). Swedenborgian farmer.

2. Faraday (1855c).

3. Emanuel Swedenborg (1688–1772, DSB). Swedish theologian.

4. Swedenborg (1846), 1: 211.

5. For example Faraday (1852b), ERE28 and (1852d), ERE[29a].

6. See, for example, Swedenborg (1846), 1: 271.

7. Swedenborg (1846), 1.

Letter 3251

Julius Plücker to Faraday

14 March 1857

From the original in IET MS SC 2

Bonn | 14 Mar 1857

My dear Sir!

I take liberty to send to you the announced paper on the magnetic induction of crystals¹ – not without some hesitation. Is it not rather strange that a Professor of Bonn presents so large a paper to the Royal Society?

Not being acquainted with the “coutumes” of this Society, I have written a few word to the foreign Secretary². I pray you, Sir, to send to him the paper with the letter, if this be the way. In addressing it to you, I principally wished to offer to you the occasion to look previously at the paper and especially to the note (p. 6) concerning Prof Tyndall³. I think there is no offense given to him.

Tis now exactly ten years ago, I made the first observation on the new way opened by yourselves. After many disappointments I finally succeeded – I think so at least – to give a theory of all the various phenomena and to support this theory, in verifying experimentally the numerical values derived from it. Let me refer here only to the experiments with a sphere of formicate of copper. You may directly find after some trials, what I have called the magnetic axis of this salt, a crystal when suspended along one of these axes (including an angle of 50°,) not being acted upon in an extraordinary way. You may obtain them by counting the oscillations of the crystalline sphere, when successively suspended along any two determined directions. You may get them by observing its position, when suspended along any known direction and hence deduce its position, when suspended along *any other* direction (p. 53–57)⁴. You may lastly determine them by a curious method, wick I applied to crystallised bismuth – to get exact number my sphere of formicate of copper wanted a different torsion – wire (note p. 65)⁵.

Excuse me, Sir, in giving to you so much trouble. I hope you are well. With all my heart | Yours | Plücker

PS. If there be *any* inconvenient to lay my paper before the Royal Society, I beg you to put it aside and to burn the letter to the foreign Secretary⁶.

1. Plücker (1858f). See letter 3109.

2. This letter was addressed to William Henry Smyth. See letter 3257.

3. Plücker (1858f), 545.

4. *Ibid.*, 574.

5. *Ibid.*, 579.

6. William Hallowes Miller. Faraday did send the paper to the Royal Society, see letter 3257, and it was received by the Royal Society on 26 March 1857. The manuscript has not survived in the archives of the Royal Society.

Letter 3252

Ernst Becker to Faraday

15 March 1857

From the original in IET MS SC 2

Buckingham Palace, SW | 15 Mar 1857

My dear Sir,

I had the pleasure of receiving the two copies of your lecture¹ and I have given one to The Prince. His Royal Highness, gratefully acknowledging your kindness of sending him a copy, wishes me to tell you, how much

pleasure it will give him carefully to peruse again in print the thoughts you expressed in that lecture – thoughts too deep & too momentous to be sufficiently appreciated in all their importance, when heard for the first time.

I remain, my dear Sir | Yours truly | E. Becker
Mr. Faraday, Esq

1. Faraday (1857a), Friday Evening Discourse of 27 February 1857, chaired by Prince Albert.

Letter 3253

Faraday to Carlo Matteucci

20 March 1857

From Bence Jones (1870a), 2: 382–3

Royal Institution | 20 Mar 1857

I snatch a few weary moments to have a word with you in the way of thanks for your letters of news.

...

I won't pretend to send you any news, for, when I try to remember it, all is slow to me, and you are so active and *spirituel* that it seems to me as if you were out of my sight.

Perhaps you may be inclined to say as regards gravity, and that I am out of your sight¹; nevertheless, I have a strong trust and conviction,

I am ever yours | M. Faraday

1. A reference to Faraday (1857a), Friday Evening Discourse of 27 February 1857.

Letter 3254

George Biddell Airy to Faraday

20 March 1857

From the original press copy in RGO6/471, f. 151

20 Mar 1857

My dear Sir,

I have to thank you for your kind remembrance in sending me a copy of your "Conservation of Forces"¹.

The mention of *time* in page 2² reminds me of an odd thing (having little to do with your speculations, but inducing the association connected with the word *time*) which may be worth mentioning to you. On three occasions *only* (so far as I remember) Latimer Clark has sent me notice of observed deflexions of the telegraph needles³: on those three occasions we have had magnetic disturbances; but the oddity of the thing is, that in each case our

magnetic disturbances have *followed* the *telegraph* wire disturbances by about an hour. Can you make any thing of this?

I am, my dear Sir | Yours very truly | G.B. Airy
Professor Faraday | &c &c &c

1. Faraday (1857a), Friday Evening Discourse of 27 February 1857.

2. *Ibid.*, 353.

3. See, for example, Clark to Airy, 20 January 1857, RGO6/471, f. 45 and the papers immediately following (especially f. 57 and 56) which report two further deflections in mid March.

Letter 3255

George Biddell Airy to Faraday

20 March 1857

From the original press copy in RGO6/471, f. 152

20 Mar 1857

My dear Sir,

This morning my Assistant came in in great alarm to tell me that the times of Magnetic Disturbance to which I adverted in my note¹ to you were Göttingen times and therefore exceeded Greenwich nominal times by about 40m and therefore made the Disturbance appear 40m later. In correcting the times of Disturbance for this, there is general simultaneity with the Telegraph disturbance.

I am, my dear Sir, | Yours very truly | G.B. Airy
Professor Faraday

1. Letter 3254.

Letter 3256

Faraday to George Biddell Airy

21 March 1857

From the original in RGO6/471, f. 153–4

[Royal Institution embossed letterhead],
Albemarle St. W | 21 Mar 1857

My dear Sir,

I thank you heartily for your two notes¹. It was very difficult to see what an interval of nearly an hour meant – but the identical time of the phenomena brings all together[.]

Nevertheless as an experimentalist I am thinking about the possibility of proving the existence of the Element of *time* in Magnetic phenomena:– and though we cannot look for hours or minutes or seconds, still even should it be so small as with light itself – there seems a possibility of ascertaining it.– All will depend upon the power we may have in diminishing the *time*

of *obstruction*.— I acknowledge that I have not much hope; but a proof in the affirmative would be of such extreme value to the consideration of a force acting at a distance, that I think much labour & thought would not be thrown away in trying for it, even if the results should at last be negative[.]

Ever Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. Letters 3254 and 3255.

Letter 3257

Faraday to William Hallowes Miller

23 March 1857

From the original in the possession of Hal Kass

[Royal Institution embossed letterhead],
London W | 23 Mar 1857

My dear Professor,

I have just sent a paper to the Royal Society for you – it is from Professor Plücker of Bonn one of the Foreign Members¹. In it is a letter addressed to Admiral Smyth as Foreign Secretary. It is on Magnecrystallography & contains as he tells me a perfect mathematical theory. When he told me some time ago that he was intending to send a paper to the Royal Society² I warned him that it must be original³ and he tells me that it is so. Of that & its merits you will be a far better judge than I am[.] The English is his own[.]

Ever Truly Yours | M. Faraday
Professor Miller | &c &c &c

1. Plücker (1858f). See letter 3251.
2. Letter 3109.
3. Letter 3116.

Letter 3258

Faraday to Julius Plücker

23 March 1857

From the incomplete original in NRCC ISTI

Royal Institution, London | 23 Mar 1857

My dear Sir,

Let me acknowledge, first your letter of the 2nd January¹, and now the letter² & paper³ which I received only a few days ago. The paper I have read & sent on to Professor Miller of Cambridge who is now the Foreign Secretary of the Royal Society⁴: & I have sent with it the letter to Admiral Smyth who has left the Secretaryship since his elevation in rank. Your paper appears to

me to be a very carefully elaborated work, but I am quite unable to enter into the mathematical part of it. The Formate of copper appears to have been of great service in your investigation⁵[.]

With regard to the note at p6⁶. I hardly know what to say. I understood that you & Tyndall met each other at Vienna⁷ & I was in hopes that you would have come to a thorough understanding with each other. I do not mean that you would have agreed in conclusion because every scientific man has a right to his own – but that both sides would have found reason to believe that the other did not charge him with wilful misstatement. Dr. Tyndall thinks that you have implied such a charge and says that whether you meant it or not your words conveyed the meaning in which he understood them to many others in the same sense as to himself. I suppose that as long as you say they did not convey that meaning & *could* not convey it he will think he can only clear himself from the charge of *wilfully* misrepresenting you by proving that they did convey it and so the disturbed feeling is kept up.

In the present case I should have thought that such explanations as we frequently have in our House of Commons would have been satisfactory. There when one party says they did not mean to convey a certain impression the other party is immediately satisfied that there was no intention to convey that impression. But such things as these are far more easily settled by word of mouth than by letter & that was the reason why I hoped much from your meeting at Vienna. I am very sorry for this affair I do not remember the expressions referred to and would

1. Letter 3220.

2. Letter 3251.

3. Plücker (1858f).

4. Letter 3257.

5. Plücker (1858f), 553–4.

6. *Ibid.*, 545.

7. At the meeting of the Gesellschaft Deutscher Naturforscher. Tyndall to Hirst, probably 2 October 1856, RI MS JT/1/HTYP, p. 470–1.

Letter 3259

John Tyndall to Faraday

24 March 1857

From the typescript in RI MS JT TS Volume 12, p. 4067

Royal Institution | 24 Mar 1857

My dear Mr. Faraday,

I think I ought to let you know that my feelings with regard to M. Plücker are, that he is not dealing with me in an open and upright manner. I can hardly imagine myself writing of him as he does not scruple to write of me, My relation to him has been altogether of a public kind, and he ought to deal

with it in a public manner. If the facts do not justify what I have done let him shew this, and I am willing to make reparation. But M. Plucker must feel that a reference to facts would only prove that I have been very tender of his reputation as an experimenter, and hence he resorts to a private canvass of my "motives". What guarantee have I that he does not write to all his friends as he has written to Wheatstone, where he broadly insinuates that I have unfairly influenced you against him – a charge which you know to be as unjust as it is unwarranted¹. Instead of dealing with facts in a philosophic spirit, M. Plücker deals in suspicions regarding me which arise purely out of his own constitution. I think I have reason to complain of this. If he wishes to influence you in his favour let the case be laid fully before you and I pledge myself to abide by your decision. If you can call to mind your own impressions regarding his meaning when he experimented with you here, I venture to say that they will be substantially the same as mine.

Ever yours | John Tyndall

1. See Tyndall, *Diary*, 1 April 1856, 6a: 297 and also Tyndall to Hirst, 6 April 1856, RI MS JT/1/HTYP, p. 442.

Letter 3260

Faraday to James Clerk Maxwell

25 March 1857

From the original in ULC Add MS 7655/II/6

[Royal Institution embossed letterhead],
Albemarle Street W | 25 Mar 1857

My dear Sir,

I received your paper and thank you very much for it¹. I do not say I venture to thank you for what you have said about Lines of force because I know you have done it for the interests of philosophical truth but you must suppose it is most grateful to me and gives me much encouragement to think on. I was at first almost frightened when I saw such mathematical force made to bear upon the subject and then wondered to see that the subject stood it so well – I send by this post another paper to you². I wonder what you will say to it. I hope however that bold as the thoughts may be you may perhaps find reason to bear with them. I hope this summer to make some experiments on the time of magnetic action³ or rather on the *time* required for the assumption of the electrotonic state round a wire carrying a current that may help the subject on. The time must probably be short as the time of light but the greatness of the result if affirmative makes me not despair – Perhaps I had better have said nothing about it for I am often long in realising my intentions & a failing memory is against me.

Ever Yours Most truly | M. Faraday
Professor C. Maxwell | &c &c &c

1. Maxwell (1856).
2. Faraday (1857a), Friday Evening Discourse of 27 February 1857.
3. See Faraday, *Diary*, 30 March 1857, 7: 15404–19.

Letter 3261

Francesco Zantedeschi¹ to Faraday

25 March 1857

From the original in RS MS 241, f. 147

Padoue | 25 Mar 1857

Monsieur,

J'ai l'honneur de vous faire savoir que dans la Séance de l'Institut Impl. et Royal² qui eût lieu le 23 Courant, à ma proposition, on vous élut membre de ce corps scientifique.– Je ne doute pas que l'Instt. Impl. et Royal ne soit honoré lui même de l'aggregation d'un sujet distingué et justement célèbre par des travaux Scientifiques de tous genres.

Agrèez, Monsieur, l'expression de la consideration distinguée avec la quelle j'ai l'honneur d'être

Votre très-humble Servr | Zantedeschi

TRANSLATION

Padua | 25 Mar 1857

Sir,

I have the honour of informing you that at the Session of the Imperial and Royal Institute² that took place on the 23rd of this month, on my proposition, you were elected a member of this scientific body. I do not doubt that the Imperial and Royal Institute is honoured by the addition of so distinguished a person, justly celebrated for his scientific works of all kinds.

Accept, Sir, the expression of the distinguished consideration with which I have honour of being

Your very humble servant | Zantedeschi

1. Francesco Zantedeschi (1797–1873, P2, 3). Retired Professor of Physics in Padua.
2. That is the Imperial and Royal Institute of Venice. See letter 3369.

Letter 3262

Anthony Panizzi to Faraday

27 March 1857

From the original in IET MS SC 2

British Museum | 27 Mar 1857

Dear Sir,

I have received an intimation from the Secretary of the National Gallery Site Commission¹ that it has been considered desirable you should have an

opportunity of examining the surfaces of Works of Sculptures in the British Museum, and I beg to assure you that I shall be happy to afford you every facility in my power whenever it may be most convenient for you to pursue your investigations.

Believe me, Dear Sir, Yours truly | A. Panizzi
Professor Faraday | &c &c &c

1. Henry Montague Butler (1833–1918, ODNB). Later headmaster of Harrow.

Letter 3263

François Napoleon Marie Moigno to Faraday

27 March 1857

From the original in IET MS SC 2

2 rue Servandoni | 27 Mars 1857

Mon cher Monsieur Faraday,

Je vous remercie de m'avoir envoyé de bonne heure le résumé de votre belle leçon Sur la conservation des forces¹. Je me Suis empressé de l'analyser et vous trouverez la première partie de mon travail dans la livraison du Cosmos que vous recevrez en même temps que cette lettre².

A propos de votre lecture permettez moi d'appeler votre attention Sur une exposition que j'ai faite il y a déjà longtemps dans des doctrines de Mr. Seguin Sur la cohésion³. Vous y trouverez exprimées des idées très conformes aux vôtres Sur la pesanteur et la Nature des dernières molécules de la matière. Permettez moi malgré vos nombreuses graves et Savantes occupations de vous prier instamment de lire ces quelques pages trop peu connues il me Semble. J'ose aussi vous conjurer, dussé-je paraître importun de lire dans ma traduction de la corrélation des Forces physiques de Mr. Grove⁴, l'ensemble des idées de Mr. Seguin⁵. C'est un homme excellent, un véritable philosophe, qui a fait dans l'industrie une grande fortune, dont il fait le plus excellent usage, c'est lui qui a fait toutes les dépenses de mon Cosmos jusqu'au jour où ce journal s'est suffi à lui-même. Il se désole de ne pas pouvoir appeler l'attention, et faire entrer dans le domaine de la philosophie naturelle, des doctrines qui lui semblent aussi vraies que nécessaires. Si à l'exemple de Mr. de Humboldt vous vouliez être assez bon, après avoir lu les notes additionnelles à l'ouvrage de Mr. Grove, pour lui écrire une petite lettre de sympathie et d'encouragement, vous le rendriez bien heureux, et je vous l'en serais reconnaissant comme d'un grand bienfait. Ne me refusez pas cette grâce. Personne n'est plus à main que vous de comprendre et de juger cette Synthèse philosophie; l'explication de la *cohésion* et la théorie de la *distension* Sont certainement de bonnes choses qui méritent d'être étudiées.

Si vous rencontrez Mr. Tyndall, j'oserai vous prier de lui dire que j'ai éprouvé un grand plaisir à développer sa théorie si simple et si vraie des phénomènes des glaciers⁶.

J'ai été heureux aussi de rendre justice à votre si bon ami Mr. Andrews en analysant sa note sur la décomposition bipolaire de l'eau par l'électricité de tension⁷. Mon temps est consacré tout entier à l'exposition des travaux, des découvertes et des progrès accomplis, c'est une besogne bien aride, bien fatigante, qui rapporte peu de gloire et de profits; mais il vaut souvent se dévouer et s'effacer pour le bien de la Science; et je me conforme de bon coeur à cette sublime maxime de *St. Jean: Il faut qu'il croisse et que moi je diminue*⁸. Les Savans anglais vous le Savez Sont chers à mon esprit et à mon coeur, j'ai traduit Grove avec bonheur, je traduirais avec plus de bonheur encore Vos recherches experimentales sur l'électricité, et peut être déciderai-je Mr. Séguin à faire les frais d'une traduction française quand votre travail sera terminé⁹. Je n'ai pas malheureusement vos deux premiers volumes complets, et le plus grand plaisir que vous pussiez me faire serait de me les envoyer avec votre Signature.

Je vous verrai certainement cette année en allant à Dublin prendre part aux réunions de l'association britannique.

Je Suis dans les Sentiments de la considération la plus respectueusement affectueuse et la plus distinguée

Votre très humble Serviteur | l'abbé F. Moigno

TRANSLATION

2 rue Servandoni | 27 Mar 1857

My dear Mr. Faraday,

I thank you for promptly sending the summary of your beautiful lecture on the conservation of forces¹. I hastened to analyze it and you will find the first part of my work in the issue of the *Cosmos* that you will receive at the same time as this letter².

In connection with your lecture, permit me to draw your attention to an explanation that I expounded a long time ago on the doctrines of Mr. Seguin on cohesion³. You will find expressed there ideas very similar to your own on the weight and the nature of the latter molecules of matter. Permit me, in spite of your numerous serious and scholarly occupations, to ask you urgently to read these few pages which are too little known, it seems to me. I also dare to implore you, at the risk of appearing to be a nuisance, to read in my translation of the correlation of physical forces of Mr. Grove⁴, the collected ideas of Mr. Seguin⁵. He is an excellent man, a true philosopher, who made a large fortune in industry, of which he makes the most excellent use; it was he

who covered all the expenses of my *Cosmos* until the day this journal became self-sufficient. He is sorry not to be able to draw attention, and bring to the domain of natural philosophy, doctrines that seem to him to be as true as they are necessary. If, after having read the additional notes in the work of Mr. Grove, you would like to follow Mr. Humboldt's example and be so good as to write him a little letter of sympathy and encouragement, you would make him happy, and I would be grateful for a great kindness. Do not refuse me this request. No one is more able than you to understand and to judge this philosophic synthesis; the explanation of *cohesion* and the theory of *distension* are certainly good things that deserve to be studied.

If you meet Mr. Tyndall, I beg you to tell him that I felt great pleasure in developing his simple and true theory of the phenomena of glaciers⁶.

I was also happy to do justice to your good friend Mr. Andrews while analyzing his note on the bipolar decomposition of water by tension electricity⁷. My time is dedicated entirely to the exposition of works, discoveries and accomplished progress, it is a very arid, very tiring task, which brings little glory and profit; but it is often worth devoting and effacing oneself for the good of Science; and I comfort myself cheerfully with this sublime maxim from St. John: *It is necessary that it grows and that I decrease*⁸. English Savants, as you know, are dear to my spirit and my heart, I have happily translated Grove, I would translate with greater pleasure your experimental researches on electricity, and perhaps I can persuade Mr. Seguin to cover the costs of a French translation when your work is finished. Unfortunately I do not have your first two completed volumes⁹, and the greatest pleasure that you could give me would be to send me signed copies.

I will certainly see you this year by going to Dublin to take part in the meeting of the British Association.

I am, with the sentiments of the most respectfully affectionate and most distinguished consideration

Your very humble servant | abbé F. Moigno

Address: Monsieur Faraday de la Société | royale de Londres, professeur à Royal Institution | Albemarle Street | Piccadilly

1. Faraday (1857a), Friday Evening Discourse of 27 February 1857.

2. Moigno (1857b).

3. [Moigno] (1852).

4. Grove (1856).

5. Seguin (1856).

6. Moigno (1857a).

7. [Moigno] (1857c).

8. John 3: 30. 'He must increase, but I must decrease'.

9. Faraday (1839, 1844). These were not translated into French.

Letter 3264**Thomas Henry Farrer to Faraday****31 March 1857****From the original press copy in TNA MT4/29, p. 1947**

31 Mar 1857

Sir,

I am directed by the Lords of the Committee of Privy Council for Trade, to transmit herewith a Draft on Her Majesty's Paymaster General for the sum of One hundred pounds (£100) which My Lords have ordered to be paid to you for your Services in Inspecting the Lighting Apparatus for Lighthouses in course of erection in the Colonies during the past year according to the arrangement made at the time when you first undertook this duty¹.

An adhesive Stamp should be affixed to the Draft and it should be signed by you. It may be then placed in your Bankers hands who will receive the amount from Her Majesty's Paymaster General[.]

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
Professor Faraday F.R.S. | Royal Institution | Albemarle Street

1. See letter 3096.

Letter 3265**Faraday report to Trinity House****3 April 1857****From the incomplete original copy in GL MS 30108/2/77 and *Parliamentary Papers*, 1862 (489), LIV, p. 2**

Royal Institution | 3 Apr 1857

Sir,

Though I cannot pretend to say any thing that was not evident to the Deputy Master¹ & Elder Brethren as well as myself, on the night of Tuesday last², when certain lights at Blackwall Wharf were observed from Woolwich, still it may be considered as part of my duty to offer the following observations.

The first trial was made with the two pannels (containing four lenses) of the Bishops light apparatus, which had been before examined at the Trinity house (see letter of the 19 Decr. 1856³); the object being to ascertain that the expected alternation of illumination & darkness was certain. The results confirmed that expectation & the former conclusion. Each illumination had but a moderate degree of intensity (not more than half that of the great lens was to be expected); for only a twenty fourth part of the light of the whole circle could be received on each lens; i.e. the $\frac{1}{24}$ th part of the light from the central four-wicked lamp.

Then two successive casts or faces of the Buchaness light were exhibited. Each face consisted of three Argand lamps in Parabolic reflectors; the arrangement being such that eight faces occupied the circle.— The illuminated & dark intervals were here very good & distinct, as was to be expected. The light from one face of three lamps was much better than from one lens of the Bishops light apparatus; but that was to be expected also; for now 24 lamps fill the circle & an eighth of that (or 3 lamps) sent all their rays to the eye at once; whereas when the refractor was employed, a lamp equal to only 12 or 14 Argand lamps was in the center and only $\frac{1}{24}$ th part of its light was sent to the eye in each flash.

The Bishops light pannels were then again employed, but the central lamp was changed for the electric light of Professor Holmes: An extraordinary alteration in the results was now evident. As the apparatus revolved, a beam of light issuing from it was seen to sweep round, which when it came up to the eye, struck it for an instant with a beautiful & intense light and then passed away on the other side. The brilliancy at the moment was far above that of either of the former arrangements. This was chiefly due to three circumstances; first the greater amount of light from the electric lamp than from the concentric wick lamp;— next the concentration of the light into a small space, not more than the third or fourth of an inch in diameter; and lastly the absence of any thing like the great burner, which causes so much shadow in respect of the lower rays of light from the four wicked lamp. It is the second circumstance or smallness in size of the light itself, which allows every part to be brought nearly into the focus of the lenses, causes the brilliant ray already described to be sent forward to the observer at a distance. The divergence of the issuing beam is greatly diminished and its intensity proportionately increased.— The duration of the light is of course diminished also. The power of gathering up the light into an intense brief beam, which is obtained by the electric lamp, is unattainable, as I believe, by any lamp dependant⁴ on the combustion of oil or fuel of any kind.

The electricity for this light was procured, not from a chemically acting voltaic battery, but from the power of moving magnets, in what is known as a magneto-electric machine; a two-horse steam engine is required to move it, but there is no consumption of the parts, otherwise than by ordinary wear and tear. When the lenses were removed and the electric and concentric-wicked lamps placed in sight at once, the former was evidently much the brightest, but Dr. Holmes has not as yet made any comparison of the two, or given expressions of their relative illuminating power. Being examined near at hand, I found the Electric Light very pure and white, and very steady; there was none of the variation or flickering which occurs with the *voltaic light*; what alternations existed were so small, constant, and numerous, that the result to the eye was a perfectly steady illumination⁵.

(signed) M. Faraday

1. John Shepherd.
2. That is 31 March 1857.
3. Letter 3212.
4. Hereafter the text is from *Parliamentary Papers*.
5. This letter was discussed at Trinity House Court, 7 April 1857, GL MS 30004/27, p. 39 and it was agreed that part of it would be sent to Holmes.

Letter 3266

Faraday to François Napoleon Marie Moigno

4 April 1857

From the original in AS MS Collection Bertrand

Private

Royal Institution, London W | 4 Apr 1857

My dear Abbe Moigno,

I must not longer delay answering your very kind letter¹. I have been waiting to see Grove who has been out of town on circuit but can wait no longer. I have given your message to Tyndall & will do the same to Grove very soon. Your kind expression with regard to my papers & your practical kindness is sending me the sheet of *Cosmos* of Nov 1852 containing the account of M. Seguins² views deserve my best thanks. I have been very greatly struck by the latter and cannot but rejoice to find such aid surrounding the cause of Newton & science in relation to the points of attraction both at a distance & in the case of cohesion I am glad to find myself in such good company. When I have seen the view you give of M. Seguins philosophy as set forth in your translation of *Groves Correlation*³ & I think I shall certainly see reason to do as you ask me & write to M. Seguin[.]

You say you want to be in England at the time of the Association⁴. I do not mean to wait for that however but have sent to Mr. Taylor⁵ the publishers for copies of the volumes of the *Experimental Researches*⁶ and intend to send them by Rail way as soon as I can. You complement me greatly in wishing to have them. But I cannot understand what you say about translating them – for I am sure it would never pay you & I should be sorry that your good will & kindness should cause expence & inconvenience to any one⁷. I know your kindness & that of all my friends in France without any such serious proof as that.

I must leave this paper for I am fatigued and busy – and slow[.]

Ever My dear Abbé | Yours very faithfully | M. Faraday

1. Letter 3263.
2. [Moigno] (1852).
3. Grove (1856); Seguin (1856).
4. The meeting of the British Association in Dublin.

5. Richard Taylor (1781–1858, ODNB). Publisher and one of the editors of the *Philosophical Magazine*.
6. Faraday (1839, 1844, 1855c) of which Moigno, in letter 3263, asked for the first two volumes.
7. They were not translated into French.

Letter 3267**Faraday to William Robert Grove****4 April 1857****From the original in RI MS G31**

[Royal Institution embossed letterhead]
 Albemarle St. W | 4 Apr 1857

My dear Grove,

Have you a copy of Moigno's translation of Your Correlation &c¹? if so can you lend it to me Moigno refers me to it for Seguin's views of the forces of matter²[.]

Ever Truly Yours | M. Faraday

1. Grove (1856).

2. Letter 3263; Seguin (1856).

Letter 3268**Faraday to William Robert Grove****6 April 1857****From the original in RI MS G32**

[Royal Institution embossed letterhead]
 Albemarle St. W | 6 Apr 1857

My dear Grove,

I have many thanks to offer you for the Forces¹. I take it with me tomorrow as a holiday in the Country air – I hope my note² has not (unwittingly on my part) laid you under contribution. It was written because of Moigno's letter to me³[.] You must be sure of the value of the volume as a remembrance to me:– but still if when I return you have reason to let me know you have need of a copy in another direction do not hesitate to let me know.

Ever Truly Yours | M. Faraday

1. Grove (1856).

2. Letter 3267.

3. Letter 3263.

Letter 3269**Peter Henry Berthon to Faraday****8 April 1857****From the original in GL MS 30108/2/77**

Trinity House, London, E.C. | 8 Apr 1857

Sir,

I am directed by the Elder Brethren to convey to you their Thanks for your Report on the result of the examination of the several Lights exhibited at Blackwall on the Evening of Tuesday the 31st ultimo¹.— and with reference to the Observations you have offered on the apparatus which it is at present intended to fit up at the Light House on the Bishop Rock,— I am to request you will favor the Elder Brethren with your opinion, whether a Light exhibited with that Apparatus would under ordinary circumstances, be found as effective for the purpose of illuminating a widely extended Sphere as with one composed of Lenses of the usual magnitude, or be as powerful and distinctive as one exhibited upon the ordinary system with Argand Lamps and Parabolic Reflectors.—

I am | Sir | Your most humble Servant | P.H. Berthon
 Professor Faraday | &c &c &c

1. Letter 3265.

Letter 3270**Faraday to Peter Henry Berthon****11 April 1857****From the original copy in GL MS 30108/2/77**

Royal Institution | 11 Apr 1857

Sir,

Your letter of the 8th instant¹ requires my opinion whether a light exhibited with the 24 lens or Bishops rock apparatus would under ordinary circumstances be found as effective for the purpose of illuminating a widely extended sphere as the light from one composed of lenses of the usual magnitude i.e. of eight to the circle_[.] In reply I have to state that in my opinion it would not. The 24 lens apparatus aided by refracting reflectors above & below would send forth 24 beams at once in different directions towards the horizon whilst the apparatus of eight ordinary lenses would send forth only eight beams_[.] I have already given the reason in my letter of the 19th Decr. 1856² why we should expect that the brilliancy of the latter should be at least twice as great as the former.

You also enquire whether the light of the former or Bishops rock apparatus will be as powerful & distinctive as one exhibited upon the ordinary system with Argand lamps & parabolic reflectors: I think not and have given what appeared to me to be the fact & reasons in my last letter of the 3rd instant³. These reflectors with their three Argand lamps will send forth much more light in one common beam than a refracting lens collecting at the utmost $\frac{1}{24}$ part of a lamp light equal to about 14 Argands. There is nearly five times as much light in the one case as in the other & though the refractor gives a ray of less divergence than the reflectors still the improvement in that respect is not to such a degree as to compensate for the small quantity of light employed. To my eye the three reflectors much surpassed the Bishops light refractor on the evening of the 31st March last⁴.

I am Sir | Your Most obedient Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letter 3269.

2. Letter 3212.

3. Letter 3265.

4. This letter was read to Trinity House By Board, 14 April 1857, GL MS 30010/41, p. 3 when it was agreed to defer consideration until the following meeting. Trinity House By Board, 21 April 1857, GL MS 30010/41, p. 19 also deferred discussion.

Letter 3271

Charles Manby to Faraday

16 April 1857

From the original press copy in ICE MS letter copy book, p. 486

16 Apr 1857

My dear Sir,

We are to have a paper next Tuesday evening by Mr. Robert Hunt¹ "On Electro-magnetism as a Motive Power"².

May we hope to have the pleasure of your attendance on that occasion? I trust that we shall be so favoured, & that you will give us your views on this interesting question, in the course of the discussion³.

Yours faithfully | Charles Manby | Secretary
Professor Faraday FRS | Royal Institution | Albemarle Street | Piccadilly

1. Robert Hunt (1807–1887, ODNB). Lecturer on mechanical science at the Royal School of Mines from 1851 and on experimental physics from 1853.

2. That is 21 April 1857 when Hunt (1857) was read.

3. No evidence has been found which indicates whether or not Faraday attended the meeting at the Institution of Civil Engineers.

Letter 3272**John Tyndall to Faraday****18 April 1857****From the typescript in RI MS JT TS Volume 12, pp. 4068–9**

Saturday, 18 Apr 1857

My dear Mr. Faraday,

For some time past the thought of writing to you on a subject of some importance to myself has occurred to me at intervals, and while in the country lately I was only prevented from doing so by the fact of having other matters to deal with which left me no time for writing.

When I concluded my last course of lectures I felt so weary that I looked with some dismay on the course to come after Easter, and did I not know that a change of arrangement at the time would be practically impossible, I should certainly have endeavoured to transfer the burden to stronger shoulders. I am now comparatively rested, but still I think I ought to endeavour to make some other arrangement for years to come¹.

Far even as they fall below the standard which I should like them to reach, I find 19 lectures to one audience to be a work of such labour that I would willingly shorten it if possible. They consume half the year in delivery, and an additional portion of the year in thinking of them, thus leaving me a comparatively small amount of time, and no great stock of strength for original research. If I thought that the labour of lecturing would sufficiently diminish with practice, I should hardly write thus; but four years experience warns me that the respect which I feel to be due to our audience would prevent me from ever dealing lightly with the lectures, and would always urge me to seek after matter suited to their taste and comprehension.

Under these circumstances I am inclined to think that it would be better for me, and I hope also better for science, if I occupied a position as regards the number of lectures to be given, and the remuneration to be derived from them, similar to that occupied by Mr. Brande during his connexion with the Institution. And if you see nothing objectionable in a proposal to this effect, I should be very thankful to you if, at the proper season, you would have the kindness to lay it before the Managers².

Believe me | dear Mr. Faraday | Yours most faithfully | John Tyndall

Endorsement: read to the Managers on (I think) the 27th May following
MF | Managers' attention drawn to it again 15th Feby 1858. MF³

1. Tyndall had delivered eleven lectures on sound from 22 January to 2 April 1857 (RI MS Le4/196) and would deliver a further eight on the same subject from 23 April to 11 June 1857 (RI MS Le4/201).

2. When Tyndall and Faraday discussed this letter, Tyndall understood that Faraday would bring about the change he had asked for. Tyndall, *Diary*, 22 April 1857, 6a: 427.

3. There was no meeting of managers on 27 May 1857 and RI MM, 15 February 1858, 11: 215–7 does not refer to the issue. But between 21 January and 29 April 1858 Tyndall delivered thirteen

lectures on heat (RI MS Le4/207 and 220) while between 27 January and 14 April 1859 he delivered twelve lectures on gravity (RI MS Le4/217).

Letter 3273

Faraday to François Napoleon Marie Moigno

22 April 1857

From the original in National Maritime Museum MS GAB/5

Private

Royal Institution, London | 22 Apr 1857

My dear Abbe Moigno,

To day I send off by the Railway Company three volumes of the Experimental Researches¹ about which you enquired² and which I trust you will do me the favour to accept – I send them by the Railway Company addressed to your name at 2 Rue Servandoni and I hope they will arrive safe. I have read your translation of Groves volume³ and the matter added by M. Seguin⁴. The latter has interested me exceedingly and will keep my thoughts going for some time but it requires a great deal of thought to compare such a view with ones accustomed notions & ideas of matter especially if one tries to carry it out in relation to the different varieties of matter and their chemical and electrical relations one to another – and my thoughts are now very slow and soon weary by exertion. It is wonderful to the mind when one endeavours to form a conception of matter & of force – it would be still more wonderful if those who have dealt with these things did not strive to form a conception and though we cannot hope in this life to know the beginning or ending of these things still we may hope to develop an extra link in that part of the chain of reasoning which comes within our comprehension. At the same time I must confess my feeling of great insufficiency in these matters and am constrained to hold my views under continued subjection & cross questioning and not having the honor of knowing in the slightest manner M. Seguin I dare not intrude upon him my very hesitating views – or give any expression to the feeling of great pleasure which I have had in reading his vigorous & as it seems to me philosophic view. It is very encouraging to see how men's minds are moving[.] These things cannot be developed quickly & M. Seguin must not be disheartened – It requires a generation to pass away that prejudices may die out with it & though that has happened since the time of Montgolfier⁵ still the change required is of such extent that the dissolution of another may be needed before the mind is freed from her trammels[.]

Believe me to be | Your faithful Servant | M. Faraday

1. Faraday (1839, 1844, 1855c).

2. In letter 3263. See also letter 3266.

3. Grove (1856).

4. Seguin (1856).

5. Joseph Michel de Montgolfier (1740–1810, DSB). Balloonist and engineer. His work is discussed in Seguin (1856), 267–70.

Letter 3274

Faraday to Anthony Panizzi

23 April 1857

From the original in BM CA

[Royal Institution embossed letterhead],
Albemarle St. W | 23 Apr 1857

My dear Sir,

I should be glad to be able to examine some of the Marbles in the Elgin gallery tomorrow (Friday) and purpose being at the place about 11 o'clk for that purpose if convenient to you¹[.]

Ever Truly Yours | M. Faraday

Panizzi Esqr | &c &c &c

1. For outcome of Faraday's visit, which was for the National Gallery site commission, see letter 3278.

Letter 3275

Christian Friedrich Schoenbein to Faraday

25 April 1857

From Kahlbaum and Darbishire (1899), 282–3

Basle | 25 Apr 1857

My dear Faraday,

I write you these lines to ask you the favor of letting occasionally know your Friend at Stamford-hill¹, that, about the time indicated by her, my daughter will keep herself ready for her projected journey to England and hope that some proper travelling companions will in the right season make their appearance at Basle, under whose guidance Miss Schoenbein may safely be carried over to London.—The last holidays I passed at Munic and as you may easily imagine in a very agreeable manner, though in consequence of having sprained my leg I was kept in bed there for nearly a week. Liebig and many other scientific and artistic friends were at home, so that there was no want of rational enjoyments in the bavarian capital. Amongst the curiosities seen there, there was a specimen of the poisoned chinese bread sent to Liebig for chemical analysis. He found considerable quantities of Arsenic in it. Of late I (have) worked again a good on my favorite subject and ascertained a number of facts which seem to me to put beyond doubt the correctness of that old notion of mine, according to which oxygen, such as it exists in the atmosphere, is in a chemical point of view an entirely inert body and any oxidation being apparently brought about by it, is preceded by its allotropic change

of condition. The essential oil of bitter Almonds (Benzule) has become to me a beautiful object of research, which substance, being assisted by solar light, enjoys the power of rapidly taking up common oxygen and transforming it into that state, in which it produces all the oxidizing effects of ozonised oxygen decomposition of iodide of potassium, coloring blue the solution of guajacum etc. Leaving that oxygen associated to the oil, it rather rapidly oxidized that matter into hydrated benzoic acid. In presenting my best compliments to Mrs. Faraday and begging you to drop the inclosed into a letter box I am my dear Faraday

Yours | most truly | C.F. Schoenbein

1. Jemima Hanbury Hornblower.

Letter 3276

Faraday to Henry Bence Jones

27 April 1857¹

From a transcription in RI MS

Royal Institution | Monday, 27, 1857

My dear friend,

I have seen Mr. Leslie² – & obtained the Articles of Partnership – I send them herewith with my conclusion – I will call on you tomorrow morning if convenient at 9 o'clk or soon after – & then take them or any thing you may approve of to Mr. Leslie.

Ever Yours | M. Faraday

Dr. Bence Jones | &c &c &c

1. Dated on the basis that this was the only Monday during 1857 on which the 27th fell.
2. Unidentified.

Letter 3277

Josiah Latimer Clark to Faraday

29 April 1857

From the original in IET MS SC 2

35 Adelaide Road, Haverstock Hill NW | 29 Apr 1857

My dear Sir,

I have lately met with some very strange phenomena which I am quite unable to understand and which I think it to be almost a duty to make known to you. They are not scientific phenomena but yet may perhaps be worthy of scientific investigation. My brother-in-law Capt. Drayson¹ of the Royal Artillery at Woolwich a very clever and clear headed and sensible man first told me of them – he said he had been invited by a mutual friend to a private house at Blackheath to witness what he called "*spirit rapping*"! and other

similar phenomena. Like every sensible and well informed person he laughed at the idea, but his friend (a Captain R.N.) was so positive that there was no delusion in it that he went, and to his surprise was *forced to believe* in the phenomena he witnessed. He then begged an interview with me, and I was earnestly requested to attend some evening as they feared they might be done by some electrical legerdemain which from my familiarity with such subjects I should be able to detect. I went three times and abundantly satisfied myself that there was nothing electrical about it.

What I witnessed was as follows, a succession of gentle taps given apparently beneath the tables round which we were sitting, and *guided by intelligence* – so that by a preconcerted request they would reply to any question asked of the tables. Thus in asking any question and then distinctly repeating the alphabet they would tap thrice at the letter they wished to indicate and by a repetition of this process they would spell complete words and sentences[.]

I am quite sensible of the apparent nonsense I am writing & the credulity of science – Nothing on Earth would ever have induced me to believe or to make such *ridiculous statements* except the most undeniable proof and demonstration – and I did not believe until I had satisfied myself that *deception was impossible*.

The other phenomena I saw were equally strange but one only struck me as interesting in a scientific point of view – I saw heavy tables half lifted off the floor by the contact of the point of the fingers only, or sometimes take a sudden lurch pressing against me with great force, and I am credibly assured by Capt Drayson (whose veracity is beyond question) that on two occasions he has seen a small round table remain suspended in the air 6 inches from the ground for two or three minutes occasionally rising & falling elastically.

At the house of a friend to which they had been invited for the first time I saw them experiment on a very heavy and old fashioned black mahogany table which had never been seen by them before – this moved about the room with some considerable force, but more singular still I saw its centre (with a lamp & glasses &c that were upon it) apparently bend and heave and undulate up and down like a miniature Earth quake. On after examination I found it to be still as hard and rigid as ever.

These are the sort of phenomena I have witnessed and I am requested to invite you to attend and witness them privately if you wish to do so, and they are so mysterious that I think every one ought to see them once in their lives at least. At first I regarded them as supernatural, in fact as miracles but they produce the same effects as often as they choose – night after night & therefore they must be formed by certain *laws* & not be miracles – The suspension & overpowering of the laws of Gravity strike me as being very curious. If you are inclined to witness them I shall be glad to accompany you privately to Blackheath where you will receive every welcome. You have my assurance

that the family is a highly respectable one & living in very comfortable style – their names are Mr. and Mrs. Crosland² – they have no children and are highly educated & very matter of fact people – They would like you to see them at least twice, but as Mrs. Crosland will be going to the sea side shortly (and perhaps to the continent) for the benefit of her health it is necessary to make an early appointment or the opportunity will be lost. If you decide on going I should like to see you previously at the Royal Institution & will call on you any day you choose. One thing at least I must beg of you – do not think that I have lost my senses *or that I have been deceived* – I do not understand the nature of the phenomena, but their existence is beyond question[.]

Yours very sincerely | Latimer Clark
Professor Faraday

PS. I ought to add that the question is regarded by them in a very religious light, and they always commence & end by prayers – especially by secret prayer. There is however nothing very serious or impressive in witnessing the phenomena – the conversation goes on in the most ordinary & animated way.

Endorsed by Faraday: Spirit rappings from Latimer Clark

Address: Professor Faraday | Royal Institution | Albemarle Street

1. Alfred William Drayson (1827–1901, *Month. Not. Roy. Ast. Soc.*, 1902, **62**: 241–2). Royal Artillery captain (later a general) and story writer.

2. Newton Crosland (1819–1899, ODNB under C.D. Crosland). Wine merchant who married, in 1848, the writer and spiritualist Camilla Dufour Toulmin (1812–1895, ODNB).

Letter 3278

Faraday to Henry Hart Milman¹

30 April 1857

From *Parliamentary Papers, 1857, 2nd session [2261], XXIV, p. 149*

Royal Institution, Albemarle Street, W | 30 Apr 1857

My dear Dr. Milman,

I wish I could write anything satisfactory, in reply to your note about the marbles in the British Museum. I examined them, in respect of their condition as to dirt, on the 24th instant²; and more particularly a Caryatide, No. 128; the Shaft of a Column, No. 118; and some of the Metopes in the Elgin Gallery. The marbles generally were very dirty; some of them appearing as if dirty from a deposit of dust and soot formed upon them, and some of them, as if stained, dingy, and brown. The surface of the marbles is in general rough, as if corroded; only a very few specimens present the polish of finished marble: many have a dead surface; many are honeycombed, in a fine degree, more or less; or have shivered broken surfaces, calculated to hold dirt mechanically.

I found the body of the marble beneath the surface white. I found very few places where the discolouration seemed to be produced by a stain penetrating the real body of the unchanged or unbroken marble. Almost everywhere it appeared to be due to dirt (arising from dust, smoke, soot, &c.) held, mechanically, by the rough and fissured surface of the stone.

The application of water, applied by a sponge or soft cloth, removed the coarsest dirt, but did not much enlighten the general dark tint. The addition of rubbing, either by the finger, or a cork, or soft brushes, improved the colour, but still left it far below that of a fresh fracture. The use of a fine, gritty powder, with the water and rubbing, though it more quickly removed the upper dirt, left much imbedded in the cellular surface of the marble.

I then applied alkalis, both carbonated and caustic; these quickened the loosening of the surface dirt, and changed the tint of the brown stains a little; but they fell far short of restoring the marble surface to its proper hue and state of cleanliness. I finally used dilute nitric acid, and even this failed; for, though I could have gone on until I had dissolved away the upper marble, and left a pure surface, even these successive applications, made, of course, with care, but each time producing a sensible and even abundant effervescence, and each time dissolving enough marble to neutralize the applied acid, were not sufficient to reach the bottom of the cells and fissures in which dirt had been deposited, so as to dislodge the whole of that dirt from its place.

The examination has made me despair of the possibility of presenting the marbles in the British Museum in that state of purity and whiteness which they originally possessed, or in which, as I am informed, like marbles can be seen in Greece and Italy at the present day. The multitude of people who frequent the galleries, the dust which they raise, the necessary presence of stoves, or other means of warming, which, by producing currents in the air, carry the dust and dirt in it to places of rest, namely, the surfaces of the marbles; and the London atmosphere in which dust, smoke, fumes, are always present, and often water in such proportions as to deposit a dew upon the cold marble, or in the dirt upon the marble, are never-ceasing sources of injury to the state and appearance of these beautiful remains. Still, I think that much improvement would result from a more frequent and very careful washing; and I think that the application of a little carbonated alkali (as soda) with the water, would be better than soap, inasmuch as the last portions of it are more easily removed. It requires much care in washing to secure this result; but whether soap or soda be employed, none should be allowed to remain behind.

Dry brushing or wiping is probably employed in some cases; if so, it should be applied with care, and never, whilst the objects are damp, or from the conditions of the weather likely to be so. In several cases there is the appearance as if such a process had resulted in causing the adhesion of a

darker coat of dirt than would have been produced without it; for convex, front, underlying portions of a figure are in a darker state than back parts of the same figure, though the latter are more favourably disposed for the reception of falling dirt³.

I am, my dear Dr. Milman | Humbly and truly yours | W. Faraday [sic]
The Very Rev. the Dean of St. Paul's, | &c &c &c

1. Henry Hart Milman (1791–1868, ODNB). Dean of St Paul's, 1849–1868, and a member of the National Gallery site commission.

2. See letter 3274.

3. Faraday undertook this examination of the Elgin marbles for the National Gallery site commission.

Letter 3279

Faraday report to Trinity House

1 May 1857

From *Parliamentary Papers*, 1862 (489), LIV, pp. 2–5

1 May 1857

[1. I [Holmes] will endeavour to answer your letter of 22d instant in the best manner I can, hoping at the same time that the Elder Brethren will be satisfied with my answers as far as they go. As, without seeing the particular locality, it would be impossible, perhaps, to say in that particular case how I would adapt the machine. But in hardly any possible case can there arise any real difficulty in adapting the machine, as the light may (where necessary) be at a considerable distance from the machine without much lessening the amount of light.]¹

1. In order to present such observations upon Professor Holmes' letter, as arise in my mind, I have numbered the paragraphs of his communication, and hope that a reference to them by figures will be clear and sufficient.

[2. The cost of a single machine and steam engine would be about 500*l*. The steam engine supplied with the machine would be of the most simple construction, acting directly on a crank fixed to the axle of the machine. No strap or band would be employed.]

2. The cost and arrangement of the machine can only be tested by practice.

[3. The expense of the light would be about from 1½*d*. to 3*d*. per hour, for coal and carbons, and the pay of a skilled workman until the ordinary light-keepers would be sufficiently instructed by him to maintain the light without his assistance.]

3. The expense of the light can only be learned by a continued practical trial.

[4. If the Elder Brethren will allow us to place an Electric Light in any lighthouse where there would not be any great expense incurred for building, &c. for (say) three months, we will be at all expenses (and the ordinary lights need not be disturbed during that time) on condition that at the expiration of the three months the Corporation will adopt the light generally in their lighthouses if it fulfils the following conditions to *their entire satisfaction*, viz.]

4. It would not be safe to give any pledge of general adoption; the application might be very difficult and uncertain in the results at out-of-the-way stations. Better far to pay a sum for expenses of trial, and remain at perfect liberty to reject or apply the lamp according to circumstances. The Trinity House only can be the safe, as they are the responsible, judges for any and every case.

[5. First, that it costs less than the ordinary lights.]

5. Does this statement mean that for the same amount of light it will cost less? or that the lights, referred to in pars. 3 and 8, as the Electric Lights, cost less than the first class three-wicked lamp referred to in par. 8, or does it mean that the sum of all expenses, including interest of capital invested, wages of intelligent workmen, fuel, &c., will be less for the electric lamp of *fivefold* light power (8 & 9), or less for an *equal* proportion of light power?

[6. Second. That it is more effective in bad or foggy weather.]

6. No doubt true for the fivefold light power.

[7. Thirdly. That during that time it is not stopped on account of accident for any time equal to that taken to trim an ordinary lamp.]

7. In relation to this condition, will it not be necessary to have a second magneto-electric engine ready to supply the light, when, in course of time or by accident, the one in use might be thrown out of employ? Would not a second steam-engine be required for the same reason? Manufacturers are accustomed to keep an extra boiler ready for work, with steam engines, on such occasions.

[8. I will here, sir, beg you to lay before the Elder Brethren the results of certain photometric experiments made with a view to ascertain the comparative powers of a first-class three-concentric wick lamp and our Electric Light, as it will serve as a demonstration of the assertion made above.]

[9. The first experiment was for the comparison of the *quantity* of light given off by each, and was made at 12 feet distance. The results were—

Quantity of Light	
Oil lamp	1
Electric ditto	$5\frac{1}{2}$

As the distance was increased so did the difference increase also; and thus I was enabled to arrive at the *intensity*.

Quantity of Light

Oil lamp	1
Electric ditto	5,625]

8 and 9. Can say nothing about the estimate; it is an experimental result. But the light I saw was considerably greater than that of the French lamp.

[10. By *intensity* is to be understood that property of light which enables it to penetrate to a distance; and, taking the above example, suppose that the two unassisted lights are viewed from a distance, on a foggy night, and that it is found that the oil lamp could usefully penetrate that fog to the distance of 100 yards, then the Electric Light would be equally visible at the distance of $100 \times \sqrt{5,625} = 7,500$ yards.]

10. Needs a practical result for its illustration.

[11. No *quantity* can ever make up for the want of intensity. For were there a hundred of such oil lamps employed, where one would become invisible, from distance or fog, *each one* would equally become so.

Intensity, therefore, and not *quantity*, is that property required in a lighthouse, and this property exists only in the Electric Light to any great degree.]

11. Not altogether so. If it were, then a common Argand lamp would be visible as far off as a concentric wick lamp; and six lamps and reflectors in one cant of a revolving apparatus would not penetrate, to a seaman's observation, further than one lamp and reflector. Without reasoning on intensity and quantity, what is wanted is, simply, *that light* which is either more luminous or more visible at greater distances than another, to the eye of an ordinary observer.

[Holmes: 12. When the Electric Light is employed with a reflector, the quantity of light thrown off from it is much greater than when an oil lamp is used. Because, as the reflector only repeats the light of any lamp in proportion of – area of flame, divided into area of reflector – and as our area of flame is more than a thousand times less than the area of the oil lamp, the light given off would be a thousand times greater, even though the original *unassisted* lights were equal in quantity.

(signed) F.H. Holmes]

12. Not so. The quantity of light reflected is not *in greater proportion*. The good effect (which is undoubted) depends upon the power of diminishing the divergence, and throwing almost all the light into one intense central beam, instead of spreading it into a wider, weaker beam.

I hope a situation may be selected where the magneto-electric lamp can be safely and effectually tried, for a time and under circumstances during which all the liabilities may be thoroughly eliminated. The light is so intense, so abundant, so concentrated and focal, so free from under shadow (caused in the common lamp by the burner), so free from flickering, that one cannot but desire that it should succeed. But it would require very careful and progressive introduction; men with peculiar knowledge and skill to attend it; and the means of instantly substituting one lamp for another in case of accident. The common lamp is so simple, both in principle and practice, that its liability to failure is very small. There is no doubt that the magneto-electric lamp involves a great many circumstances tending to make its application more refined and delicate, but I would fain hope that none of them will prove a barrier to its introduction. Nevertheless, it must pass into practice only through the ordeal of a full, searching, and prolonged trial².

(signed) M. Faraday

1. The text in the square brackets contain Holmes's letter to Trinity House. In the text Faraday's response was printed after each section.

2. This letter was read to Trinity House Court, 5 May 1857, GL MS 30004/27, p. 61. It was agreed to that Trinity House could not generally adopt Holmes's light, but would contribute to the cost of trials and also that a copy should be sent to the Board of Trade.

Letter 3280

Josiah Latimer Clark to Faraday

2 May 1857

From the original in IET MS SC 2

35 Adelaide Road, NW | 2 May

My dear Sir,

I have inclosed your note and Dr. Tyndall's to Mr. Crosland¹ and I feel confident he will be extremely pleased to see Dr. Tyndall there, unless it should happen that they have sent out invitations too numerous to day since receiving a note from me to the effect that you and I were not going to attend. If Dr. Tyndall goes I can assure him that he will be most intensely interested².

The phenomena have now been witnessed by so many & have got so generally known that they are quite unable to gratify more than one twentieth of those who request permission to attend. But they are at the same time only too glad to get the attention of men of character and science who are competent to investigate the phenomena and willing to receive facts with an unbiassed mind.

I have heard of the subject abundantly and for a long time past but set it all down as a mass of self-delusion or intentional trickery. I was therefore greatly astonished when after a *careful examination* I discovered the facts to

be real. Nevertheless I have little intention of pursuing the subject – I do not as yet see the use & tendency of it. I am satisfied that it is often if not generally mixed up with much that is delusive & bad – in plain English I believe more of the communications come from bad spirits than from good ones. And I believe it is capable of producing a degree of mental excitement very analogous to *insanity* in weak minds it is therefore not a thing to be trifled with. At the same time I find much that is beautiful in it & that harmonises wonderfully with the truths of Sacred Scripture.

Yours very faithfully | Latimer Clark
M. Faraday Esqr | &c

Endorsement: Enclosure³

1. Newton Crosland (1819–1899, ODNB under C.D. Crosland). Wine merchant who married, in 1848, the writer and spiritualist Camilla Dufour Toulmin (1812–1895, ODNB).
2. Tyndall, *Diary*, 8 May 1857, 6a: 437 noted that he attended a séance at the Croslands. See also entries for 9 and 10 May, pp. 437–8. See letter 3277 for the origin of this invitation.
3. That is Crosland's letter to Clark which reads:

3 Hyde Vale Cottages, Blackheath, S.E. | 30 Apr 57

My dear Sir,

In the absence of my husband I have opened your letter to him, & for the purpose of saving a post reply to it.

The Spirits named Tuesday the 5th, or Friday the 8th of May, as days on either of which Mr. Faraday might be invited. The reason we pressed for an answer was that half a dozen people are waiting to know when they can come, and we wish to fix whichever day you & Mr. Faraday reject:— Our dinner hour is half past six – In very great haste believe me my dear Sir
yours very truly | Camilla Crosland

Letter 3281

Herbert Spencer¹ to Faraday

4 May 1857

From the incomplete original in IET MS SC 2

The eternity of Force and not of forces

Economist Office Strand | 4 May 1857

Dear Sir,

Being much interested in the subject of your late lecture on the conservation of Force² I respectfully ask you to consider, if you can possibly spare time, the following remarks.

You have doubts whether the *considerations* you advance are metaphysical or physical, but as you state that "*we know matter only by its forces*"³, you can I think have no doubt that the *subject* you discuss is purely metaphysical. It concerns the convictions of mind of which all the discoveries of the chemist and all the calculations of the mathematician are parts and consequently embraces them all[.] It does not concern any properties of matter

which we learn by observation and by our senses, but something beyond all properties, viz the possible annihilation or eternal existence of matter. This is not a question of sense, of course it is not a question of physics, and therefore is metaphysical[.] As such I treat it[.]

You "agree with those who admit the conservation of force to be a principle in physics as large and *as sure* as that of the indestructibility of *matter*"⁴, but as you also say that we know matter only by its forces, you imply that these *forces* are indestructible and you argue against the common mode of expressing the law of gravity, that it admits a ["]creation of power to an enormous amount" and "an annihilation of force"⁵. The point to which I beg to call your attention is the difference, apparently a very slight one, between the annihilation of force and the annihilation of forces, but which is all important. We continually find forces, as electricity and magnetism, lost or merged in one more general force, and we may find gravity so merged and lost, and yet the idea of force may for ever remain as long as man is in existence and the conviction of its eternity become only the more overwhelming as we merge all separate forces by our successive discoveries investigations and generalizations into one force. The forces of matter, so called are in truth only *names* which we for the help of our memories and the progress of our investigations give to a number of certain similar phenomena[.] You do not require to be told that there is no *thing*, answering to the terms gravity electricity caloric, as there are things answering to the terms a pound weight of lead, a Leyden jar and a rod hot poker; and the terms gravity electricity caloric etc are merely general names for many similar or precisely identical phenomena[.] In banishing every one of these names from our vocabulary we should not annihilate the phenomena; and by the same rule, should we banish them from science, we should not lessen nor destroy one atom of the force whatever it may be which is the cause of them all.

"The strict science of modern times" as you say has ["]tended more and more to produce the conviction that force can neither be created nor destroyed"⁶. This may with propriety I think be said to be one of the latest results of all inquiry and to be an ultimate result in which the mind will rest[.] Every days experience will confirm it. At the same time it is in harmony with our earliest and latest convictions. Every action of our lives is based on an internal conviction – expressed in the proverbial saying – a burnt child dreads the fire, that the force whatever it be which burns one day will burn the next and for ever. Under certain circumstances the fire may not burn, then doubt of the accuracy of the conviction arises, observation sets to work explains the circumstances but confirms the inherent conviction that force is one day what it was the day before[.] In such cases as the return of day and night which has never been interrupted the conviction is never followed by doubt and we invariably believe and always act on the belief that the sun will rise to morrow

as he has risen to day; or if we express the matter more scientifically, we all practically and forever believe that the force which gives us day and night will continue to exist and to operate. If we carry our researches back to the beginning and onward to the possible annihilation of the race of man, we learn that day and night have been from the beginning and we believe that they will be hereafter; or that the force which produces them was before man and will, though he come to an end be eternal. In truth Astronomy and geology carry back the theme before history or man gets hold of it and prove that the force (or forces as science yet says) which now sustains man on the earth and sustains the earth and all the stars in their places has been in operation for countless ages bringing the crust of the earth to its present condition and keeping in harmony and order a wonderful universe parts of which it is mans greatest merit to comprehend. The last results of philosophy are then on this point strictly in accordance with our instinctive convictions and physics and metaphysics agree in the conclusion that force is indestructible[.]

To this great and universal principle you find the usual *expression* of the law of gravity an exception. "It appears["] you say to ["]ignore entirely the principle of the conservation of force and to be in opposition to it"⁷ – because according to the expression it varies inversely as the square of the *distance*. The whole difficulty as the whole problem rests entirely on the word *distance* and the only definition to be given of this word must be obtained from metaphysics[.] Mathematics can neither explain nor define it[.] What is distance according to the square of which gravity is said to vary inversely? It is no *thing*[.] In mathematics it is only a succession of invisible points without length breadth or thickness[.] It is a mere relation and the law of gravity which is said to vary as the square of the distance, varies as the square of no thing a sorry kind of law to counteract our instinctive convictions and all the deductions of science.

Distance as a relation has a definite meaning and now in consequence of almost instance telegraphic communication through great distances an immense interest. To understand its signification is of equal importance. What it means; or the sources of our ideas of it, to state which is to define it – is *motion*. First dawning on the mind by the muscular sensations of the hand in conjunction with the eye not yet trained to complete vision, as it gropes over the fount whence the infant draws his nourishment – then extended and confirmed by free use of arms and legs, then by wandering or being carried across a continent, combined with *inherited* knowledge, that other men have traversed continents, and finally observing and ascertaining that he is carried by the globe itself through the vast regions of space; the notion or idea of distance is formed in man. It corresponds to motion. It is the portion of space – the whole of which extends beyond the reach of our telescopes which motion on the surface of the earth, and the motion of the earth round the

sun including its motion with the sun have enabled man to compass and to measure. It is for is an *idea* or state of mind arising from our muscular contractions, and the perceptions of our senses, including *those of our predecessors* as well as our own. When mathematicians therefore speak of gravity varying inversely as the distance, they only mean inversely to an idea of ours, or to the motion which is its source. Motion too is the source of our idea of time which corresponds to total known motion and both are in space which includes them includes all things and all the relations of things[.]

But space itself is no *thing* and it is not manifested to us as a force though our idea of it borrowed from motion is inseparable from all things[.] Every thing comes with it the idea of space[.] Even the communication by electricity, which transmitted westward actually annihilates time, (i.e. is made with greater velocity than the revolving motion of the earth) has its battery at one spot, and its needle and dial plate at the other, and includes the idea of space though not of distance[.] Traveling like electricity seems and may be wholly independent of time or its measure motion. Its *vis inertia* is perfect rest. It like space too, if we cannot say that it includes, it belongs to all things. There is no *thing* though there are imponderable forces without gravity; It pervades the universe. It is greater than our idea of time measured by motion, The motion of longest duration yet ascertained. It was before the beginning of our reckoning the idea of it is involved in all the researches of the geologist and is wherever our reckoning reaches. It has no relation to time though time has a relation to it, ie, the motion which measures time is not independent of gravity. The same reasoning applies to the other word distance which is only a definite portion of space measured by motion as *time* is the whole of space yet known to man by motion. Gravity on this explanation is wholly independent of our idea of distance, and though we may be profoundly ignorant of its mode of operation – the expression of the law which you object to may be only a phrase, of which there are many in existence used to conceal our ignorance[.]

To me there seems to be many other relations which exist independent of time or of the motion by which time is measured. The existence of Queen Elizabeth⁸ influences at the moment the hopes and fears and character of English Men[.] So the existence of Julius Ceasar⁹ [sic], the Roman Republic, the Assyrian empire have a present influence, or operate with a certain force over all living men, The ideas of Berkeley¹⁰ as well as those of Shakespeare, the ideas of Mr. Faraday and those of Mr. Macaulay¹¹ are at this moment present to minds in the United States in Australia and in the [word illegible] of London. Our present ideas of all things – even of time included depend in part on ideas of our ancestors[.] The force whatever it might be which called these ideas originally into being is observably still in existence & still operating. Old empires and great writers have passed away, but not their influence.

They and we and all of us exist *in time* ie in conjunction with the motion of the heavenly bodies, but the present evidence of the influence of the Assyrian and of ideas generated in England, extending to America and Australia has no other relation to time_[.] It has no dependence on distance. It operates across oceans and ages by means of little marks which have no resemblance to the idea_[.] The force in its origin was mental the present influence is solely and entirely mental_[.] The minds of successive generations and of generations living far apart are linked together by the perishable body or even by signs which the perishable body makes, the links pass away and the influence or the force remains_[.] This is quite as wonderful as the instant operation of electricity in different places, or as the constant operation of gravity be the law or the expression of the law what it may. The relations of electricity to space and of mental influence to time, a portion of space and gravity operating at a distance – or in relation to a portion of space seem kindred phenomena. They all negative the assumption that force cannot operate at a distance, or independently of the motion which measures time, electricity does so operate through many leagues, the mind does so operate through many ages. What is called attraction with all the forces which operate at insensible distances may be placed in the same category. No motion similar to that which is the source of our idea of time accompanies the operation_[.] Except as all nature is in ultimate analysis mysterious there is nothing mysterious in gravity operating independently of motion or of distance, and nothing in its operations but the very contrary to teach us the eternity of force_[.]

Let me add with reference to the transformation or absorption of forces one into another as mans generalizations proceed that the common supposition that what are now called elements are to be eternal is a mistake. Take iron as yet I believe an undecompounded metal as an example; in this condition it is generally speaking a production of ash_[.] It is continually abraded corroded destroyed and dispersed_[.] The common supposition is that the iron is always to be in existence_[.] But it is susceptible of many combinations and as corroded or abraded may form combinations unknown to us so that the force which even in iron may never be by one atom diminished or destroyed while the metal itself having taken the new form may cease to be. The extraordinary progress of arts in modern time enabling us to pass from place to place with a velocity unknown to our ancestors, and to communicate from place to place with a still more,– to them

1. Herbert Spencer (1820–1903, ODNB). Philosopher. Identified on the basis of handwriting and the similarity of the views expressed in this letter with those of Spencer (1855), especially pp. 230–76.
2. Faraday (1857a), Friday Evening Discourse of 27 February 1857.
3. *Ibid.*, 352.
4. *Ibid.*, 353.
5. *Ibid.*, 355–6.

6. *Ibid.*, 352.
7. *Ibid.*, 355.
8. Elizabeth (1533–1603, ODNB). Queen of England, 1558–1603.
9. Gaius Julius Caesar (100–44 BCE, ODNB). Roman general, politician and writer.
10. George Berkeley (1685–1753, ODNB). Irish bishop and philosopher.
11. Thomas Babington Macaulay (1800–1859, ODNB). Historian.

Letter 3282

Faraday to Christian Friedrich Schoenbein

7 May 1857

From the original in UB MS NS 430

Royal Institution | 7 May 1857

My dear Schoenbein,

On receipt of your last¹ I spoke to Miss Hornblower who said she had written fully either to you or Miss Schoenbein and had indeed been expecting an answer as she was obliged to keep her own arrangements open until she heard from you she seemed glad to learn how the decision went but you must judge from her letter whether it requires a direct answer.– I think she said that in it she had spoken of time &c &c & I think she mentioned the time but whether it was September or any other month I cannot now tell – I forget every thing and I am obliged to be content to forget and this makes me anxious that no point of the arrangement should depend upon what I may say but that direct communication should convey the necessary information. I should almost certainly introduce some blunder – I am daily occupied in making and repairing mistakes even in the very house I live in.

I have every conviction that Miss Schoenbein will like Miss Hornblower & when she knows her will soon highly esteem her. She is a woman of business but she has always left a strong & kind impression on the minds of those ladies who have been with her from abroad & I have no doubt it will be so with Your daughter. It can be no slight thing for you to part with her for a while but you may be sure that at Miss Hornblower's she will have a safe home. We shall see what we can of her though our residence here & the circumstances of our having *no house* cuts short our means of seeing friends as we could wish – but all that must be left – Let me say a word of sympathy on our part to Mrs. Schoenbein under the coming circumstances:– for the mother cannot but be anxious on the matter. My wife is an invalid at present & not yet out of her room or I am sure she would join me in kindest thoughts to you all.

Ever My dear Schoenbein | Yours | M. Faraday

1. Letter 3275.

Letter 3283**Peter Henry Berthon to Faraday****9 May 1857****From the original in GL MS 30108/2/75**

Trinity House | 9 May 1857

Sir,

Enclosed I transmit Copy of a Communication from Messrs Chance Brothers & Co.¹ in respect to a Revolving Dioptric Light Apparatus in course of manufacture by them, and have to signify the request of the Elder Brethren that you will examine the Apparatus as early as may be convenient after it is reported ready for your inspection, and favor the Board with a Report of the result.

I am | Sir | Your very obedient Servant | P.H. Berthon
M. Faraday Esq.

1. Taylor to Berthon, 5 May 1857, GL MS 30108/2/75.

Letter 3284**Peter Henry Berthon to Faraday****12 May 1857****From the original in GL MS 30108/2/75**

Trinity House | 12 May 1857

Mr. Berthon presents his Compliments to Professor Faraday, and with reference to his Letter of the 9th Inst¹: and it's Enclosure respecting the New Apparatus for Lundy Island Light, begs to say that by a Communication received this morning from Messrs. Chance, it appears that the Lens will be ready for inspection by Professor Faraday at the time appointed, viz:– Wednesday (to-morrow).–

Mr. Berthon begs to add, that the Board will be obliged by a particular examination being made of the Catadioptric Zones of the Apparatus in question.–

1. Letter 3283.

Letter 3285**Robert Lucas Chance to Faraday****12 May 1857****From the original in GL MS 30108/2/75**

Glass Works near Birmingham | 12 May 1857

My dear Sir,

The apparatus is ready and if you come by the train which leaves Euston Square on Friday¹ at 10 AM you will be just in time for a train leaving Birmm. at 1.40 PM for Spon Lane.–

My servant will meet you at the Station & attend you to the Glass works– You can then see the Lighthouse & afterwards come to Summerfield to dinner & in the evening see it again & I will see you off Saturday morning & if you wish it we can call on Mss. Phillips² in the road.

Always My dear Sir | Yours very faithfully | R.L. Chance
Michael Farad[ay] Esq | &c &c &c

1. That is 15 May 1857.

2. Unidentified.

Letter 3286**Robert Lucas Chance to Faraday****16 May 1857****From the original in GL MS 30108/2/75**

Glass Works near Birmingham | 16 May 1857

My dear Sir,

When I heard that some of the lenses were defective I at once recollected that I had guaranteed to Captn. Shepherd, & afterwards to the Board at the Trinity House that the Lighthouse apparatus should be at least equal to the French, & on my arrival at the Glass works I found that measures were already commenced for replacing the defective ones.–

On Monday morning¹ my partners meet me, & by that days post I shall be able to inform you on which day you may see the apparatus without an imperfect lens – I hope it will not be later than Friday next² – I am exceedingly annoy'd that Mr. Masselin should have had so little regard to his & our credit so to have permitted this to have occur'd, the more especially as it will lead to you having the trouble of coming down a second time.–

We shall not permit the Trinity House to be at the expence of your coming a second time, & feel that we owe you & them many apologies for not having presented for your examination yesterday a perfect apparatus–

I remain | My dear Sir | Yours very faithfully | R.L. Chance
Mich Faraday Esqre | &c &c &c

1. That is 18 May 1857.
2. That is 22 May 1857.

Letter 3287**Faraday to Peter Henry Berthon****18 May 1857****From the original copy in GL MS 30108/2/75***Report*

Royal Institution | 18 May 1857

Sir,

I have been to Birmingham¹ and examined the apparatus for the Lundy light by day & night, and hasten to make the following report. The apparatus which is to revolve consisted of eight principal lenses occupying the middle part;—eight upper holo catadyoptrical pannels each acting virtually as a lens;—and a series of lower prisms which refracting only in a vertical plane gave a continuous light (though feeble) all round the horizon[.]

The *colour* of the glass is good & regular throughout being very small in quantity even in those parts of the upper prism where the light has by reflection & refraction to pass through six inches in thickness of the glass — it is of a pale bluish green in character[.]

The glass is remarkably clear from *bubbles*.

In respect of the perfection of *form & fitting* of the different pieces of glass I have to observe that the eight chief lenses are exceedingly well worked. My close inspection discovered a deficiency of true form in the central piece of two of them; these are to be changed. Excellent machinery for the grinding of the glass is in course of erection and it is in the first pieces ground by this machinery that I find the condition, it did not occur afterwards: and now that the parties know how to observe this machinery it is not likely to occur again. — The upper holophotal pannels or lenses are also well shaped & worked. Some of the lower zones prisms i.e. of those at the bottom of the apparatus are not so perfect. The fact is that because the place of the burner shuts off in all cases the true focal point from these lower zones, a higher focal point has to be taken for them. It is well known how little power these zones add in any case to the whole amount of light. If however such a source of light as that supp[or]ted by the Magneto electric machine ever came into use then the true focal point will be as important to the lower as to the upper part of the apparatus; & then the lower pieces must be worked in accordance with the upper.

Striae. On the whole the glass was good in respect of striae;— as good as any apparatus that I have seen.— Since parts were much marked by fine striae I endeavoured to ascertain whether, at night time, they produced any sensible injurious effect but could not distinguish any such[.]

Several of the chief lenses were examined by the sun's rays and gave very good foci. The two that I spoke of as having the centres slightly deformed

did not shew any ill effect here; neither indeed could they, the imperfection being only over a small surface[.]

The middle & the upper part of this apparatus revolves;— they are fixed to each other so as to revolve together;— the upper tier of lenses (or parts of lenses) may be made to coincide with the middle or chief tier so that the lights shall unite or they may be moved more or less on one side so as to give separate flashes. In the present case they are on one side about 7° and give 16 lights in the circle i.e. a principal flash then darkness — then a second flash & then a longer darkness[.]

A four-wicked central lamp was placed in the apparatus and the effect examined at night (which being moonless was favourable for the purpose). Different distances were chosen:— the following results were obtained at 100 yards[.] The light of the eight chief lenses was excellent, very regular & bright in every part, & apparently uniform in the whole course of its path across the eye:— its width was $26\frac{1}{2}$ feet at this distance showing a divergence of 5 degrees or a little more. The light from the eight upper lenses was very excellent in character it was not so abundant of course as that from the chief lenses, nor so steady because the rays forming this light come from the top or forked part of the lamp flame & take up a slightly varying character accordingly. The width of the flash or beam was again $26\frac{1}{2}$ feet or 5 degrees as it ought to be:— The dark interval between these two flashes was 12 feet or $2\frac{1}{3}$ degrees nearly: the larger dark interval was 171 feet or $32\frac{1}{2}$ degrees.

I measured the proportion of light just formed by the upper & the lower lenses:— it was as 8 to 14 nearly that is, the upper tier of lenses (for such they vertically are) gave rather more than half as much light as the chief or middle tier[.]

The upper lenses (which I believe are the proposition of Mr. Stevenson) appear to answer their purpose perfectly. Whether the present arrangement of them & the lower is the best is for the Trinity Board or the Mariner to judge[.] At present there are sixteen lights in the circle: the lights vary being alternately larger & less, & the intervals vary being alternately larger & shorter; the intervals can be varied in any degree. The lights also may be varied to a certain degree, for if they overlap more or less, the effect will be a beam of a certain brightness, then an increase to greater brightness,— then a diminution, & finally a disappearance: or if the lenses coincide there will be eight very bright & equal lights, with eight long equal intervals. When the arrangement is decided upon, it can be made & rendered permanent with the utmost facility²[.]

I am | Sir | Your Very Obedient humble Servant | M. Faraday
P.H. Berthon Esqr | Secretary | &c &c &c

1. See letters 3285 and 3286.

2. This letter was read to Trinity House By Board, 19 May 1857, GL MS 30010/41, pp. 32–3 and Trinity House Court, 2 June 1857, GL MS 30004/27, p. 71. For the outcome see letter 3290.

Letter 3288

Faraday to George Biddell Airy

18 May 1857

From the original in RGO6/471, f. 187

Monday Morning

My dear Mr. Airy,

I have this moment learnt that there will be the Magneto electric light *this evening* at 8 ocl^k & after – at the Trinity house wharf Blackwall – I cannot be there but if *you can – and wish* – and will ask for Professor Holmes you will be admitted to the Wharf & lighthouse. I should think the best plan would be to go across in a boat to the Trinity Wharf landing place. Captⁿ. Poulter is the chief at the wharf & if you mention my name or your own to him you will have every attention_[.]

In haste | Yours truly | M. Faraday

Endorsed by Airy: 1857 May 18

Letter 3289

Robert Lucas Chance to Faraday

18 May 1857

From the original in GL MS 30108/2/75

Summerfield House near Birmingham | 18 May 1857

My dear Sir,

When we met this morning I found that my nephew J.T.C was very indignant with Mr. Masselin & after a thorough investigation into the facts we removed him from his post as manager of the Light house department.– Had my opinion been follow'd he would have been removed long ago–

He attempted to shew that he could replace the imperfect pieces in a few days & make the lower part good to the upper, but it was proved that the whole affair would be imperfect & before we separated we made arrangements for making an entire new set of moulds, & for casting & finishing the whole of the fix'd part entirely new, which will be ready by the 30th of June for your inspection & also to make a fresh apparatus *entirely* from top to bottom by the 5th of Sepr.–

I purpose (DV) coming by the Mails to night & to call on you about 9 to 9.30 AM tomorrow, & to consult you as to my waiting on the Trinity House Board. We wish them not to take the present apparatus but to wait until the 5th. of Sepr. because we are confident we can make one superior to any thing

we have ever made & answering fully to our guarantee of being equal to the best French light as respects the colour & quantity of the glass – & in all other respects. We are quite prepared to put up the present apparatus at once & afterwards to remove the whole or at any part that is found defective.

I remain | My dear Sir | Yours very faithfully | R.L. Chance
Michael Faraday Esq | &c &c &c

Letter 3290

Peter Henry Berthon to Faraday

19 May 1857

From the original in GL MS 30108/2/75

Trinity House | 19 May 1857

My dear Sir,

Mr. Chance has had an interview with the Board this morning and it has been arranged that the Apparatus is to be put up at Lundy as it is, that is with the two defective Lenses, and that he shall undertake in writing to replace them at his Expense when required to do so, or at any time he may wish, there will not therefore be any occasion to trouble you to make a second visit to Birmingham¹.]

I return you his Note as requested.

Excuse haste & Believe me | My dear Sir | Yours very faithfully |
P.H. Berthon
Professor Faraday | &c &c &c

1. See letter 3287 and Trinity House Court, 2 June 1857, GL MS 30004/27, pp. 70–1.

Letter 3291

George Biddell Airy to Faraday

19 May 1857

From the original press copy in RGO6/471, f. 188–9

19 May 1857

My dear Sir,

I took advantage of your kind notice¹, and betook myself to the Trinity Wharf before the commencement of the light, and saw the apparatus pretty well, and then saw the light extremely well, and very good it is.

I have just written to Professor Holmes to the care of Capt. Poulter (which address I suppose will find him), to ask for another inspection of the apparatus, and to suggest that for trial of the light it would be well to direct the beam to the front of the Royal Observatory. The building is the most conspicuous and I believe the most distant within sight of the Trinity Wharf,

and there is scarcely a liability to interruption. Moreover, there is something official, scientific, and solemn, in such a reference. – If this pleases you, any advantage might be taken on the Blackwall side for directing various lights simultaneously, and for giving them the motion of revolving lights, and every advantage might be taken of the R. Observatory for yourself and others to see the effect.

I am, my dear Sir | Yours most truly | G.B. Airy
Professor Faraday

1. Letter 3288.

Letter 3292

Faraday to George Biddell Airy

20 May 1857

From the original in RGO6/471, f. 192

[Royal Institution embossed letterhead],
Albemarle St. W | 20 May 1857

My dear Sir,

I am very glad you have seen the Magneto electric light¹. Is it not a remarkable & wonderful source. If Professor Holmes agrees to what you propose I hope you will let me know_[.] I should like to see the light from the Observatory though I have already seen it from Woolwich. I do not know what arrangements are possible for Professor Holmes_[.] I think the means of variation at the Wharf are but few but I cannot doubt that he will do all he can & that you require.– I should like him (if they can arrange it) to shew you an Argand lamp in a Parabolic reflector both with the naked electric light & with the same light in a reflector; & I should hope also that they could make both the lights traverse that you might see the coming on & passing away of the beam_[.]

Ever Very Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. See letter 3291.

Letter 3293

Cresswell Cresswell¹ to Faraday

21 May 1857²

From the original in RI MS Conybeare Album, f. 41

21 Princes Gate | Thursday, 21, 1857

Mr. Justice Cresswell presents his compliments to Mr. Faraday & at the request of Sir B Brodie has forwarded to him the enclosed memorial to be signed by him & returned to Sir B. Brodie³_[.]

1. Cresswell Cresswell (1794–1863, ODNB). Judge in the Court of Common Pleas.
2. Dated on the basis that the only Thursday falling on the 21st during 1857 was in May.
3. This letter may relate to letter 3221.

Letter 3294

Benjamin Cheverton to Faraday

23 May 1857¹

From the original in IET MS SC 2

45 St James Sq | Notting Hill – W | 23 May

Dear Sir,

I thank you sincerely for your kind letter² & feel honoured that you should in any – the slightest sense deem me to be “a companion in the search after physical truth”.–

I have been induced to trouble you with another communication of mine to the Editor of the *Mechanics Mag*³, because there is in it, something like an *amende* to yourself, as well as a justification of the free use which we both make of the word “force” – a freedom arising perhaps out of a similarity of views, as to force being, the all in all, of insentient nature, so far at least as the latter is cognizable to *our* faculties_[.] It is possible that an existence may reside at the center of atomic forces, but without dimensions or any other quality but force, of which however we do not, nor can we know any thing. I am not aware whether these views still appear strange to scientific men but to my apprehension, all philosophical investigations have for a long time past tended in this direction_[.]

You will perceive that I have made use of the inclosure⁴ you sent me in your letter, and also of the information you were so good as to communicate respecting Mr. Maxwells investigations⁵_[.] I did not feel authorised in mentioning names, and I hope I have not been indiscreet in adverting to it at all–

I am Dear Sir | Yours respectfully | Benj Cheverton
Mr. Faraday Esq

P.S. The Editor was not disposed at first to insert this second communication

1. Dated on the basis of the references to Cheverton’s articles.
2. Not found.
3. Cheverton (1857b); the first was Cheverton (1857a).
4. Maxwell (1856). See letter 3301.
5. Mentioned in Cheverton (1857b), 495.

Letter 3295**Mark Lewis¹ to Faraday****29 May 1857****From the original in ULC Add MS 8546/I/133**

198, Strand. | 29 May 1857

Dear Sir,

I am requested by Mr. Ingram² to enclose an invitation to the Opening of the Boston & Sleaford Railway & to say that your attendance will give him the greatest pleasure.

A Special Train leaves the Great Northern Railway King's Cross at 9.30. and a pass shall be sent if you are kind enough to accept the invitation.

The Special Train returns in the Evening[.]

I am truly | Mark Lewis

M. Faraday Es. | &c &c &c

1. Unidentified.

2. Herbert Ingram (1811–1860, ODNB). Liberal MP for Boston, 1856–1860. Chairman of the Boston, Sleaford and Midland Counties Railway.

Letter 3296**Edward Jones to Faraday****4 June 1857****From the original in IET MS SC 2**

Vicarage, West Peckham, Maidstone | 4 Jun 1857

Dear Sir,

A short time back my brother¹ who is a member of the Royal Institution put into my hands a pamphlet containing among others your lecture upon the Conservation of Force².

As an old Cambridge man, the subject was so interesting, and the principle, tho' of course I never thought of it before, so self evident, that I could not help considering how & why it was that the laws of gravitation should seem to be opposed to it[.]

I have put down on paper my views of the question³, and if you do not think it too much trouble to read them, tho' they may not assist you[.] you will see that some of the difficulties which in your anxiety after the truth you imagine to exist, do not exist in the minds of some others[.] Those which I cannot at present see my way thro I yet believe to be removable for it appears to be impossible that any other law should obtain or that any law can be sound which ignores or will not admit of the principle of Conservation of Force – I have often had the Privilege & pleasure of hearing you tho' not on this occasion, and I hope to have that pleasure again on the 12th⁴.

I am | Dear Sir | faithfully yours | Edwd Jones

(51 Strand)

Michael Faraday Esq. | Royal Institution

1. William Jones (1807–1866, Plarr (1930), 1: 632). Surgeon.
2. Faraday (1857a), Friday Evening Discourse of 27 February 1857.
3. This is in IET MS SC 2.
4. Faraday (1857b), Friday Evening Discourse of 12 June 1857.

Letter 3297

Faraday to Edward Jones

9 and 10 June 1857

From the original copy in IET MS SC 3

Royal Institution | 9 Jun 1857

My dear Sir,

I have received your very kind letter¹ and paper & am delighted at such a result of my evening². If nothing else had come of it but that, it would have been a sufficient reward:– but much else has come & I expect much more.

I do not think you can find in my papers any word or thought that contradicts the law of gravitating action – my observations are all directed to the *definition or description of the force of gravitation* with the view of clearing up the received idea of the force so that if inaccurate or insufficient it may not be left as an obstacle in the present progressive state of science[.] If I am wrong in believing that according to the present view the mutual gravitating force of two particles A & B remains unchanged whatever other particles come to bear upon A or B then the sooner I am corrected publicly the better. If your view (whether old or new) that the power of A remains unchanged in amount but is subdivided upon every particle which acts upon it is the true or the accepted one then I shall long to see it published & acknowledged for I do not find it received at present. I have proved to my own satisfaction that such is the case with the dual powers Electricity* & Magnetism^ø and it is the denial of it as regards gravity which makes up my chief difficulty in accepting the established view of that power. Your statement that (A) may attract or act on B.C. with a force of one whilst BC act on A with a force of two, seems to me inconsistent with the law that action & reaction are equal; but I suppose I am under some misconception of your meaning.

The cases of action at a distance are becoming in a physical point of view daily more & more important[.] Sound, Light, Electricity, Magnetism, Gravitation, present them as a series[.] The nature of sound & its dependence on a medium we think we understand pretty well – the nature of light as dependent on a medium is now very largely accepted – the presence of a medium in the phenomena of Electricity & magnetism becomes more & more probable daily. We employ ourselves & I think rightly in endeavouring to elucidate the physical exercise of these forces or their sets of antecedents & consequents & surely no one can find fault with the labours which eminent men have entered upon in respect of light or into which they may enter as regards electricity & magnetism. Then what is there about gravitation that should exclude it

from consideration also: Newton did not shut out the physical view but had evidently thought deeply of it \times , and if he thought of it why should not we in these advanced days do so to[o.] Yet how can we do so if the present definition of the force, as I understand, is allowed to remain undisturbed; or how are its inconsistencies or deficiencies as a description of the force to be made manifest except by such questions & observations as those made by me & referred to in the last pages of your paper. I believe we ought to search out any deficiency or inconsistency in the sense conveyed by the received form of words that we may increase our real knowledge striking out or limiting what is vague. I believe that men of science will be glad to do so & will even as regards Gravity, amend its description if they see it is wrong. You have I think done so to a large extent in your M.S. and I trust (& know) that others have done so also. That I may be largely wrong I am free to admit. Who can be right altogether in physical science which is essentially progressive & corrective; still if in our advance we find that a view hitherto accepted is not sufficient for the coming development we ought I think (even though we risk something on our own part) to run before and raise up difficulties that we may learn how to solve them truly. To leave them untouched hanging as dead weights upon our thoughts or to respect or preserve their existence whilst they interfere with the truth of physical action is to rest content with darkness & to worship an idol[.]

I take the liberty of sending by this post copies of two papers. The one on Conservation of force³ is I suppose that which you have read[.] I have made marks in the margin which I think will satisfy you that I do not want to raise objections except where the definition of gravity originates them of itself. The other is on the same subject two years anterior⁴. If you would cause your view of Gravity as a force *unchanging* in amount in A but disposable in part towards one or many other particles, to be acknowledged by Scientific Men; you would do a great service to science[.] If you would even get them to say yes or no to your conclusions it would help to clear the future progress. I believe some hesitate because they do not like to have their thoughts disturbed. When Davy discovered potassium it annoyed persons who had just made their view of chemical science perfect⁵ & when I discovered the Magneto-electric spark, distaste of a like kind was felt towards it even in high places⁶. Still science must proceed, and with respect to my part in the matter of gravitation I am content to leave it to the future. I cannot help feeling that there is ground for my observations for if there had been an evident answer it must have appeared before now[.] That the answer when it comes may be different to what I expect I think is very probable, but I think also it will be as different from the present received view. Then a good end will be obtained – and indeed your observations & views appear to me to be much of that kind[.]

If it should be said that the physical nature of gravitation has not yet been considered but only the law of its action & therefore that no definition

of gravity as a power has hitherto been necessary that may be so with some; but then it must be high time to proceed a little further if we can, & that is just one reason for bringing the principle of the conservation of force to bear upon the subject. It cannot I think for a moment be supposed that we are to go no further in the investigation. Where would our knowledge of light or magnetism or the voltaic current have been under such a restraint of the mind.

Again thanking you most truly for the attention you have given to me & the subject I beg you to believe that I am Very Gratefully

Your faithful Servant | M. Faraday

Revd Edwd Jones | &c &c &c

10th June. Dear Sir I omitted a passage in my reply to your letter which ought to have appeared at the end of the second paragraph. The passage is (A) – will you have the goodness to consider the passage as inserted[.]

Ever Truly Yours M. Faraday

* Exp. Researches 8vo vol 1. par 1177, 1215⁷, 1681⁸, &c

ø Exp Researches xxviii. Vol iii⁹ p. 328 &c. Paragraph 3109, 3121¹⁰, 3225¹¹ & –

Also Par. 3324 of the same vol III p. 544¹²

× Note p7 of the paper on Conservation of force¹³

1. Letter 3296.

2. Faraday (1857a), Friday Evening Discourse of 27 February 1857.

3. *Ibid.*

4. Faraday (1855a), 10–13.

5. See Golinski (1992), 221–3.

6. See Gee (1990).

7. Faraday (1838a), ERE11, 1177 and 1215.

8. Faraday (1838d), ERE14, 1618.

9. Faraday (1855c).

10. Faraday (1852b), ERE28, 3109 and 3131.

11. Faraday (1852c), ERE29, 3225.

12. Faraday (1855b), ERE[29b].

13. Faraday (1857a), Friday Evening Discourse of 27 February 1857, p. 358 which refers to the letter from Newton to Bentley. See note 8, letter 3033.

Letter 3298

Edward Jones to Faraday

15 June 1857

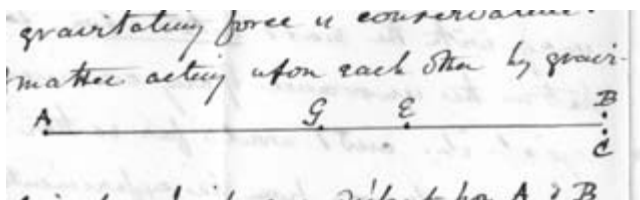
From the original in IET MS SC 2

Vicarage West Peckham, Maidstone | 15 Jun 1857

My dear Sir,

I fear when you see this, that you will bitterly repent having condescended to bestow so long and kind a notice¹ upon the paper I sent you the other day, as I now venture to send you another, in part examination of one or two points on which I think you have scarcely apprehended my meaning. First allow me to say that I do not differ with you, but on the contrary entirely concur with you as to the necessity of imagining every possible position in

which a case can be put, as the best means of arming yourself against the opposition which will naturally be offered to it. Secondly that I did not consider that you had misstated the "received idea" of Gravitating force, but that you were weakening your position in so far admitting it.— Untill I read your lecture², I am not aware that I ever troubled myself to consider whether "received ideas" either in fact or in toto, were true or no. I took it for granted — but *since* reading your pamphlet, that part of the received idea which makes the gravitating force of any particle vary with the mass *acted upon*, has seemed to me so inconsistent that in the ignorance of any convincing proof of its truth, I unhesitatingly reject it, and I would far rather question the correctness of the inferences drawn from the experiments or calculations made for the purpose of proving it, than admit it — But here I must beg it distinctly to be understood that I speak of the *whole* gravitating force of any particle, I do not mean to assert that any given particle of matter exerts *no* greater force upon ten particles than upon one — I admit, and it is necessary even to my argument that I should admit that a particle of matter does actually exercise a force upon one or more particles of matter according to and in proportion to their number, but I *do not* admit that if any particle of matter A exercised its *whole* force upon any other particle of matter B, that the addition of a second particle C to B would cause A to exercise a similar and equal force also on C, and so to exercise altogether twice the force which it did before. The reason why a given particle exercises ten times more force upon ten than upon one is, I maintain, because it never did and *never could* exercise upon that one more than a little part of the force which it does upon the ten and that it did always exercise more other nine portions of its force upon the nine.— Now the received idea of the Law of gravitating force in habit varies in two ways, first inversely as the square of the distance secondly in proportion to the mass acted upon — The definition only names one (the first) of those. The first of these I waive for the present because as I said before I have not yet sufficiently considered it — I address myself to the second; and for this purpose restate my problem (I hope more clearly) which was intended to prove that the *whole* gravitating force of any particle of matter is not increased by the increase of the number of particles *acted upon*, and therefore that as far as regards *mass* of matter, gravitating force is conservative. Let A & B be any two particles of matter acting upon each other by gravitation at a given distance AB



their tendency is to meet at their common Centre of Gravity G which is at a point equidistant from A & B_[.] Let another particle C be added to B, then the tendency would be to meet in their common Center of Gravity at the point E so placed that

$$AE : EB :: B + C : A :: 2:1$$

i.e. the distance which A tends to draw B + the distance which A tends to draw C are together equal to the distance which B + C tend to draw A. Now if the force of A on B remained unchanged and a new and equal force were to rise up in A to be exercised on C, then the tendency of the three bodies would still be to meet at the original point G; but this, as G is no longer their common Center of Gravity is not the fact.

The gravitating action here of the three bodies is to their common Center E – But now the *apparent* result is that the force of A has *increased* as much as the force of B has decreased, and that the force of A now actually equals the combined forces of B + C. and hence that the “received idea” of gravitating force in a particle varying as the numbers of particles acted upon is confirmed (vid last line page I.)

But now let me call into action another principle which I conceive to be the true one – That particles of matter should not be treated individually but as parts also of *the mass* or system and so that all force, whatever should be referred to common Centers of Gravity of all matter both acting and acted upon. Thus we have two particles A & B or a system A + B whose common Center of Gravity is G. A does not act on B, nor B on A, but A acts on A + B or the whole mass, & B in like manner acts on A + B the whole mass A + B acts on A it also in like manner acts on B.

All at the point G.

Add C to B. Then the system is A + B + C whose Center of Gy is E.

Then A acts on A + B + C at E and the action of A = BE

B + C acts on A + B + C at E and the action of B + C = AE.

and $\therefore AE = 2BE$. therefore the action of B + C = twice the action of A as before_[.] But now to compare the force of A with itself under the cases.

$A = \frac{1}{2} \times A + B$ and its action $AG = \frac{1}{2} \times AB$ in the first case

$A = \frac{1}{3} \times A + B + C$ and its action $BE = \frac{1}{3} \times AB$. in the second case

Hence the whole force of A measured or represented by its action is in proportion to *itself* the *acting* body and *not* in proportion to the body *acted* upon so also the action of the whole mass A + B + C on A is equal to twice its action on B + C because $AE = 2BE$

and this line of reasoning agrees I think in a measure with your remark page 5 upon the relation of a lone particle to gravitation or “to something which causes gravitation, and with which whether the particle is alone or one of a universe of particles it is always related”³. And I think that it shows, if it be

correct, and I candidly believe it to be so, that while the Law of Gravitating action remains as it was before. The Law of gravitating force becomes *conservative*. I do not think that the above view at all militates against the doctrine of the equality of action and reaction. but rather the contrary; for the action of A on A + B + C is BE + CE or 2BE and the action of A + B + C on A is AE = 2BE_[1]

I have now put down all that I am prepared to say upon *this division* of the subject of gravitation, but the papers you have so kindly sent me open an extensive field of enquiry, and tho' I am sorry to say I know scarcely anything of magnetism & Electricity, & but little of Light & sound, the whole question has now become to me one of such exciting Interest that I cannot refrain from following it so far as my means will permit me. I am not sanguine of success, still not altogether without hope, & should anything strike me as appearing even in a remote degree to assist the enquiry you will perhaps permit me once more to trespass on your patience & kindness. Again pressing my sense of the honour you have done me in noticing my communication

I remain | My dear Sir | Ever faithfully & obliged yours | Edward Jones

P.S. I am going to Town tomorrow until Saturday⁴_[1] My address will be at my brother's⁵ No. 51 Strand tho' I shall not be staying with him.

Michael Faraday Esq | F.R.S. &c

1. In letter 3297.

2. Faraday (1857a), Friday Evening Discourse of 27 February 1857.

3. *Ibid.*, 356. Jones's reference was to the pagination of the offprint.

4. That is 20 June 1857.

5. William Jones (1807–1866, Plarr (1930), 1: 632). Surgeon.

Letter 3299

Faraday to Marc Seguin

16 June 1857

From the original in The Bakken MS

Private

Royal Institution, London | 16 Jun 1857

My dear Sir,

I must write to you in English, for if I tried to use your language on paper, I should be sure to leave my thoughts unexpressed; and I should be very sorry not to express my deep sense of your most kind invitation for which I am exceedingly grateful to you. I can imagine the pleasure of a visit to you:– but there is no hope of its being realized. Every day shows me its impossibility. Time & labour have had their course with me; they have left me happy, but with little power of active social engagements. My memory fails me: – society depends much upon it and when I make calls upon it for

that purpose, then it becomes fatigued and fails for real scientific use. I beg you whilst accepting my sincerest thanks for yourself to express my sense of gratitude to those members of your family who were willing to accept us into their company & do us kindness. I sympathise deeply with you in respect of the illness of your daughter which must be a heavy trouble to you.

Monsieur L'abbe Moigno was so good as to give me some printed accounts of parts of your researches and views¹, and Monsieur Tremblay² has left with me other portions. I rejoice to see with what power you work amongst the molecular forces, and how you break up the fallow ground; though indeed as regards the grand views of Montgolfier³ it can hardly be called fallow to you, though it may be to others⁴. You must not be disheartened: for however brightly the light may shine, those see but little of it who will not look towards it. Neither indeed can one expect that a new principle in physics should make rapid advance, unless it be accompanied by new phenomena. When once we have formed an opinion from the phenomena it becomes a *prejudice*, and we are very lo[a]th to give it up; and when that prejudice is shared by the multitude then we trust in the multitude as justifying our opinions. I think that such convictions & developments as yours, being published, are as seed sown, and if good seed, is sure to produce its fruit in due season. For my own part I am obliged to suspend my opinion daily and wait for further reasons. I receive many views of natural forces; some I feel I can reject at once, others, though contradictory, I feel bound to reserve, thinking it possible that parts may hereafter be found to be truth. My own views (always very imperfect & insufficient) I of course think well of, or I would not make them mine; but I cannot doubt that they will fail in parts as others fail. Looking to past times and to those sciences which have made the most distinct advances, as chemistry, Electricity, magnetism &c. how different are the results we now possess from the view or expected results which the leaders in those different paths entertained 50 or even 30 years ago. – And so I think it will be with our views – in many points I doubt not they will be confirmed & enlarged; in others they will be corrected; – and we must not expect to see the development but leave it as an inheritance to future ages.

It is a great cause of rejoicing to observe how many workers in molecular science have joined you in late years; and more will come. As men perceive the inconsistency or insufficiency of the present views they will become workers; but I expect more from the rising generation than from that already established in character: – *we* may yet live to see great changes.

I hope you will forgive me the freedom of my writing but I have run on just as I thought without waiting for consistency of expression: for I think I see in your papers a true man[.] Again thanking you for your extreme kindness I beg to call myself

Your Very Obligated & faithful Servant | M. Faraday
a Monsieur | Monsieur Seguin aimé | &c &c &c

1. See letter 3263. [Moigno] (1852).
2. A. Trambly. Proprietor of *Cosmos*.
3. Joseph Michel de Montgolfier (1740–1810, DSB). Balloonist and engineer.
4. Discussed in Seguin (1856), 267–70.

Letter 3300**Faraday to Angela Georgina Burdett Coutts****17 June 1857****From the original in BL Burdett-Coutts papers**

Wednesday Morning

Dear Miss Coutts,

I shall be very glad to have a third person there^[.]

Ever faithfully Yours | M. Faraday

Address: Miss Coutts | Stratton Street | Piccadilly | W

Postmark: 17 June 1857

Letter 3301**Benjamin Cheverton to Faraday****21 June 1857****From the original in IET MS SC 2**

45 St James Sq, Notting Hill | 21 Jun 1857

Dear Sir,

I return you Mr. Maxwell's paper¹ with many thanks for your kindness in sending it to me. I have been greatly interested in its perusal, so far at least as I could follow it, but I feel somewhat disappointed that the form given to your conceptions, should not have been that of an hypothesis – if even only of a provisional character – and that to serve the purpose better of calculation within a limited degree, it should have been so very arbitrary & artificial, – so much so perhaps, as to exclude it from more general applications than the subject & the simple idea of "lines of force" would of themselves impose. This is not satisfying I should think to a mind like yours, nor probably will it be to the philosophic mind generally, if we may judge from the eagerness with which both the emissive & the undulatory ideas of light, are accepted as theories of what is real. And yet it is very true, perhaps, as Mr. Maxwell intimates, that neither each of these should be taken at present for more than analogical mathematical forms of treating the subject, until a course like your own, of interrogating nature herself, shall lead to something determinate²^[.]

Probably also, you do not yourself attach to your own "lines of force", any distinct physical ideas; but are content for the present to adopt them as a method of thought, which enables you with greater freedom & especially with

greater generality, to reason concerning phenomena, to form conceptions as to their connections & relations, and so derive hints for further experiments. In truth if matter be only the manifestations of force – of which through a consciousness of the ability to exist it, as well as from feeling it, we know to be something real, and real after the same manner externally as it is sensibly so – I do not see how, in ultimate inquiries any additional physical ideas beyond those of equal external reality, time space & motion, can have any place; inasmuch as any others would be only the affections of percipient beings – psychological effects of force as the cause. So that to be chary of physical ideas, even to the absence of almost every thing from “lines of force”, would be rather to approach than to recede from the truth of nature, what is wanting thereto, is an hypothesis that shall be truthful *looking* at least, & which shall not merely serve to being the subject under calculation – for Mr. Maxwell shows us; that arbitrary conceptions will to a certain extent of application answer that purpose – but which shall unite all phenomena under one comprehensive view. To do that however, I am afraid we have no other means at hand, at present, than the occult qualities (as in a sense they are) of attraction & repulsion; but how to evoke sensible from general attraction, or *vice versa*, as Mossotti³ proposed to do⁴, is the great difficulty.

This leads me to observe, that the simplicity & paucity of physical ideas to which I have adverted, belong only to an investigation which *is* in the track of ultimate enquiries. But building on this foundation, complexities will arise, & so introduce other physical conceptions if only from the combinations of the primary ideas, and notwithstanding that we carefully exclude any thing merely of sensational origin, such for instance as may be suggested by a too liberal inference from the electric spark or fire. Now in regard to electricity & magnetism, it may be a question, whether this is not the case with their phenomena – whether they *do* belong to the ultimates. Am I not right in thinking, that their attractions & repulsions are so dependent on circumstances, – are so variable & conditional in obedience to mere form & to mere extent of surface, as to indicate that they are secondary consequences, & not the result of primary properties, or of primary conditions in the action of force? Now this is not observable in gravitation, where accordingly, we seem to arrive at something that *is* ultimate, – it not being dependent on physical ideas resulting from secondary combinations but on the mass – simply & entirely on the *mass*. Therefore I do not see how the form which Mr. Maxwell has given to your “lines of force”, can be applied to gravitation – reasoning merely in the manner of calculation – seeing that the action of the imaginary fluid, varies after a manner with the sources & sinks on the surface; whereas gravitation, is proportional to the mass, however contracted or extended may be the surface. I may be wrong – still this application is no where alluded to by him. And yet “lines of force” in some more simple guise, would I conceive apply to gravitation.

Mr. Maxwell has not attempted philosophy – indeed he takes care that his physical conceptions shall not by any possibility be mistaken for it. As a mathematician intent only on the means of calculation, this may be very proper, especially if by so doing he secures, as he certainly does, great simplicity in that respect, founded on the Engineer’s method, of taking account of force only in pressure & resistance_[.]

As far as in their relations to crystallisation have engaged your attention, I will take this opportunity of stating, what I mentioned many years since at the meeting of the British Association at Dublin⁵ to Dr. Robinson⁶, Dr. Dalton⁷ & Col Sabine, but which did not seem to attract any attention, except from the last named gentleman, that I have found diamonds in the state of splintered sparks, show signs of attraction, without excitation by heat or any extraneous cause. I have drawn them along, on not a smooth surface, by a piece of brass; & if I recollect aright have seen them suspended from it by their points. As I wanted them for immediate use, I made no other experiments, & can speak to two instances only. Perhaps however others have observed the same thing, but I have never seen it stated.–

I find I have here been led on, into what *to you*, I feel to be almost unpardonable garrulity. I have only to hope, & beg of you that you will not conceive yourself engaged in courtesy, to lose your valuable time in taking any notice of it.

I remain | Dear Sir | Yours respectfully | Benj Cheverton
Professor Faraday &c

1. Maxwell (1856).

2. *Ibid.*, 28–9.

3. Ottaviano Fabrizio Mossotti (1791–1863, DSB). Professor of Mathematics at Pisa, 1841–1863.

4. Mossotti (1836).

5. In 1835.

6. Thomas Romney Robinson (1792–1882, ODNB). Director of the Armagh Observatory, 1823–1882.

7. John Dalton (1766–1844, ODNB). Chemical philosopher who lived in Manchester. Developed a version of the atomic theory of matter.

Letter 3302

Faraday to Isambard Kingdom Brunel

22 June 1857

From the original in BrUL MS

[Royal Institution embossed letterhead],
Albemarle St. | 22 Jun 1857

My dear Brunell,

I am purposing to go and see your ship¹ next Thursday² with a nephew a young Engineer Mr. Buchanan – but I hear that the tickets we obtain at the place only allow one to stop for a *very limited time* a quarter of an hour_[.] Could

you give me a note or in any way the power of Extending the time a little?
& oblige

Ever Truly Yours | M. Faraday

1. That is the *Great Eastern* which was being constructed at Greenwich. For the ship generally see Buchanan (2002), 113–33, and especially p. 117 for the problems with visitors hinted at in this letter.

2. That is 25 June 1857.

Letter 3303

Isambard Kingdom Brunel to Faraday

22 June 1857

From the original in IET MS SC 2

22 Jun 1857

My dear Faraday,

I am much annoyed that Thursday is a day and time I can never go to the ship¹ – I should have wished to meet to have shown it you myself and explain a few of the general principles which are so little understood by the public who look on it only as an attempt to build a larger ship than any previous one – which would have been a very contemptible object–

However my assistant Mr. Jacomb² will show you everything – I should advise you *not* to go at the times named on the ticket namely 1P.M, and [word illegible] you will not need for tickets the enclosed will answer a better purpose.

I should like to have you there on a Saturday between 4. and. 6. –

Yours faithfully | I.K. Brunel

1. See letter 3302.

2. William Jacomb (1832–1887, B2). Pupil of Brunel's, 1851–1859.

Letter 3304

Faraday to Isambard Kingdom Brunel

25 June 1857

From the original in BrUL MS

[Royal Institution embossed letterhead] | 25 Jun 1857

My dear Brunel,

I (and my nephew Mr. Buchanan) went this morning to the Great ship works¹ but all in vain. Fate in the form of a Porter was inexorable and there was no one in a higher rank to appeal to.– so we came back – as we went – We should have gone at 1 o'clk but your letter² advised us *not* to go at that hour – Do I understand that you are there on Saturdays between 4 and 6 o'clk & that you would like to have me there at such a time? If so would Saturday

Week the 4th of July do – and if so again may I bring my nephew with me or leave him at home?

Ever Truly Yours | M. Faraday

1. See letter 3302.

2. Letter 3303.

Letter 3305

Isambard Kingdom Brunel to Faraday

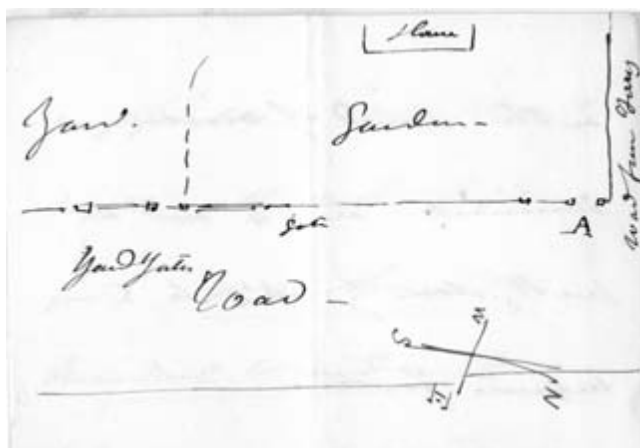
26 June 1857

From the original in IET MS SC 2

26 Jun 57

My dear Faraday,

I am vexed beyond description – of all men in the world you are the last I should have expected to have been treated with disrespect in a place under my control¹ – I wish very much to see if it has been my fault in not properly directing the note I gave to you – *will you have the kindness to send it to me by bearer or by post.* Now to remedy the evil – can you conveniently go on Saturday – and bring any body you like – any time after $2\frac{1}{2}$ – the later the more pleasant as the crowd of visitors diminish at 4 and are pretty clear by $4\frac{1}{2}$ to 5 – enquire for me – and to make assurance doubly sure – I will await – and – go in at a garden gate to a house – to the North of the Yard gates – as on annexed plan –



A is the gate. a Green Carriage gate with a small gate you enter – at the corner of the road a passage from the Ferry stairs – and the Millwall road the gate is open – go in to the lawn – and up stairs is my office – If you go by water which is the best way in fine weather – the boats (Greenwich) start from Hungerford Bridge at the times – 20 minutes and 40 minutes – and if it would suit you to go at the 2 o'clock train I will meet you at Hungerford pier –

Believe me my dear Faraday | Very faithfully yours | I.K. Brunel

1. See letter 3304 and also note 1, letter 3302.

Letter 3306

Faraday to Isambard Kingdom Brunel

26 June 1857

From the original in BrUL MS

[Royal Institution embossed letterhead] | 26 Jun 1857

My dear Brunel,

I have no doubt the man obeyed his orders very properly¹ – do not find fault with him – I send you back the note – he declined to see any note or call any body before 1 o'clk – Thanks for your kindness for the Saturday i.e. Saturday the 4th of July tomorrow week – *I cannot go tomorrow*_[.]

Ever Truly Yours | M. Faraday

1. See letter 3305.

Letter 3307

Faraday to Jean-Baptiste Biot

27 June 1857

From the original in the possession of Charlotte and Georg Both

Royal Institution | 27 Jun 1857

My dear & honoured friend,

I long to perform the duty of offering my deep respect to you with the warmest wishes for your happiness and I rejoice to think that in doing so I may introduce to you Dr. Tyndall. I know your true & catholic love for the workers in Science & that when they are young you are pleased to encourage them by your own example and I do not think you will consider me imprudent upon the present occasion but will as I hope think I have chosen a fitting messenger for the sincere affection and deep feeling which I permit myself to bear towards you_[.] Hoping and desiring that you will very long offer to

science the example of a sound mind in a sound body a pleasure to yourself & a rejoicing to all around you_[.]

Believe me to be as Ever | Your Very faithful Servant | M. Faraday
a Monsieur | Monsieur Biot | &c &c &c

Letter 3308

Faraday to Jean-Baptiste-André Dumas

29 June 1857

From the original in AS MS

Royal Institution | 29 Jun 1857

My dear friend,

Dr. Tyndall is about to visit Paris and as one whom I esteem highly both for his personal qualities and his devotion and services to science I give him this letter of introduction to you:— though indeed I think he will be sufficiently well known to you by what he has done already as hardly to need it at my hand. But the occasion gives me an opportunity of calling myself to your remembrance and though you have given me plenty of proofs that I am not out of it when there is need still I like to be there also at other times.

My dear Wife joins me in kindest remembrances to you and Madame Dumas_[.]

Ever Your Obliged & faithful friend | M. Faraday
a Monsieur | Monsieur Dumas | Secrétaire | &c &c &c

Letter 3309

Faraday to George Buchanan

29 June 1857

From the original in RI MS F1 N/1/29

[Royal Institution embossed letterhead] | 29 Jun 1857

My dear George,

Next Saturday¹ I have arranged for us to go & see the Great Eastern² if nothing arises to prevent it. At present the understanding is that we be at Hungerford Bridge next Saturday at 2 o'clk to meet Mr. Brunel & go by the Greenwich boat which leaves at that hour.

Yours affectionately | M. Faraday
Mr. Geo Buchanan

Address: Mr. Geo Buchanan | 7 Cambridge Terrace | Liverpool Road | Islington

1. That is 4 July 1857.

2. See letter 3305 and letter note 1, 3302.

Letter 3310**Julius Plücker to Faraday****1 July 1857****From the original in IET MS SC 2**

Bonn | 7 Jul 1857

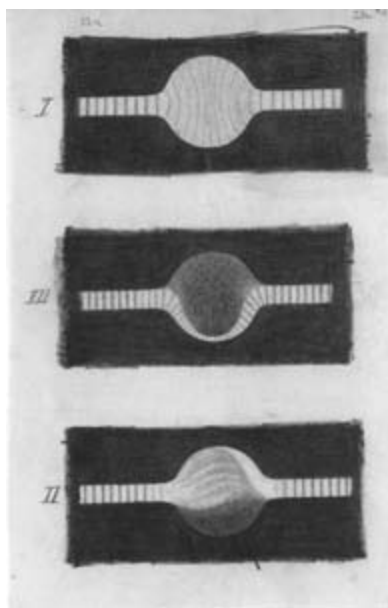
Dear Sir!

Let me first thank you for the kind letter of the 23rd March¹ and the interesting paper², I received nearly at the same time. I learned by the letter that you have sent my Memoir³ to Prof. Miller of Cambridge, the foreign Secretary. Since I got not the least notice about it, I fear therefore that I was ill informed with regard to the rules of the Royal Society, when I supposed, that papers, presented to the Society, (if approved) were published, in all cases announced in the proceedings. If the Memoir should get the honour to be printed, I would have found a friend, to eliminate the incorrectness of the language. My former views abandoned by myself since two years, being always reproduced in Journals (for instance by Mr. Verdet⁴ in the "Annales de Chimie"⁵) I am interested to have my Memoir published, but, under the actual circumstances, I think myself not entitled to publish its contents.

I fear, Sir, to annoy you, when speaking again about the difference with Prof. Tyndall. Being desirous to have this difference settled, I think it best, not to enter into any detail. Allow me only a few words in answering your letter. I don't know what reason Prof. Tyndall has to believe that I charged him with *wilful* misstatement. I published no word against him, except the note p. 6 of my paper⁶, and whatever should be my personal feelings, I would not publish such a charge.—With regard to Mr. de la Rive's relation quoted by Prof. Tyndall⁷, the fact is this. After having showed to him all my experiments, he put to me, by letter, a great number of questions. These questions, difficult to be answered by yes and no, admitted partly a double meaning, you might refer them to the pure fact or to its theoretical explication: I fear to have induced him myself to some expressions contrary to my own real meaning.—The reason of the misstatement is to be sought in some strange circumstances, by which I got my paper of 1849 (of which I gave you an abstract January 1850⁸) published only 1852⁹ and therein that it was not translated neither into English nor French.

I worked not much this last time. I got only confirmed my theory of the magnetic induction of crystals in some new cases. Lately too I made a series of experiments in order to get an explication of the stratification of light, exhibited first by Ruhmkorff's apparatus within certain rarified vapours, but equally obtained in every rarefied gas and also by the electric spark or a series of them taken from a conductor, by retarding the discharge by means of less well conducting bodies¹⁰. I have the opportunity here to obtain tubes of glass, of different forms, filled with any gas or vapour whatever at a measured tension, through which you may send the electric charge by means of

platina wires. The various experiments are beautiful. You may easily observe the influence of tension and heat, the various spectra, sounds etc. But *most beautifull* is the effect, when the tubes are placed in the Electromagnet, in different ways, as well axially as equatorially. A tube 16–18 inches long showed many hundred blak intervals equally distant from each other; in other cases the blak intervals are larger and distant $\frac{1}{4}$ to $\frac{1}{6}$ of an inch. The blak spaces are differently directed when put axially on the iron pieces of the Electromagnet. The current is under certain circumstances interrupted by the Electromagnetism. It is impossible to give by a sketch an idea of the most splendid phenomena. I join a *very imperfect* sketch of a more simple case. (I the tube and the sphere contains rarefied hydrogen gaz, II & III the sphere is put in the equatorial direction on the iron pieces and the polarity comūtated) Whatever may be the variety of appearances the *general* phenomena of inflection of the luminous current by the Magnet is indicated by theory.



With all my heart | Yours | Plucker

Endorsed by Faraday: 1 July 1857

1. Letter 3258.
2. Probably Faraday (1857a), Friday Evening Discourse of 27 February 1857.
3. Plücker (1858f).

4. Marcel Emile Verdet (1824–1866, P2, 3). Professor of Physics at the Ecole Normale, Paris.
5. Verdet (1854), 375.
6. Plücker (1858f), 545.
7. Tyndall (1855), 2.
8. Plücker to Faraday, 4 January 1850, letter 2249, volume 4.
9. Plücker (1852). This paper was sent to the Haarlem Society of Sciences in December 1849, but it remained unpublished. See p. 1 of the published paper.
10. On this work see Plücker (1858a, b).

Letter 3311**Faraday to Lyon Playfair****2 July 1857****From the original in IC MS LP256**

Thursday, 2 Jul 1857

My dear Playfair,

I did not mean to thank you this way for your kind invitation but I was frustrated in my intention last night & so have only this method left. I hope you & many enjoyed the evening as I should have done for I have been to the place & think it exceedingly interesting[.]

Ever Truly Yours | M. Faraday

Endorsed by Playfair: London 2 July/57 | Michael Faraday | Apologising for not being at my Conversazione

Letter 3312**Faraday to Frederick Gye¹****4 July 1857****From the original in FSL MS Y.c. 889/2**

[Royal Institution embossed letterhead] | 4 Jul 1857

My dear Sir,

Again I thank you for the feast of last night²[.] The sleep-walking scene was wonderful – all was extraordinary, & what an excellent Macbeth you had³. I was very glad of that for Madame Ristori's⁴ perfection⁵ would not have seemed right in contact with an inefficient unmeaning husband. I suppose you will hardly agree with me but it seems to me that the call for Mad Ristori immediately after that scene indicates an utter want of feeling poetry and good taste in those who make the call.

In thanking for the past, let me say that we leave town I believe on Tuesday next⁶ and should not be able to profit by any remainder of kindness that might happen to be in your mind[.]

Ever Truly Yours | M. Faraday

F. Gye Esqr | &c &c &c

1. Frederick Gye (1810–1878, ODNB). Manager of Covent Garden Opera, 1848–1878.
2. This was the Italian version of William Shakespeare's *Macbeth* performed at the Lyceum. See *The Times*, 3 July 1857, p. 8, col. f.
3. Cesare Vitaliani (1824–1893, EI). Italian actor.
4. Adelaide Ristori (1822–1906, WWW1). Italian actress.
5. As Lady Macbeth.
6. That is 7 July 1857.

Letter 3313

Josiah Latimer Clark to Faraday

7 July 1857

From the original in IET MS SC 2

Electric Telegraph, Off[ice] | 7 Jul 1857

My dear Sir,

I send you the Enclosed¹ merely for your information, and confidentially. It is from a brother in law of mine² whom I know very intimately & who is a Captain in the Royal Artillery.

The Statements he makes must of course be perfectly incredible to any minds not prepared to receive them. Either he has been deceived, or the facts are false or they are true.

As to his deception I leave you to judge – he is what the world would call a “very cute and wide awake fellow” up to all the ways & tricks of the world, intelligent, & with plenty of sharpness & common sense, & could not be deceived by any *ordinary* kind of trickery or deception, but he is not at all scientific or accustomed to scientific methods of experiment or research.

As to his intentional misstatement his position as an officer bearing Her Majestys Commission forbids the idea, & I know him personally well enough to vouch *without the slightest hesitation* for the *truth of this statements*_[.]

If we assume then that the facts are true, what are we to say to them?

All I can say is that I will endeavour to get an opportunity of seeing them for I cannot deny that I feel interested in the very astonishing statements which are made to me from time to time, or evidence which I cannot well disbelieve.

Very sincerely Yours | Latimer Clark

M. Faraday Esqr.

Do not trouble yourself to give any reply to this note

1. Drayson to Clark, 7 July 1857, IET MS SC2. This refers to Crosland, C. (1857).
2. Alfred William Drayson (1827–1901, *Month. Not. Roy. Ast. Soc.*, 1902, **62**: 241–2). Royal Artillery captain (later a general) and story writer.

Letter 3314**Faraday to Josiah Latimer Clark****7 July 1857****From the original in New York Public Library**

[Royal Institution embossed letterhead] | 7 Jul 1857

My dear Sir,

I am on the point of leaving town but hasten to acknowledge your letter¹. As I said before when you can do any of these things I shall be willing to look a little closer at them. But how is it that the believers in these things make such a shouting out for the scientific men? Why do they not become scientific themselves and prove their own so called facts as scientific men prove their facts. If they are so much wiser than scientific men as to form a sure judgment when the latter are wrong why do they want to fall back upon them? To me this desire to include the men of science is a proof that they do *secretly doubt* what they wish to believe. If they be the locomotives of knowledge why should they wait for that which to them is only a slow donkey cart jogging along the common road?

Ever Truly Yours | M. Faraday

Latimer Clark Esqr | &c &c &c

Endorsement: This has reference to some Spiritual Seances I asked Professor Faraday to attend. JL

1. Letter 3313.

Letter 3315**Faraday to Benjamin Vincent****9 July 1857¹****From the original in RI MS F1 E3**[Royal Institution embossed letterhead], Old Buckenham |
Thursday, 3 oclck

Very dear friend,

Our journey yesterday was prosperous. We arrived in due time at Attleboro, – found Mrs. Brown² there, – went with her to their house & took tea there; & then my wife, Mrs. Brown, one of her daughters, and myself went in a fly to Old Buckenham, – in time for the meeting, – and indeed in time for more:– for seeing Mrs. Fisher³ in the garden, & speaking to her, I found the time & state of our dear friend Mr. Fisher was very opportune for seeing him. He was very glad to see me – his voice & his looks more cheerful; – his words full of hope & comfort to those, who like his wife and myself, had the privilege to hear him. He talked without seeming to feel it a burden; though of course I would not stop long, – but his voice was feeble. He had no cough whilst I was there & the room was most comfortable in its state. I told him of

the happiness we had on Sabbath evening & it was just the subject to rejoice him. After a little while the brethren met in the meeting house & we had reason to be happy together[.]

This morning I hear that Mr. Fisher passed a night that may be considered as quiet & favourable – this morning as was to be expected he had a heavy fit of coughing & clearing – after that he went into a quiet sleep which continued & as yet we have not seen him to day. Mrs. Fisher is to let us know how things proceed – or if a favourable time for my wife occurs. Mrs. Fisher herself bears things with quietness & as a Christian woman & the friends seem well. Mrs. Bigsby⁴ as you will know is in town & we have heard that her son is gone⁵.

We have seen several here but the year which has passed since I was here the last time⁶ has made a great change in many points – some of which you will be aware of. My wife is not fit for much here but I am in hopes we shall not be too much for our friends – or unacceptable in being here – they are very kind – our views take no shape as yet – but will depend day by day.

With kindest love to all believe me my dear friend.

Very Truly Yours | M. Faraday

Mr. Vincent

$\frac{1}{2}$ p. 4 oclk. Have just seen Mr. Fisher – he is about as yesterday – but Mrs. Fisher says the Doctor gives no hopes.

1. Dated on the basis of the visit that Faraday and Sarah made to Old Buckenham between 8 and 14 July 1857 (DUA Acc M/409/5/3, p. 143) and also on the death of William Fisher on 21 August 1857.

2. Ann Brown. Member of the Old Buckenham Sandemanian Church from 1852. DUA Acc M/409/5/3, p. 131.

3. Mary Marsh Fisher (d.1898, age 82, GRO). Wife of William Fisher and member of the Old Buckenham Sandemanian Church, 1832–1876. DUA Acc M/409/5/3, pp. 121, 131.

4. Maria Bigsby (d.1884, age 78, GRO). Member of the Old Buckenham Sandemanian Church from 1840. DUA Acc M/409/5/3, p. 131.

5. William Bigsby (1838–1857, GRO). Tailor who died on 7 July 1857.

6. Faraday's visit is noted in DUA Acc M/409/5/3, p. 142.

Letter 3316

Peter Henry Berthon to Faraday

10 July 1857

From the original in GL MS 30108/2/77

Trinity House, London, E.C | 10 Jul 1857

Sir,

Referring to previous correspondence on the subject of the Catadioptric Lighting Apparatus intended for use at the Bishop Rock Light House, I am directed by the Elder Brethren to state that, being desirous that the Light to be exhibited from that Light House should be of the most powerful description, and having been informed that the Apparatus in question,

although purchased in the Year 1850, combines all the advantages of recent improvements, and having reason to believe that the Experiment to which the two Sections of it which are fitted up at Blackwall, were subjected on the Evening of the 31st March last¹, did not afford a just Estimate of it's power, in consequence of the shortness of the distance from which it was observed, the Elder Brethren are desirous of obtaining your further opinion on the subject.

It has been suggested that a holophotal Apparatus of 12 Sections to the Circle may be found practically more effective than that in question which contains 24 Sections, I am therefore to request you will favor the Elder Brethren with your opinion as to which of the three you consider may with the best effect, having regard to the Elevation, Position, and intended purposes of the Bishop Rock Light, be used in the exhibition of the Light thereat, viz.

The Apparatus already procured, A holophotal Apparatus of 12 Sections, or a Catoptric Apparatus, with the reflectors arranged in the same manner as the three which were observed at Blackwall on the Evening above alluded to.—

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq. | &c &c &c | Royal Institution | Albemarle Street

1. See letter 3265.

Letter 3317

Faraday to Julius Plücker

11 July 1857

From the original in NRCC ISTI

Norfolk | 11 Jul 1857

My dear Professor,

I left town very shortly after receiving your last letter¹ but not before I had inquired at the apartments of the Royal Society about your paper². My memory is so bad, that I cannot recollect the circumstances under which I presented it, or the time; but I believe I wrote you word on that occasion³. When I called lately I saw Mr. Weld the assistant secretary, and found, from him, that the paper had been received and entered in a book:— he told me that Mr. Stokes the Secretary, was to communicate with you; but as there was no further note made, he concluded that he had not yet done so — Mr. Stokes is just married. I did *all I could* to expedite proceedings, but as I do not belong to the Council or take any office I am quite without power in the matter.— I have myself presented a paper to the Royal Society⁴ — I believe it is to be printed though I have never been informed officially that that is to be the case.— I think

it must be near a twelvemonth since I sent it in⁵, but I have seen no printers proof as yet – The proceedings are indeed very slow.

With respect to You & Tyndall I cannot pretend to explain the misunderstandings which exist:– and having tried a little I do not think that any third person between you can be of any use. To be clear it requires that the parties should communicate directly, and plainly, with each other; and then I think it would be impossible not to discover where the mistake lies. He is at present abroad.

I am very glad that you are working on the stratified electric light.– I hope that you will very shortly give us the fundamental explanation of the phenomenon. I cannot help thinking that it will aid us in developing some very important points about the nature of the electric discharge. We would rejoice to understand, truly, the first principles of that very striking electric action. The variation of the intervals to a certain degree at pleasure is exceedingly interesting – but what is the state of an interval?

I must conclude saying that I am as ever

Very Truly Yours | M. Faraday

Professor Plucker | &c &c &c

1. Letter 3310. Faraday left London on 7 July 1857, see letter 3314.

2. Plücker (1858f).

3. Letter 3258.

4. Faraday (1857c).

5. The Royal Society received this paper on 16 November 1856.

Letter 3318

Jean-Baptiste Biot to Faraday

11 July 1857

From the original in RI MS F1 I62a

Très cher et très honorable ami,

Je vous remercie de m'avoir adressé le Dr. Tyndall¹. Je l'apprécie doublement: pour son mérite propre, et pour l'ardeur heureuse avec laquelle il continue de poursuivre les voies de découvertes que vous avez révélée aux explorateurs de la nature. Mais, ce donc je vous remercie encore davantage, ce sont les marques d'obligeant souvenir et de bonté affectueuse, que vous me donnez dans la lettre qu'il m'a remise de votre part. rien ne saurait m'être plus précieux que d'avoir obtenu l'estime et l'amitié d'un homme tel que vous, dont j'admire la génie, autant que j'honore le caractère. Je reçois avec un vif sentiment de reconnaissance les vœux que vous formez pour moi et j'en suis profondément touché. Mais c'est à vous surtout que la science doit souhaiter une longue vie et une santé solide, afin que pendant bien des années encore, vous puissiez continuer de l'agrandir par des découvertes aussi importantes qu'imprévues.

Agréez, avec votre bienveillance habituelle, l'expression sincère |
 de ces sentiments, qui vous sont depuis longtemps acquis | votre ami |
 J.B. Biot
 Monsieur le professeur Faraday à Londres

TRANSLATION

Very dear and very honourable friend,

I thank you for sending Dr. Tyndall to me¹. I appreciate him both for his own merit and for the happy ardour with which he continues to follow the paths of discovery which you have revealed to the explorers of nature. But I thank you even more for the signs of obliging recollection and affectionate kindness which you give in the letter which he delivered on your behalf. Nothing could be more precious to me than to have obtained the esteem and friendship of a man like you, of whom I admire the genius as much as the character. I receive with a true sense of gratitude the wishes that you send me and I am profoundly touched by them. But it is to you above all that science must wish a long life and robust health, so that for many years to come, you may be able to continue to enlarge it by your discoveries, which have been as important as they have been unforeseen.

Please accept, with your customary kindness, the sincere expression |
 of these sentiments, which you acquired a long time ago,

your friend | J.B. Biot
 Professor Faraday in London

Address: à Monsieur | Monsieur le Professeur Faraday l'un des | associés
 étrangers de l'institut de France | &c &c &c | Royal Institution | London
 Postmark: 11 July 1857

1. See letter 3307.

Letter 3319

Faraday to Peter Henry Berthon

13 July 1857

From the original copy in GL MS 30108/2/77

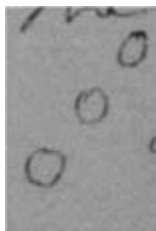
Old Buckenham, Norfolk | 13 Jul 1857

Sir,

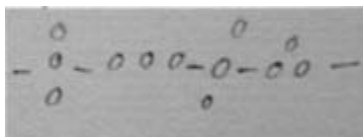
Although the distance at which the 24 section holophotal light was observed at Blackwall was comparatively small yet it was quite sufficient to determine the point then under consideration, namely, the separation of the successive flashes by intervals of darkness¹. As to the difference between

a twelve & a twenty four section apparatus, equal in workmanship and supplied with the same central lamp it is dependant upon optical principals which cannot be altered by distance. A beam from the 12 section apparatus will have twice the power of a beam from the 24 section apparatus at equal distances from both whatever that distance may be. If the two apparatus revolve in a given time – 12 beams from the first & 24 from the second will pass a given place in that time. The duration of the light of each beam will be nearly the same but the light from the first will be of twice the intensity of that from the second – at the same time the dark periods of the first will be much longer than those of the second probably three times as long. But I think there can be no doubt that the light from the first will be far more penetrating & therefore better than that from the second[.] If the second apparatus is made to revolve in twice the time of the first then an equal number of beams will pass the same place on the horizon in the same time. As before they will be of only *half the intensity* of those from the first apparatus but they will endure for *twice the time*. Speaking therefore in respect only of Optical principles I should conclude that an apparatus of 12 sections would be much better for the Bishop rock lighthouse than one of 24 sections[.]

I am not prepared to speak with the same distinctness as to the comparison of a 12 section light and a Catoptric apparatus consisting of 36 lamps & reflectors arranged on 12 faces[.] The arrangement of the three reflectors at Blackwall was thus



and such an arrangement is very good for preventing the interference of the currents from the upper & lower lamps, but as respects the light at a distance the effect will be the same whatever the position of the three lamps



provided the reflectors be close to each other & their axes parallel. There ought to be more light in the beam from three reflectors with their lamps than in the beam from a twelve section apparatus; for the amount of light in three good Argand lamps is between 2 & 3 times that which goes into & is sent forward by one of the twelve sections. But I have not my notes of the Blackwall Evening here² & though I should be surprized if the 3 reflectors did not much surpass one section I am not sufficiently acquainted with the respective appearance of two such beams at a distance at Sea as to offer any opinion which can be of value in the presence of the experience & observation of the Elder Brethren³[.]

I am | Sir | Your Obedient Humble Servant | M. Faraday
P.H. Berthon Esq | &c &c &c

1. This was Faraday's reply to letter 3316.

2. These notes are in GL MS 30108/2/77.

3. This letter was read to Trinity House Court, 14 July 1857, GL MS 30004/27, p. 96. It was referred to the Lights Committee.

Letter 3320

John Tyndall to Faraday

18 July 1857

From the typescript in RI MS JT TS Volume 12, pp. 4074–6

Chamouni, Montanvert | 18 Jul 1857

My dear Mr. Faraday,

I think I may fairly allow myself the rest and luxury of writing to you this morning for I have had a week's hard work as far as limbs and arms are concerned. I reached Paris just in time to learn that the youthful wife¹ of a young friend of mine², whom you once saw (before he was married) at my lodgings in Islington³, was in her coffin. I accompanied my smitten friend to the cemetery of Montmartre, and there accident caused it to fall my lot to shake ashes to ashes, earth to earth, dust to dust. I thought it would never do to leave my friend amid scenes which would incessantly revive the memory of his loss and so persuaded him to come with me⁴. He is now here, and we are working hard together.

At Paris I saw Mr. Biot⁵ and he desired me to say to you that he was as happy as man can be. He received me with great kindness. Pouillet⁶ and Chevreul⁷ I also saw⁸, and they were very cordial. [Blank in TS] introduced me afterwards to M. Becquerel⁹, but I found him *hard* and not cordial¹⁰. I must crave your forgiveness for asking you to write to Dumas¹¹, for when it came to the point I could not render to myself a sufficient reason why I should call upon him; I therefore did not do so. I spent a couple of days in Geneva: Those I knew were all in the country, but DelaRive found me out at my

hotel and I spent one delightful afternoon at his country house¹². He is a fine genial loveable man. From Geneva to Chamouni was a day's journey. The weather was glorious: as we passed Mont Blanc a patch of reddish light was thrown upon the mountain snow from some clouds which floated in the west. Around this patch a subjective green glory spread itself to some distance: it was very curious and very beautiful. I stayed 3 days at Chamouni, and rose one morning to see the sun rise on Mont Blanc. My bedroom opened into a corridor from one end of which the east was visible, and the other commanded a view of Mont Blanc and of the west. The east sky was of an amber hue, fading insensibly into a rosy violet, which again blended with the deep blue of the zenith. The morning star was glistening between east and west, and not far from it the moon turned her pale face towards the rising day. The mountain rose chaste, and cold, and white, as the unsoiled snow could make him. I walked to the other end of the corridor and looked eastward, hoping to see the sun lift his disk above the mountains, wholly forgetful for the moment that if I waited until he appeared, it would be sunrise not only for Mont Blanc, but for the whole valley. People do very absurd things thus unconsciously. I walked to the other end of the corridor, and saw the highest summits smitten by the sunbeams. Peak after peak then lost its severity and melted into a golden smile. It was a glorious scene. I watched it till the morning star was quenched and the moon became invisible, and then being very tired went to bed again!

On last Wednesday¹³ I had all my things carried up to the Montanvert. There is a kind of hotel erected here which possesses three bed rooms. They are divided from each other by partitions of wood, and the noise of the tramping visitors would render sleep in my case an impossibility. I have therefore chosen a little temple as my habitation which was erected many years ago by an Englishman named Blaire¹⁴, and dedicated by him "*a la Nature*". Its floor is of stones which are rather wet, its walls of the same material and in the same condition; They have put a bed into it, and given me a goatskin to keep my feet from the flags; and I contrive to drive away a little of the moisture by a pine fire. I never felt more like a philosopher than when I sit there alone at night, listening to the wind moaning over the glacier, and to the distant rumble of the stones upon the moraines as they tumble into the crevasses. Hitherto I have chiefly occupied myself with observing the motion, and obtaining a general notion of the glacier. I have seen Balmat and he informs me that Forbes is on his way over, so that I expect to have the pleasure of seeing him soon. From what I have thus far seen the divergence between us is likely to become *wider*. But the question is merely shaping itself in my brain, and I can only hope that after a little time it will take definite form. The weather thus far has been so good that I have been every day upon the ice. I am very strong and sometimes admonished by my guide for making use of my strength in jumping the crevasses. I regret that I did not bring a pair of Nichols prisms

with me: but I shall have plenty to occupy me without them. With kindest remembrances to Mrs. Faraday and Miss Barnard

Believe me always | Most sincerely Yours | John Tyndall

Would you have the goodness to ask Anderson to put the enclosed into envelopes, with the addresses and post them for me? If he would send me my letters once a week until I write to him he would oblige me[.]

1. Anna Hirst, née Martin (d.1857 see ODNB under T.A. Hirst). Married Hirst in 1854.
2. Thomas Archer Hirst.
3. See Hirst, *Diary*, 21 January 1854, RI MS JT/2/32b, p. 1108.
4. Tyndall, *Diary*, 3 and 4 July 1857, 7: 2–5.
5. See letters 3307, 3318 and Tyndall, *Diary*, 6 July 1857, 7: 7–8.
6. Claude-Servais-Mathias Pouillet (1790–1868, DSB). Professor of Physics in Paris.
7. Michel Eugène Chevreul (1786–1889, DSB). Director of Dyeing at the Gobelins tapestry works and Professor of Chemistry at the Muséum d'Histoire Naturelle.
8. Tyndall, *Diary*, 6 July 1857, 7: 7–8.
9. Antoine-César Becquerel (1788–1878, DSB). French chemist who supported the contact theory of the Voltaic cell.
10. Tyndall, *Diary*, 6 July 1857, 7: 7–8.
11. Letter 3308.
12. Tyndall, *Diary*, 9 July 1857, 7: 11–16.
13. That is 15 July 1857.
14. P. Blair, an English Alpinist who erected a hut in the late eighteenth century, is mentioned in Engel (1971), 64.

Letter 3321

Faraday to Charles Richard Weld

25 July 1857

From the original in RS MS RR 3.223

[Royal Institution embossed letterhead] | 25 Jul 1857

My dear Sir,

I return you Professor Plücker's paper¹. Though founded on experiment it is mathematical in character and in that respect far beyond my powers of judgment. I am therefore unable to form any opinion of its accuracy and because of that am unable to help the Committee of papers in deciding upon its eligibility for publication²[.]

I am | My dear Sir | Very Truly Yours | M. Faraday
C.R. Weld Esqr | &c &c &c

Endorsement: 1857 Plücker by Faraday (decl[in]ing to report)

1. Plücker (1858f).
2. The paper was refereed by William Thomson and George Gabriel Stokes (RS MS RR 3.222 and 224 respectively) and approved for publication on 10 December 1857 (RS MS CMB90d).

Letter 3322**Faraday to Culling Eardley¹****25 July 1857****From the original in the Maddison Collection, Templeman Library,
University of Kent at Canterbury**25 Jul 1857²

Mr. Faraday presents his compliments & very sincere thanks to Sir Culling Eardley for an exceedingly kind invitation for the 23rd. He returned to town only yesterday afternoon & so was unable either to profit by or acknowledge it[.]

1. Culling Eardley (1805–1863, B1). Politician and evangelical.

2. The Royal Institution embossed letterhead has been cut out from this letter.

Letter 3323**Faraday to John Barlow****27 July 1857****From the original in RI MS F1 E2**

Royal Institution | 27 Jul 1857

My dear Barlow,

I consider that whether a letter of mine to you contains five or five hundred lines of writing it would be of equal value for I know that I can tell you little that you do not know better than I do that the matter in fact is of no value & on the other hand that five lines from a true friend is abundantly sufficient to call very much to remembrance. Mr. Vincent gave me your address this morning until the 31st & that sets me off. We came in on Friday¹ after a fortnight of physical rest though we had been among friends where death as we believe is present though our friend yet lives². We are both pretty well. We find the house in great actual confusion but in the full order of repair³ – nearly all the drains open but happily very fine weather for our purposes. As far as I know all is well with us & I trust it is so with you & Mrs. Barlow. If I remember rightly you did not enjoy your last trip abroad. I hope the present will bring & leave nothing but pleasant remembrances[.]

Tyndall is away among the ice at Chamouni. I had a letter from him on Saturday⁴ – he expects (as he hears from Balmat) to meet Forbes there. So we may expect that regelation may be thoroughly worked out & all its principles & results developed both as regards man & ice[.]

My wife & Jeannie are out or I should have messages from them to you & Mrs. Barlow but you know what they would be as well as I do. All health & happiness & pleasure be with you both[.]

Ever My dear Barlow | Most Truly Yours | M. Faraday

1. That is 24 July 1857, after visiting Old Buckenham and Chesterfield. DUA Acc M/409/5/3, p. 143.
2. That is William Fisher.
3. See RI MM, 1 June 1857, 11: 193 and 6 July 1857, 11: 198.
4. Letter 3320.

Letter 3324

Faraday to John Tyndall

28 July 1857

From the typescript in RI MS JT TS Volume 12, p. 4142

Royal Institution | 28 Jul 1857

My dear Tyndall,

I received your very welcome letter¹ just on our return from Derbyshire², and thank you heartily for its news. So far you seem to have gone on well, i.e. increasing in health and spirits. I trust it will be so, and that your pursuit of nature and her truth will be such a labour of love as to bring health to both body and mind. So you are at Chamouni, and on the ice, and likely to meet Forbes there. I am not sorry for it. I have trust enough in you as to believe that two such men as yourself, though you may have differed on some conclusions, cannot meet in the face of nature, with like love of truth, without advantage. I shall hope the best. You both have a very important witness and mistress to refer to in nature, and I know she will favour the most right minded.

Since you were at Paris, I have had a very pleasant letter from Biot³. It would give you a just pleasure to know the terms in which he speaks of you. What a fine old man he is! I am very glad that you saw him, and that he has seen you.

We have no news just now, except about drains and painting and such like matters⁴. The house is in such a state that I cannot work, and if it were in order, I am too weary. Perhaps I ought to say, too lazy. All I know is that I feel tired in creeping up stairs, and find the sofa and a book the best things for me.

We have many foreigners, visitors here. De Vry⁵ [sic] has shewn himself, and is now off for Java. Soret⁶ of Geneva is here. He brought me a letter from De la Rive⁷, speaking of your visit to him. The brothers Schlagintweit⁸ from India are here also, as wiry and active as ever; there is no wearing out about them, and they bring me excellent accounts of Humboldt.

You see what poor things I have to tell you about; but great is nature and will prevail. I wish I had a little *Schönbein* power, and then you should have some letters fit to class as such. But indeed you do not want to be teased with many. All things are well as they are, if we would only be content.

My dear wife and niece desire their kindest remembrances and wishes. You know we are of one mind in these thoughts towards you. I am glad to hear

you are able to have your friend with you⁹. I hope it will be some comfort to him, and some pleasure to you. Pray make my sincere respects to him. I have no right to intrude with sympathy, but I do feel deeply for his loss.

Ever, my dear Tyndall | Most truly yours | M. Faraday

1. Letter 3320.
2. DUA Acc M/409/5/3, p. 143 noted that Faraday was going to Chesterfield after leaving Old Buckenham on 14 July 1857.
3. Letter 3318.
4. See letter 3323, note 3.
5. Willem Hendrik de Vriese (1806–1862, BWN). Dutch botanist.
6. Jacques Louis Soret (1827–1890, P2, 3). Swiss physicist.
7. Not found.
8. Adolph Schlagintweit (1829–1857, ADB). Geologist at the University of Munich. Hermann Rudolph Alfred Schlagintweit (1826–1882, ADB). German geologist and explorer.
9. Thomas Archer Hirst. See letter 3320.

Letter 3325

Faraday to Juliet Pollock

7 August 1857

From the original in RI MS F1 N/1/30

[Royal Institution embossed letterhead] | 7 Aug 1857

My dear Mrs. Pollock,

I write to you not to answer your letter but to acknowledge it. One cannot answer formally – the feelings must do that. I was very grieved to hear of the sad loss¹, and called upon you very spontaneously; for as you know I am so far out of the usages of Society that my calls may often be considered as intrusions; but I know it could not be so in this case and with you[.] Give my kindest remembrances & thoughts to those whom you are with. We often have to contemplate death:– we grieve when those we love & esteem are taken from us. Yet death ought not to be an unusual thought:– and I think it ought not to be an unhappy thought, – for there is abundant provision made against that. But this leads to sacred things, which, though indeed they should be common things, are such as I ought not to talk about here[.]

You speak of a book,– and if that comes to pass without any constraint or necessity upon those who may have to interfere in the matter, I shall indeed be very glad to have such a remembrance. It was but a few weeks ago that I was looking at certain photographs which brought Miss Herries very vividly to mind[.]

I hope you will meet Miss Herries the sister² with a grateful & grave happiness – my kindest thoughts towards her. As regards the things which make the memory of a friend dear to us, we ought to remember that in proportion as they have been worthy of our respect & imitation so is our sorrow; yet the worthiness that makes the sorrow we would not wish to have been

less:– it was for the time, & is still, an added blessing. Kindest remembrances to Mr. Pollock³. My wife (& niece) kindest remembrances – I have sent off Tyndalls letter⁴._[.]

Ever Very Truly Yours | M. Faraday

1. Maria Julia Herries who died on 14 July 1857.
2. Isabella Ann Herries (d.1897, age 79, GRO). Daughter of John Charles Herries (1778–1855, ODNB). Tory MP for Stamford, 1847–1853.
3. William Frederick Pollock (1815–1888, ODNB). A Master of the Court of Exchequer, 1846–1886.
4. Pollock to Tyndall, 5 August 1857, RI MS JT/1/P/165.

Letter 3326

Faraday to Percival Norton Johnson¹

12 August 1857

**From the original in Håndskriftafdelingen, Det Kongelige Bibliothek
Copenhagen, MS Palsbo AC**

[Royal Institution embossed letterhead], 21 Albemarle St. |
12 Aug 1857

My dear Sir,

Could you send me by post *three* portions of *platinum* wire – of about the respective diameter of $\frac{1}{100}$ $\frac{1}{80}$ $\frac{1}{60}$ of an inch – about an ounce altogether² – with the bill & I will send in a day or two & settle it – & so oblige

Yours Very Truly | M. Faraday

P.N. Johnson Esqr | &c &c &c

1. Percival Norton Johnson (1792–1866, ODNB). Assayer and metallurgist.
2. Faraday was working on platinum light at this time. See Faraday, *Diary*, 11 August 1857, 7: 15582–92; 31 August 1857, 7: 15616.

Letter 3327

John Tyndall to Faraday

15 August 1857

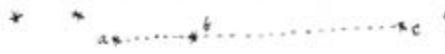


From the typescript in RI MS JT TS Volume 12, pp. 4070–3

Chamouni | 15 Jul [sic] 1857

My dear Mr. Faraday,

I felt very thankful to you for your letter¹, which quite connected me with the life of London by making me acquainted with what was going on there. I am at present seated in the house of my guide at Chamouni, with Huxley seated at the opposite end of the table at which I write. Last Sunday and Monday the rain descended heavily at the Montanvert, and snow fell upon the higher elevations. Yesterday (Friday) clouds collected around the summit of Mont Blanc, and thunder was the harbinger of heavy rain, which

has continued incessantly all this day. I have thus noticed two rainy termini, between which however a sunny interval stretched, a fine clear day and a fine moonlight night, and in this interval it was my fortune, or doom, to make an attempt upon the "Monarch of Mountains", wishing to learn something from it if possible. Accompanied by a single guide, the best part of whose valour was discretion, my friend Hirst and myself set out from the Grand Mulets at 2 o/c. on Thursday morning, and after 14 hours toil reached the summit of Mont Blanc, made some experiments upon the snow, fired a little extempore cannon, and pledged a toast to some of our absent friends. I thought you would not object to your name being linked with that of Saussure, so I pledged you both together, for to say the truth, my quantity of beverage was too scanty to afford a separate bumper to each. I had also the pleasure of carving Mrs. Faraday's name and those of two or three other ladies upon the atmosphere at that high elevation. Our return to the Grand Mulets occupied upwards of three hours, making the excursion, from this point to the summit and back to the Mulets again, 17 hours in all. It is usually accomplished in 10. But the snow presented a fearful obstacle to our progress; we had to wade through it for hours knee deep, and sometimes actually up to the hips. The toil was excessive, and we reached the Grand Mulets just in time to prevent the peril of darkness from being added to the peril of the crevasses which beset us. Standing upon a rock in the midst of the ice and snow on Wednesday night, I noticed a singular effect of star twinkling which you have probably observed, but which was quite new to me. Supposing this to represent the Great Bear,

Bear, a line drawn through the two stars a,b, and produced,
cut a  ^{bright} high star at c.
This star I   noticed changed its colour incess-

a line drawn through the two stars a,b, and produced, cut a bright star at c. This star I noticed changed its colour incessantly, sometimes it was a bright red and the next instant it changed to a vivid green. In some cases the succession from one colour to the other was very rapid, but in others the colour remained constant for several seconds. I called Huxley's attention to it, and he saw the green and red at exactly the same times as I did. I had also the corroborative testimony of my friend Hirst, who was with me. When Hirst and I were wandering up the ice slopes by moonlight a few hours afterwards, he noticed that the self same star, which had approached the zenith, had ceased to twinkle. Probably all this contains nothing new to you, but the vividness of the colours was to me extremely remarkable. I suppose everybody must have observed the influence of the position of a star upon its twinkling. In the

squares and parks of London I have often noticed that stars near the horizon twinkled powerfully, while those in the zenith shone with an almost steady light; I think you once mentioned to me that you had observed this yourself.

Since I wrote to you last², I have been hard at work on the glacier; but the problem is a large one, and it is only now that I am commencing to feel some mastery over the particular form under which it presents itself here. It is always so. It requires long pondering and experiment before I can seize anything with clearness, and I am often disposed to consider myself a very slow coach. But I suppose I must be content with the conditions under which Nature grants me insight, and not repine because I do not possess the quick flashing intellect of other men. Since I last wrote to you I also made the ascent of the Col du Géant, with no guide, but with a little boy, who can climb well, for my companion. Neither of us knew anything of the route; and we found ourselves during part of the time surrounded by considerable perils. We succeeded however in reaching the summit, and in returning to the Montanvert without a single broken bone. The pass is, I believe, considered to be one of the most perilous in the Alps. In this way, by refusing to conform to the rules of the guides, and claiming from the Guide Chef the liberty which ought to be granted to a scientific observer, I have been able to accomplish very heavy excursions for about the tenth part of the ordinary expense.

But my work here is drawing to a close simply through the exhaustion of my funds, so I hope to be in London in about 10 or 12 days from the present time. You would oblige me by asking Anderson not to forward me any letters after the date on which this reaches you. I hope to find Mrs. Faraday and Miss Barnard and yourself well and happy on my return. I am in capital health: burnt brown as an Indian. My guide and friend are both almost blind from ophthalmia produced by the glare of the snow, but this I have escaped, and should have been hard at work today if the weather had permitted it.

With best wishes | Believe me always | Most sincerely yours |
J. Tyndall

1. Letter 3324 which is the basis on which this letter is dated.

2. Letter 3320.

Letter 3328

Julius Plücker to Faraday

17 August 1857

From the original in IET MS SC 2

Bonn | 17 Aug 1857

Dear Sir,

During your glorious career of scientific researches it certainly may often happened, that you entered a question having very little hope to get a satisfactory result. Such is my case on the present occasion. There is held at Bonn from

18th to the 24th of September the 33th meeting of the Germain Association¹. All is done to render it a splendid one, a great number of eminent men from all countries are expected. Every body would regard your presence as a most precious one. You dont like festivals, I know,– but you may move here quite free. You dont like traveling abroad, but changing the air of London and its environs with the air of the Rhine, will certainly do good to your health – when my own head is tired, I run away in any direction, and, when returned, I feel myself restored. Let me be the advocate of our Association, if I should succeed – a case more wished for by myself then expected – I shall be happy to take care of *all* regarding your stay here at Bonn. Having no office on this occasion you may fully dispose on me.– I dont expect any answer but in the extraordinary case of a favourable decision.

I thank you for your last kind letter², which announces me that the presented paper (addressed to the foreign Secretary³) is not lost⁴. With patience I'll expect its future destiny.

From all my heart | Yours | Plücker

1. That is the Gesellschaft Deutscher Naturforscher.

2. Letter 3317.

3. William Hallowes Miller.

4. Plücker (1858f).

Letter 3329

Faraday to John Barlow

19 August 1857

From Bence Jones (1870a), 2: 380–1

Highgate | 19 Aug 1857

I am in town, and at work more or less every day. My memory wearies me greatly in working; for I cannot remember from day to day the conclusions I come to, and all has to be thought out many times over. To write it down gives no assistance, for what is written down is itself forgotten. It is only by very slow degrees that this state of mental muddiness can be wrought either through or under; nevertheless, I know that to work somewhat is far better than to stand still, even if nothing comes of it. It is better for the mind itself – not being quite sure whether I shall ever end the research, and yet being sure, that if in my former state of memory, I could work it out in a week or two to a successful and affirmative result.

Do not be amazed by what I am telling you: it is simply the thing I remember to tell you. If other things occurred to my mind, I would tell you of them. But one thing which often withholds me, is, that if I begin a thing, I find I do not report it correctly, and so naturally withdraw from attempting it. One result of short memory is coming curiously into play with me. I forget how to spell. I dare say if I were to read this letter again, I should find four or

five words of which I am doubtful, “withholds, wearies, successful” &c.; but I cannot stop for them, or look to a dictionary (for I had better cease to write altogether), but I just send them, with all their imperfections, knowing that you will receive them kindly.

Ever, dear Barlow, truly yours | M. Faraday

Letter 3330

Faraday to Benjamin Vincent

25 August 1857

From the original in RI MS F1 E4

Highgate | 25 Aug 1857

My dear friend,

I take the cool of the morning to communicate with you for the weather has been so exceedingly hot, and I feel it so much, that even to write a letter or notes in the middle of the day has been oppressive. We hear that all is going on well, & my wife was glad to have a letter: she has thanked you really, if she has not yet written, I cannot call her writing to mind.— Here we are all about as usual. Many away for health mingled with recreation, some for illness alone. Among the latter Mr. Martin¹ was confined at home on Sabbath day which is not usual with him. I had hoped that he was in the country, for I think it is fifteen years since he took a trap to the seaside or elsewhere. Mr. Paradise whom you will know to be with us looks quite as well as usual:— he returns home this week — It is interesting to observe how fast this world changes. When we are away from our accustomed places the gathering alteration goes on & soon makes an amount that we hardly expected:— and so, if we only remember it, information to those who are away soon waits in quantities. With us, we have had both grave & joyful matters. Yesterday midday I had a note from Mr. Macomie² at Old Buckenham, which however was dated Friday³, stating that Mr. Fishers time had come. He died on the Friday supported, as he had been so evidently of late, by a power not human, but of God. On Sabbath day we had the the [sic] happiness to receive a young man William Douglas into the church⁴[.] He is no connexion of any of the friends, though Mrs. Charles Baxter⁵ has been his very kind acquaintance for a little while back. He made his way into the Church nothing stopping him:— he spoke to the Elders before dinner; to the Church in the Evening, & was joyfully received by all. It is often striking, & was in this case, to see one come in who, not been accustomed to the manner amongst us, is in that respect new; & to many, as it may appear peculiar, the *truth of the matter* being however the same; & then it strikes most as a new thing for a little while, & gives rise to questions; & the answers to them may often make oneself think of our own habits of thought. It just shews that wherever God gathers his people out of the world the teaching is the same. It was delightful to hear

him take the scriptures, & *them only* as the rightful declaration of the purpose & way of God & the guide to the Church. So by gathering into his house, & also by gathering into his everlasting kingdom, living & dying, we have had given to us the evidence of the presence of him who is ever watchful over his creatures & is bringing all things on in their course towards *that day*.

We looked in at your house the other day (when we were taking tea at Mr. Whitelaws⁶)_[.] All your party looked well there; & they say they are getting on quite successfully. From what I heard from Charles [Vincent] I hope there is some probability of a good entrance into practical life;— but you will know all about it.

Margery [Ann Reid] is here;— they are coming down stairs & I am sure would wish me to send their remembrances to you & Mrs. Vincent. You know how we should like to be remembered to friends, Mr. Boyston⁷, Mr. Philip⁸, Dr. Chrichton [sic]⁹, Mr. Macintosh¹⁰ &c &c_[.] I put ourselves in your hand[s.]

Ever dear Brother | Affectionately Yours | M. Faraday

Address: Mr. Vincent | Mr. T.H. Baxter¹¹ | Murraygate | Dundee

1. David Watson Martin (1798–1884, Cantor (1991), 301). A member of the London Sandemanian Church.

2. Alexander Macomie (d.1872, age 77, GRO). Bookbinder and deacon in the London Sandemanian Church, but excluded for the second time in November 1857. DUA Acc M/409/5/4, p. 48.

3. That is 21 August 1857.

4. William Douglas. This admission was noted in the roll of the London Sandemanian Church, DUA Acc M/409/5/4, p. 55b. He was excluded in 1858, but restored until 1871 when he was excluded for a second, and thus final, time.

5. Unidentified.

6. George Whitelaw (d.1872, age 68, GRO). Publisher's manager and an Elder of the London Sandemanian Church. Cantor (1991), 302.

7. Unidentified.

8. Unidentified.

9. John Crichton (1772–1860, B1). Dundee surgeon and member of the Glasite Church there.

10. Daniel MacIntosh (d.1860 age 78, DUA MS 9/4/2(28)). Schoolmaster of 9 Panmure Street, Dundee, and member of the Glasite Church there from 1807.

11. Thomas Handyside Baxter (d.1863, age 79, SRO). Merchant of 95 Murraygate, Dundee, and member of the Glasite Church there.

Letter 3331

Faraday to Julius Plücker

27 August 1857

From the original in NRCC ISTI

Royal Institution | 27 Aug 1857

My dear Sir,

Your letter¹ is so kind that I cannot let it pass in silence, though it gives me pain to say no to you:— but, though I cannot come, I thank you and all my friends about you for the kindness. I should have rejoiced to have been

amongst you all but the time is past; Years & their consequences limit our powers, & though I trust yours will long run on successfully, mine are drawing nigh to their end. The British Association is now I believe holding its meeting at Dublin and I was very kindly & considerately pressed to be there. But I am obliged to have *one* answer & only one to these friendly applications. Remember me to my friends.— May all be happy & successful with you.

Ever Very Truly Yours | M. Faraday
Professor Plücker | &c &c &c

1. Letter 3328.

Letter 3332

Kaiserliche Academie der Naturforscher in Breslau to Faraday

3 September 1857

From the original in RS MS 241, f. 151

Academia Caesarea Leopoldino Carolina Naturae Curiosorum,
Breslau den | 3 Sep 1857

Sr. Wohlgeboren

Herrn Professor Dr. Michael Faraday | zu | London.

Von Dr. *Berthold Seeman*¹ und Dr. *Joseph Dalton Hooker* zur Aufnahme in die Kaiserl. Academie der Naturforscher (zu Breslau) vorgeschlagen, ersuchen wir Sie hierdurch ganz ergebenst der Academie zur Ausfertigung des Diploms *alle Taufnamen, Titel, Aemter, Orden und Würden, Doctorgrade, sowie alle Mitgliedschaften von gelehrten Gesellschaften und wissenschaftlichen Vereinen, jedoch alles in der grössten Vollständigkeit und womöglich deutsch*, baldigst einzusenden, da wir auf andere Weise zu diesen Notizen *vollständig* für unsern Zweck nicht gelangen konnten. Hochachtungsvoll

Ew Wohlgeboren | Die Academie der Naturforscher

TRANSLATION

Academia Caesarea Leopoldino Carolina Naturae Curiosorum,
Breslau den | 3 Sep 1857

Honourable Sir

Mr. Professor Dr. Michael Faraday | in | London

Dr. *Berthold Seeman*¹ and Dr. *Joseph Dalton Hooker* have nominated you for admission into the Kaiserliche Academie der Naturforscher in Breslau. We request that you send us as soon as possible *all your baptismal names, titles, positions, orders and honours, doctoral degrees, as well as all memberships of learned and scientific societies* and if possible *in German* because we are unable to obtain this information in any other way.

With great esteem | Yours honourably | Die Academie der Naturforscher

Endorsed by Faraday: Prussia

Address: An to 21 Albemarle St | Professor der Physik Dr. Michael | Faraday | Wohlgeboren | London.

Angelegenheiten der Kaiserl | Leopoldinisch Carolinischen | Academie der Naturforscher.

1. Berthold Carl Seemann (1825–1871, ODNB). German-born explorer and botanist.

Letter 3333

Faraday to John Tyndall

5 September 1857

From the typescript in RI MS JT TS Volume 12, p. 4143

Highgate | 5 Sep 1857

My dear Tyndall,

I have hesitated in writing to you to acknowledge yours¹, for I thought you were coming home quickly – at least so I understood Anderson; but I hope you will enjoy the *quiet* country, for I doubt whether Switzerland could be called *quiet* to you, occupied as you were. We shall be very glad to see you in due time, and then you and I will talk of the lectures and other things which you refer to. I do not feel as if my hand could write much distinctly, and indeed I doubt whether this will find you in Hampshire; but wherever and ever yours | Very truly | M. Faraday

1. Not found.

Letter 3334

Faraday to Hamilton George Edward Earle¹

9 September 1857

From a photocopy in RI MS

[Royal Institution embossed letterhead] | 9 Sept 1857

Sir,

If a tube were so full of fog as that you could not see through it I do not expect that a current of electricity would convert the fog into water: I should think you might glaze the ends of your tube so well as that fog should not get into it in an injurious degree in any moderate time.

I believe the proposition to observe the compass placed above has been already made[.]

Do not quote my name [in] any way in reference to your plan[.]

I am Sir | Your Obedient Servant | M. Faraday

Lieut. Earle R.N | &c &c &c

Address: Lieut Earle RN. | &c &c &c | 12 Downing Terrace | Cambridge

1. Hamilton Edward George Earle (1829–1902, *The Times*, 28 May 1902, p. 12, col. c). Royal Navy officer.

Letter 3335

Christian Friedrich Schoenbein to Faraday

17 September 1857

From the original in UB MS NS 431

Bâsle | 17 Sep 1857

My dear Faraday,

The moment is fast approaching, which will separate my eldest daughter from her home, parents and friends, and as you may easily imagine, all of us, and Mrs. Schoenbein most particularly, are looking for that painful hour with feelings appropriate to the case. The girl herself, however, exhibits on that occasion more courage than I thought she could command and though being fully conscious of what she is about to undertake, the wicked woman does not seem to have lost any of her wonted spirits. If I were not fully convinced that my daughter should find a second home with your friend, I certainly would not have given my consent to her emigrating there; and, besides, my knowing, that you and Mrs. Faraday are near her, gives me confident hopes, that she will be well off in every respect and meet with good counsel as often as she will happen to stand in need of it. Pray be kind to the girl, for though my child, I am allowed to assure you, that she is a good and excellent creature, who, I have no doubt, will please you Mrs. Faraday and Miss Hornblower.

Within a month my second daughter Sophia, who by the bye was intended to be a philosopher, also will leave me to go to the far North and stay over the winter with some friends of mine at Altona. Though she will be placed in very good hands, still to be deprived of two daughters at once, who have these many years given so much life to our little domestic circle, is rather hard to their parents and younger sisters; but we cannot help and must suffer the girls to go their own way. To Mrs. Schoenbein those separations will cost floods of tears in spite of her stoical maxims. In the course of this summer I have been travelling and working. I saw once more my favorite town "Nuremberg" that splendid and interesting monument of the middle ages, where every house, court and street puts you back to times and a state of things long gone by. At the same time I visited old intimate friends, (and that was the principal end of my journey) who thirty some years ago were my fellow students at the University of Erlangen. I need hardly tell you, that I enjoyed my trip very much and spent most delicious days with those old cronies of mine, one of whom happens to be one of the most extraordinary and amiable men, I know, being highly poetical, therefore full of imagination, rich of original ideas, of a matchless humor, teeming with wit and what I do not consider as the least of his many excellent qualities, full of the milk of

human kindness, in short a genuine man every inch. In the little book which will be presented to you by Miss Schoenbein, the author has tried to depict the man in the chapter entitled "Der Freund"¹. Having once touched that "opusculum", the authorship of which I leave you to guess, I beg you to accept that trifling as a keepsake. It is sort of "quodlibet" and hardly worth while to be read. But if you should feel inclined to know a little what curious notions a friend of your's is entertaining on "Men and things", get some chapters of the little work translated to you. I see that the author has taken the liberty to talk even of yourself, but have reason to think, that in doing so he was actuated by the most friendly motives.

I have continued my researches on oxygen, that inexhaustible source of investigation, and ascertained a series of novel facts which seem to be not altogether void of scientific interest. One of those facts is queer and paradoxical enough. What do you say to a desoxidation of an oxycompound being effected by the means of oxygen itself? You are perhaps aware, that some years ago I found out a number of substances enjoying the power to transform free \ddot{O} into O i.e. to act like heat. The oxides of the precious metals and the metallic peroxides such as that of manganese, lead &c. belong to that category, and which are oxy-compounds containing either all or part of their oxygen in the ozonic condition. Now it appears, that the action taking place for instance between free \ddot{O} and $PbO + O$ is reciprocal; for not only the former happens to be converted into O , but the peroxide of lead is at the same time reduced to PbO , which seems to show that the \ddot{O} of $PbO + \ddot{O}$ also becomes desozonized and on that account eliminated. The same desoxidizing effect is produced upon PbO_2 by the ozonized oil of turpentine and the peroxide of hydrogen (to me = $HO + \ddot{O}$). To show those remarkable effects in a simple manner, I employ a test-paper being impregnated i.e. colored with minute quantities of peroxide of lead. If moist strips of that paper be suspended in strongly ozonized air, within a few hours they will be completely bleached i.e. PbO_2 reduced to PbO . The said test paper on being put in ozonized oil of turpentine or peroxide of hydrogen undergoes the same change. The strips joined will show you that effect. Now those curious facts seem to me to give room to a very strange conjecture, which, extraordinary and startling as it may sound, I cannot help communicating to you. I suspect that there are two kinds of active oxygen, standing to each other in the relation of algebraic magnitudes of contrary signs i.e. being such, as will neutralize each other into active oxygen, if brought together in equal quantities. Now supposing that there are three kinds of oxygen \oplus , \ominus , O and assuming that HO_2 is = $HO + \oplus$ and $PbO_2 = PbO + \ominus$, those peroxides, on being brought in contact with one another, must be catalized, because the \oplus of the one neutralizes the \ominus of the other peroxide into O , which as such can no more rest associated either with PbO or HO . For the reduction of PbO_2 to PbO being effected by free \ddot{O} , I am inclined to account in the same way i.e. by assuming opposite states of the two portions of oxygen, which act upon each other. In the two isomeric and crystallographically polar

acids of Pasteur's², which neutralize each other into what they call racemic acid³, we have a case of an analogous kind.

I am, of course, far from believing, that the fact above stated necessarily lead to such a conclusion, but for the present I cannot conceive any other hypothesis, by which the desoxidizing effect being produced by free O, ozonized oil of turpentine or peroxide of hydrogen upon PbO₂ could be better accounted for. Be that however as it may, as we philosophers cannot do and notably work without having some hypothetical views in our heads, I shall place myself for some time under the guidance of the conjecture alluded to and see what can be made out of it. If it leads me to the discovery of some interesting facts, I shall not feel ashamed of it, though it may turn out to be fallacious. We are no gods but shortsighted men and must be content with finding out a little bit of truth in wading through a sea of errors.

You know, it is an old notion of mine, that common oxygen as such cannot enter into any chemical combination and must undergo a change of condition i.e. become ozonized before it acquires oxidizing powers. The oxidation of phosphorus, oil of turpentine &c. being apparently effected by common oxygen is typical to me, because we know, and I think with sufficient certainty, that in those cases the ozonisation of common oxygen always precedes oxidation. In order to increase as much as possible the body of evidence speaking in favor of that assumption of mine, I have of late worked a good deal on the oil of bitter almonds (Benzule + H), which, as to its bearings to common oxygen, is certainly one of the most remarkable bodies, I know; for that oxygen being at the common temperature placed under the joint influence of the oil named and solar light, effects a number of oxidations, which only O³ but not O is capable of causing. Under the circumstances mentioned Jodine is eliminated from jodide of potassium, indigo solution discolored rapidly enough, the solution of guajacum blued, a great number of metals oxidized, even Silver not excepted &c. To convince yourself of that action in a simple way, add to dilute paste of starch, containing some jodide of potassium, a drop or two of hyduret of Benzule (free from prussic acid), shake in the dark that mixture together with ordinary oxygen, and no action will result; make the same experiment in the sun and the liquids will almost instantaneously be turned deep blue, just so as if free ozonized oxygen had acted upon the paste. The same color will make its appearance, if you treat in a similar way a recently prepared solution of guajacum. To show that even Silver is oxidized, put some drops of our oil upon a plate of pure silver and having the essence moved about in direct sunlight for a minute or two, aqueous sulphuretted hydrogen being poured upon the spot of reaction, will cause a rather abundant precipitation of sulphuret of silver, a proof of the presence of oxide of silver. I need not expressly state, that the hyduret of Benzule is oxidized along with the metals, in consequence of which benzoates are

formed: benzoate of Lead, Cadmium, Copper, Silver &c. A very pretty experiment may be made with metallic Arsenic. Lay round a glass tube a ring of that metal (according to Marsh's methode⁴) drop some oil of bitter almonds upon it, turn the tube being held in a horizontal position round its axis, no action in the dark, whilst in the direct solar light that ring will rapidly disappear under the circumstances indicated, arsenic acid being formed, just as is the case in ozonized oxygen. Rings of Antimony being not acted upon, or at least but very slightly under these circumstances, both the metals may be easily distinguished from each other by the means of hyduret of Benzule. The details of my researches on the oil of bitter almonds will be published by the academy of Munich⁵.

You know, that nitrification has been these many years a matter of interest and research to me and of late I have increased our knowledge about that subject by some novel facts. Some years ago, I found out, that ozonized oxygen transforms Ammonia into the nitrate of that base; last year I ascertained, that inactive oxygen on being put in contact with platinum or copper acquires the power of oxidizing even at the common temperature the elements of Ammonia into nitrous acid and water, nitrite of Ammonia being formed under these circumstances.

Now I have discovered that HO_2 , Mn_2O_7 (permanganic acid) or the salts of that acid, for instance permanganate of potash, on being mixt up with aqueous Ammonia produce nitrites⁶. A singular fact is that free ozonized oxygen alone seems to be capable of oxidizing the nitrogen of Ammonia into nitric acid, the ozonized oxygen of oxy-compounds or the oxygen being rendered active by the influence of copper or platinum produces nitrous acid. Are we to infer from those facts, that the formation of a nitrite is the first stage of a nitrification?

One gambol more on my hobby-horse and I shall descend from the animal. I have of late succeeded in ozonizing the oil of turpentine so strongly that one equiv. of that essence is associated to one equiv. of oxygen, and you may easily imagine the great oxidizing power of that oil. By shaking it with peroxide of lead it becomes desozonized, PbO_2 being reduced to PbO , a fact, which according to the statements above made, is a matter of course.

Now you are released my dear Friend from listening to the talkings of a loquacious philosopher to whom, I hope, you will prove indulgent as you have already so often done him that favor.

Now nothing more than the request to remember me friendly to the most gracious She-Sovereign of the Royal Institution, whom you will beg in my name to take my girl under her high protection.

In hoping that you and Mrs. Faraday are doing well I am, my dear Friend
Your's | most faithfully | C.F. Schoenbein

I must add a remark or two on my peroxide-testpaper. I prepare it by drenching strips of this filtering paper with a solution of PbO_2 and that solution is produced by shaking together (for about 15 minutes or so) two volumes of strongly ozonized oil of Turpentine and one volume of Extractum Saturni (subacetate of lead). On filtering that mixture I get a transparent liquid being colored like portwine which in fact is oil of turpentine holding some peroxide and oxide of lead dissolved. Upon the filter remains a yellow substance being a mixture of PbO_2 and PbO . Within 24 hours a similar mixture is deposited out of the colored essence. It is a remarkable fact, that the test-paper is rapidly bleached in strongly insolated atmosph. air, as you will see from a strip laid by, which in a good sun was completely bleached within an hour's time. For that reason my test paper must be kept in the dark.

1. [Schoenbein] (1855a), 168–73. That is Adolph von Zerkog (1799–1880, DBE). German landowner and politician.
2. Louis Pasteur (1822–1895, DSB). Professor of Chemistry at Lille, 1854–1857.
3. Pasteur (1850).
4. James Marsh (1794–1846, ODNB). Chemical assistant at Royal Military Academy, 1829–1846, who developed a test for arsenic.
5. Schoenbein (1857c).
6. Schoenbein (1858b).

Letter 3336

Faraday to Miss Rutt¹

25 September 1857

From the original in APS Misc MS Collection

[Royal Institution embossed letterhead] | 25 Sep 1857

My dear Miss Rutt,

You place me in a difficulty which I hope you will allow me to explain. Much as I have valued many volumes containing autographs or other collected matters I have always been opposed to their formation by direct contribution and have no other answer to the many applications than the power to say that according to my general rule I cannot accede to them_[.] It is very true that I proposed an *if* to Mr. Williams²; to come off at the time as a joke_[.] If you press me I will write the line but I thought I would throw myself (as I am circumstanced) upon your kind consideration and as I am obliged to be in town to day³ would not delay to call on you but explain at once. Be merciful & think of my future consistency_[.]

Ever My dear Miss Rutt | Most faithfully Yours | M. Faraday

1. Unidentified.
2. Unidentified.
3. From Highgate. See letter 3333.

Letter 3337**Faraday to Christian Friedrich Schoenbein****25 September 1857****From the original in UB MS NS 432**

Royal Institution | 25 Sep 1857

My dear Schoenbein,

I cannot leave my desk without telling you that yesterday I saw your daughter & received your letter¹. I went as soon as possible to Stamford Hill and found Miss Schoenbein very well though not yet recovered from her fatigue – looking cheerful & happy and as far as I could judge she & Miss Hornblower mutually pleased with each other. You need not doubt that she will find a most kind and careful friend in Miss Hornblower a woman of method & discipline but who by her tenderness & care makes all about her love her. You know one cannot judge of results in a hurry, but the first appearances are most favourable. I dare say Miss Schoenbein will find enough to do but a good moral atmosphere to do it in & hearty good will on all sides – We shall learn by degrees what opportunities the routine supplies & we shall hope to see her at our house when that is proper after our return home².

I can easily imagine Madame Schoenbein's anxiety but except from what may be founded on difference of habits in our two countries she need have none[.] It so happens that I have three nieces with Miss Hornblower at this time & I hope they will make a friend of Miss Schoenbein & that you will hear a word or two about them now & then – Two of them are sisters to Jeannie³ whom I think you have met here[.]

As to the philosophy of the letter I must enjoy & talk about that another time or else I shall lose the post.

Ever My dear friend | Yours | M. Faraday

Our kindest thoughts on this occasion to Mrs. Schoenbein & the Sisters | M.F.

1. Letter 3335.

2. This suggests that this letter was written from Highgate. Letter 3333.

3. Katharine Barnard (1844–1912, GRO) and Rachel Barnard (1845–1929, GRO, death under Blaikley). The youngest surviving children of John and Margaret Barnard.

Letter 3338**Warren De La Rue to Faraday****28 September 1857****From the original in IET MS SC 2**

110 Bunhill Row, London E.C | 28 Sep 1857

My dear Mr. Faraday,

I am greatly obliged to you for the copy of your paper on the relation of light to metals¹ which I have nearly read through – you have made a most interesting investigation of the phenomena, and will probably hereafter do

much more in the same path. It was very scrupulous on your part to name me in your paper for the slight assistance I was able & glad to render².

The full moon was on the 4th of September, as you say,— but very early in the morning (5h. 7m AM.) and as the photograph was taken on the 7th at the 14–15 hour (the 8th at 3o clock AM) nearly four days had elapsed, so that fully one fourth of the moon was in shadow:— added to which those parts most in shadow do not produce a sensible impression on the plate by the time the lighter portions are overdone; So that all the lunar surface visible to the eye is not depicted:— the original photograph, however, shows a little more than the copies taken from it, but not quite all that could be seen with the eyepiece.

Do not hesitate to do what you require with the fragments of speculum metal, they are quite at your disposal. If you would like to cast them into other forms it is easily done and they are very readily polished quite true:— will you allow me to put you in the way of doing this? It is quite easy and you would have no difficulty in doing it yourself much better than you will get it done by another, curved specula are also very easy to make.

It is quite astonishing how much of the actinic rays is stopped by the atmosphere when the moon is situated, as it has been lately, at only a few degrees from the horizon — I could not obtain on the 25th the slightest impression of the moon in 25 seconds whereas in 5 seconds when well situated the plate is almost over done. A slight haze which barely diminished Jupiter's light stopped so much of the actinic effects that I could not last night obtain the slightest trace of an image in 25 seconds under such circumstances, whereas in 12 seconds just previously I obtained a capital impression. I believe that there is room for a curious investigation in this direction. Did you try gold in its green & purple states in regard of its transmitting or excluding the actinic ray?

Yours Very truly | Warren de la Rue

1. Faraday (1857c).

2. *Ibid.*, 152.

Letter 3339

Peter Henry Berthon to Faraday

3 October 1857

From the original in GL MS 30108/2/76

Trinity House, London | 3 Oct 1857

Sir,

The Board of Trade having requested the Corporation's opinion of the accompanying Plan for sounding Fog Bells by means of an Electro Magnetic

Machine,— I am directed to forward the same to you, and to request you will favor the Elder Brethren with your views thereon.—

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq: | &c &c &c

Letter 3340

Faraday to Peter Henry Berthon

6 October 1857

From the original copy in GL MS 30108/2/76

Royal Institution | 6 Oct 1857

Sir,

The machine (of which a drawing & description has been sent to me¹) called “Barbarin & Sims Electro Magnetic fog bell” resolves itself into two parts one a clock working by ordinary mechanical power (apparently a spring) the other an Electro magnetic machine to lift the hammer & spring not shewn in the drawing by which the bell is to be struck at regular intervals determinable by the clock. The Voltaic battery & its associated arrangements have no other purpose than to perform mechanical work, exactly as any other power as that of a man horse or steam engine would do it. The work to be done is the lifting up of the hammer & the spring upon it:— and the machine, to be of any advantage, must either supply the requisite power more cheaply than other sources of force or it must be a source of force more convenient & available than any other which can be conveyed to a given station.

As to the expense of the force all attempts hitherto made have shown that it is greater with the Electro magnetic machine than with steam or horse or man power and I have not the slightest idea of any advantage in the respect. As to any particular convenience the machine may possess I do not find that such are mentioned in the description nor does any occur to me. The apparatus would require a peculiar kind of care and I expect could not be so safely left to itself as an ordinary machine supplied with a descending weight or any other motor. If a man be left in charge of it then I think it probable that the man power might be more cheaply & safely applied than the power of the Electro magnetic machine_[.]

Nothing can be easier than to try the machine and obtain proof of its ability but certainly such trials should not be made at a station but at home. All that is necessary is so to arrange the apparatus that it shall lift a weight; to ascertain what weight it can lift in a certain number of hours, to a given height, and at what expence; & then compare these results with an ordinary apparatus & the price of man power sufficient to do the same work. Whilst this comparison is being wrought out any other supposed advantages (though I do not find that such are referred to) can be observed and considered²_[.]

I am Sir | Your very faithful Obedient Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. With letter 3339.

2. This letter was read to Trinity House By Board, 13 October 1857, GL MS 30010/41, p. 147. It was agreed to write to the Board of Trade accordingly.

Letter 3341

Faraday to Theobald Schideck¹

8 October 1857

**From the original in Deutsche Akademie der Naturforscher Leopoldina
MS 28/13/2/328–31**

Royal Institution, London | 8 Oct 1857

Sir,

I have received a letter I believe from your department². I am unable to read German but a friend has told me its purport and it is such as to require my very sincere & humble thanks. To give you an account of my engagements and honors would be really out of my power[.] I therefore take the liberty of enclosing such mention of them as is placed upon the list of the Fellows of the Royal Society of London³ and think that will be abundantly sufficient to meet your wishes[.]

I have the honor to be | Sir | Your Obedient humble Servant |
M. Faraday

The honourable | The Secretary of the | Academy of Breslau | &c &c &c

Address: The honourable | The Secretary of the | Academia C.L.C. |
Naturae Cu | &c &c &c | Breslau | Prussia

1. Theobald Schideck (d.1878, Bohley (2003), 413). Secretary of the Academia Caesarea Leopoldina-Carolina Naturae Curiosorum.

2. Letter 3332.

3. See p. 12 of Royal Society list of Fellows for 1857.

Letter 3342

Apsley Pellatt¹ to Faraday

14 October 1857

From the original in RI MS F1 I82a

Staines | 14 Oct 1857

My dear Doctor,

Several years since you shewed me a number of portraits of your friends & acquaintances & asked me if I could give you a Lythograph of my own which I did not then possess; if you think the accompanying photograph would answer the same purpose I shall deem it an honour to be placed

among the eminent worthies in your portfolio² undeserving as I may be of the favour[.]

With kind regards | I remain | My dear Sir | Yours very sincerely |
Apsley Pellatt
To Dr. Faraday FRS | FGS

1. Apsley Pellatt (1791–1863, ODNB). Glass manufacturer.

2. This photograph is mounted opposite this letter.

Letter 3343

Faraday to Peter Henry Berthon

15 October 1857

From the original copy in GL MS 30108/2/77

Royal Institution | 15 Oct 1857

Sir,

Though it would be presumptuous in me to suppose it necessary that I should give an opinion on the relative appearance of the lights exhibited last Tuesday night¹, since the Deputy Master² & many of the brethren were present to make their own observations, yet it may be expected that I should make such remarks as occur to me, not merely as regards the comparative power of the lights, but the relation of the effect to the original amount of light really concerned in the four different cases.

The apparatus at Blackwall presented on its revolution Eight lights in succession, which were observed from the top of the Lunatic Asylum at Brentwood, 15 miles in direct distance in E.N.E direction. The night was clear & moonless the stars shone well; the experiments proceeded in due order & the observations were well made. One of the lights consisted of *seven parabolic reflectors* with their lamps on one face. This light was very good (rather reddish to my eye) and endured for a certain amount of time, due to the amount of divergence (which is assumed as about 15°). It had a sensible size or diameter; what the astronomers would call a disc like effect in contradiction to a star light effect. Another of the eight lights was a *great lens* 29 inches wide & 37½ high:— it was alone, having no corresponding reflectors or refractors above or below, & was lighted by a central Fresnel lamp. Its light was very good:— white, & I think brighter, than the former one; but it did not last so long because of the lesser divergence:— it was a flash appearing & disappearing suddenly. The light was more compressed & star like than the former. The two lights were very nearly equal in effect; the first would last longest but probably be extinguished before the other by distance. Two other of the eight lights consisted each of *three parabolic reflectors* & their lamps arranged on one face; Buchaness fashion. These lights were much beneath the former in power; & the effect was what might be expected from the comparison of three with seven reflectors. They were arranged in a quarter of a circle, at the angle

which would be taken by the introduction of eight into the circle. The two lights were of course well separated by darkness from each other. The other four lights resulted from the juxtaposition of four of the *lenses* of what has been called the *Bishops Light* associated with the same central lamp as that used with the great lens. Each of these lenses is 9 inches wide by $50\frac{1}{2}$ inches high; and each is accompanied by refracting reflectors above & below. It is intended that 24 of these shall occupy the circle, & the four now in the light house were placed at the proper angle; consequently the lights followed each other quickly, but were well separated by intervals of darkness; corresponding in that respect with the observations formerly made from Woolwich³. Each light however was feeble and not equal to the Buchaness arrangement of three reflectors on the same face[.]

But besides the intensity of the light, there is the cost at which it is produced & the number of times it will recur in one revolution; the latter circumstance determining in a considerable degree, though not altogether, the *time* during which it will be seen. Perhaps these points may be most simply stated by assuming four light houses. If the first of these were illuminated by the system of 7 *reflectors* on a face, then *three* such faces would fill the circle the lighthouse would use the light of 21 lamps, and each flash would use the light of 7 lamps; only three flashes could be seen in one revolution. If the second had a central lamp equal, when at the best, to 13 or 14 Argands, *eight large lenses* with their aiding reflectors above & below could be placed around it: eight flashes would be seen in each revolution, and as the whole would only take the light equal to 13 or 14 Argands, each would apportion to itself an amount of light little more than $1\frac{2}{3}$ of an Argand lamp. If the third lighthouse were filled up with the Buchaness arrangement there would be also *eight illuminations* in one revolution; each would employ the light of three Argand lamps instead of the proportion of $1\frac{2}{3}$ rd of the large lens arrangement. The fourth lighthouse would contain 24 of the *Bishops light lenses* in the circle;— there would be 24 feeble flashes in one revolution, and each would employ light equal to $[(13 \text{ or } 14)]/24$ i.e. little more than half an Argand light. It is to be remembered however that when comparing the seven reflectors with the great lens, & the three reflectors with the Bishops lens, the reflectors give a longer light than the lenses; &, that part of the excess of light which they use, is employed in producing this prolonged effect.

The level of the top of the Asylum is higher than that of the lamps at the Trinity wharf; hence the observations were not made in the axes of the rays. As at the distance of 15 miles, a degree = 1320 feet, so, even with a difference in height of 200 feet, the observer would be only 9' out of the axis of the ray, which where the divergence is 12° or 15° would give a very

small difference, & that difference would be proportionate for all the lights compared.

The signal lights & rockets were most of them doubtful or invisible to me. Of those which were seen I could only be sure of the colour of one: which was red. One of the rockets was very good for this great distance⁴.

I remain Sir | Your Very Obedient humble Servant | M. Faraday
P.H. Berthon Esq | &c &c &c

1. That is 13 October 1857.
2. John Shepherd.
3. These observations were made on 31 March 1857. Faraday's notes are in GL MS 30108/2/77.
4. This letter was read to Trinity House By Board, 20 October 1857, GL MS 30010/41, p. 160. It was ordered to be entered in the book of scientific reports.

Letter 3344

Peter Henry Berthon to Faraday

15 October 1857

From the original in GL MS 30108/2/76

Trinity House | 15 Oct 1857

My dear Sir,

I was about to write to the Board of Trade on the subject of the Electro Magnetic Fog Bell, on which subject my letter was to be nearly an echo of your report¹, when in examining the drawing it occurred to me that the principal Object of the invention, viz: the Distance from the machine at which the power can be made available, has not been adverted to by you.

I apprehend that altho' this application of the force is not dwelt upon in the plan or explanation it is that upon which the inventors mainly ground a claim to consideration, particularly as the drawing attached to the plan represents the machine on the shore linking Bells (by submerged wires) on *rocks* at a considerable distance in the Sea at which it may be said that neither steam, horse or man power could be used.

I am afraid that if this part of the subject is passed sub silentio we shall have a rejoinder from the inventor that we have missed his principal point.

Will you kindly give the matter a little consideration, and advise me as to what I should say on this point?

Believe me | My dear Sir | Yours very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. Letter 3340.

Letter 3345**Faraday to Peter Henry Berthon****16 October 1857****From the original copy in GL MS 30108/2/76**

Royal Institution | 16 Oct 1857

Sir,

A slight suspicion of the point you mention in your last letter¹ regarding the fog bells occurred to me and I refer in my letter² to the arrangement "as a source of force more convenient & available than any other which can be conveyed to a given station," but I observed that no such reference is "mentioned in the description": and none occurred to me, because, as the clock must be wound up by a man, & as a man must be at it to set it a going when required for use (for the electrical battery as described would not do that), so I concluded that a man would be in charge of it & therefore that it would be placed where men were.

But if such an application is intended as you suppose then it is not fair to ask for an opinion where no data are given. Are the rocks near the shore or are they distant will not the constant [two words illegible] of a man in the vicinity be required. Will the man in charge of the battery be within hearing of the bell in all states either of fog or surf or wind or other sources of noise. Unless he hears the bell he cannot be sure the apparatus is acting for there is no other means of communication with him; the current may be completed by the clock & go on regardless and yet the bell arrangement not be at work. If the hammer be heavy & the spring strong the battery may sink in power so as not to have sufficient pull upon the arm to be attracted & yet the working on shore continue so as to give no notion to the attendant. He must go to the rocks to wind up the clock (for the battery will not do that) and if the clock is not always to be going then he must also go to the rocks when the fog comes on to set it a going or if the clock be on shore which is not indicated in the description of the apparatus it may go on regularly & yet not be working the bell apparatus[.] Every fresh place for its application might suggest new questions and yet not one such place or even the idea generally is mentioned in the description that I saw. If such an arrangement is proposed to be made at any particular situation the apparatus should first be tested for the necessary force as described in my former letter; and if satisfactory in that respect its fitness for the peculiar place should then be tried experimentally, as to distance, isolation length of wires, &c upon the shore or elsewhere in a situation like as to noise & other circumstances[.]

I am | Sir | Your Very faithful humble Servant | M. Faraday
P.H. Berthon Esqr | Secretary | &c &c &c

1. Letter 3344.

2. Letter 3340.

Letter 3346**Faraday to Peter Henry Berthon****22 October 1857****From the original copy in GL MS 30108/2/77**

Royal Institution | 22 Oct 1857

Sir,

Perhaps a few words may be expected from me regarding the second nights observation of the four lights which took place at Hornchurch on the 20th instant. As the place is nearly 10 miles in a direct line from the Wharf, the lights ought to have appeared twice as bright as on the 13th when the distance was 15 miles¹. On the contrary they were far inferior because of the misty weather. I did not see them with the naked eye until the latter part of the time; and I learn by enquiry that the weather was so thick at the wharf at first that the persons there did not expect we should see them but it cleared up considerably afterwards.

The proportion between the 7 reflectors & the three reflectors appeared to me the same as on the 13th instant, and so did that between the great lens & the narrower Bishops light lenses. To the great lens had been added the fixed light zones above & below but they gave no signs of their presence during the revolution through their eight part of the circle either before or after the appearance of the lens ray.

There was this difference between the observations of this night (the 20th) and the 13th. The 7 reflectors distinctly & considerably surpassed the great lens in the brightness of the light. Also the three reflectors much surpassed the narrow lenses of the Bishops light, the latter indeed were at times hardly visible to the naked eye. This was to me unaccountable until upon close enquiry of Mr. Wilkins I learnt that on the first night the Fresnel lamp had burnt too fast & indeed in such a way as would soon have deranged it, whereas on the second night it was kept much lower & in what was considered the proper condition. On the first night they could not enter the revolving apparatus to lower the cottons because of the revolution. On the other hand, the reflector lamps were in much better order on the second night than on the first. They never burn so well on the first night of new cottons as on the second; the 13th. was the first night, & the 20th the second. I believe that these differences are sufficient to explain the differences between the two nights which we observed. I intended to go to the Trinity wharf and see that there was no particular window bar or other obstacle between the lenses & Hornchurch but the weather has been so heavy that there was no chance of seeing Hornchurch from the Wharf. I am assured that nothing was in the way².

I have the honor to be | Sir | Your Very humble Servant | M. Faraday
H.p. [sic] Berthon Esqr | &c &c &c

1. For which see letter 3343.

2. This letter was read to Trinity House By Board, 27 October 1857, GL MS 30010/41, p. 175.

Letter 3347**Faraday to Peter Henry Berthou****24 October 1857****From the original copy in GL MS 30108/2/77**

Royal Institution | 24 Oct 1857

Sir,

I am very sorry to trouble the Deputy master¹ & brethren with a further report but having been to the Wharf and carefully examined the situation of the apparatus employed on the 13th & 20th instant I find that my former reports² would convey an erroneous sense without some explanation. Capt'n. Poulter pointed out to me the position of Hornchurch which he saw at the time in the telescope & knew of his own knowledge. I found that the line from the lamp to it was not that which had been marked down by those who fitted up the apparatus but was more to the north forming an angle with it of $7\frac{1}{2}$ degrees. Hence even if the lights had not been dimmed by mist they could hardly have been seen when employed as fixed lights since the axis of the ray would have been $7\frac{1}{2}$ degrees to the south. This would have taken the great lens light altogether out of sight for its whole divergence is only between 6 & 7 degrees, & it would have taken the 7 lens light nearly out of sight since its whole divergence is 15° , the divergence between the axis or center & the edge of the beam being therefore only $7\frac{1}{2}$ degrees. Happily the revolutions brought all the lights into view at Hornchurch in succession & so we could observe & compare them.

But an error still remained which was serious & really interfered in the comparison of the lenses & reflectors. The reflectors were as I believe properly arranged so that the axis of their rays was horizontal. The place of observation was lifted above the horizontal axes I can hardly say how much but it was assumed in my former report³ for Brentwood as $9'$ and equal for all. The lenses ought to have been arranged so that their centers would be $\frac{3}{4}$ of an inch above the burner that the axis of the whole beams might be horizontal; in place of which I find them on a level with the burner, & as that is only $36\frac{5}{16}$ inches distant the consequence is that the axis of the ray descends forming an angle with the horizontal line of $1^\circ 14'$. This is serious with a beam whose divergence is 6 or 7 degrees only especially if to this $1^\circ 14'$ of depression be added the $9'$ due to elevations of the observer for at places have $1^\circ 23'$ out of the axis of a beam in which there is only $3\frac{1}{2}^\circ$. between the axis & the edge. It is evident therefore that the comparison of the great lens with the 7 reflectors especially as to intensity has not been just to the former & also that the narrower lenses have not been properly compared with the Buchaness three reflectors.

The window frames are only $\frac{3}{4}$ of an inch thick & would not interfere much. Nothing else but some low railing, affecting the bottom zones, was in the way⁴.

I am | Sir | Your Very Obedient Humble Servant | M. Faraday

1. John Shepherd.

2. Letters 3343 and 3346.

3. Letter 3343.

4. This letter was read to Trinity House By Board, 27 October 1857, GL MS 30010/41, pp. 175–6. It was ordered that further experiments should take place.

Letter 3348

Faraday to Christian Friedrich Schoenbein

October 1857¹

From the original in UB MS NS 434

My dear Scho<enbein>

I got as far as Stamford <Hill> [MS torn] <to>day (which I very rarely do) and can [MS torn] a better reminder of the fitness of writing to you. Poor as I may be in subject matter yet a daughter and your daughter is surely quite enough. She looked very well & I was very glad to see her so contented happy and cheerful. When she first came to this country I was very much frightened least the experiment should fail for that would have been no trifle but now all anxiety of that kind is over[.] It was impossible but that all her views of life society and manners should have been formed upon her experience & habits of home & her own country and I felt sure that much of what is good amongst us must have been hidden from her for a time by the novelty of the customs manners & occupation she would find here. But she is a girl of sense and I think was not long in passing through the show & form of things to the reality beneath. As far as I can see the reality has not been [MS torn] her and of [MS torn] <Eng>lishman feel proud & as [MS torn] very glad. However I dare say you know her mind in all these things far better than I can do. What I can see is that she seems happy in things as they are and growing in the estimation of those around her. Indeed there are many points in which I who am at a distance can see she is an example to all around her such as her judgment, her steadiness of purpose, her conscience of things, her toleration of the judgments of others, her truthfulness & her propriety & many others which make the basis of a good mental character. She appeared to be very well. I wish I could see more of her amongst us but the bad & uncertain state of my wife's health & her little strength is a great barrier to our desires.

I cannot just now remember what were the last points of philosophy which you sent me or even those of my own which are worth speaking of to you. I work very slowly now I want to proceed with action at a distance and from [MS torn] forgetting [MS torn] over I hope [MS torn] exertion – we shall see.

Commend me to Mrs. Schoenbein: even the poor talk I have given you about her daughter will incline her a little towards me. Say I hope, she will receive her back some day or another and find reason to be not less proud of her than she ever has been:– *even when she was a baby*[.]

Ever My dear Schoenbein | Very affectionately Yours | M. Faraday

1. Dated on the basis that this was written shortly after Emilie Schoenbein arrived in England and because of the further reference in Faraday's next letter to Schoenbein (letter 3360) about his work on action at a distance.

Letter 3349

Faraday to Margery Ann Reid

28 October 1857

From the original in the possession of Elizabeth M. Milton

[Royal Institution embossed letterhead] | 28 Oct 1857

My dear Maggie,

Thanks hearty thanks to you for the translation¹ & also to Mr. Deacon for his kindness. It is curious but it seems to me that few people can catch the idea which I am sure has real existence or foundation in nature[.] All as far as I can comprehend it which Brücke² says was known to all of us before but seems to me to be miles within my argument except in the few cases where he inadvertently uses expressions which include all that I have said and much more.

Patience[.] As I cannot change my view I bide my time[.]

Your Affectionate Uncle | M. Faraday

1. Of Brücke (1857). This was published in English as Brücke (1858), though with no identification as to the translator.

2. Ernst Wilhelm Brücke (1819–1892, DSB). Professor of Physiology at the University of Vienna, 1849–1890.

Letter 3350

Jules Antoine Regnauld¹ to Faraday

29 October 1857

From the original in IET MS SC 2

29 8bre 1857

Monsieur et Illustre Maître,

Mr. le Professeur Bussy² me rendant compte lors de Son retour à Paris de la Visite qu'il a eu l'honneur de Vous faire à London m'a engagé à

Vous adresser le mémoire cy joint³. Je n'avais pas osé Vous faire cet envoi jusqu'ici craignant que ce travail fût de trop petite importance pour être mis sous les yeux du physicien contemporain le plus riche en découvertes. Cependant comme ces recherches ont été faites avec beaucoup de soin, je serai heureux que Vous daigniez y jeter un regard. A défaut de cette originalité qui n'appartient qu'aux esprits d'Elite, Vous y trouverez, j'ose l'espérer, la Conscience et l'amour du Vrai que j'ai puisés dans la lecture bien souvent répétée de vos recherches expérimentales sur l'Electricité.

Excusez, Monsieur, la liberté que je prends de Vous occuper de moi un instant et Croire à la profonde admiration de Votre Serviteur.

Jules Regnauld

Professeur de Physique à l'Ecole de Pharmacie | de Paris

TRANSLATION

29 Oct 1857

Sir and illustrious Master,

Professor Bussy² who gave me an account, as soon as he returned to Paris, of the visit that he had honour of making to you in London, has encouraged me to send you the enclosed paper³. I had not dared to send this to you until now, fearing that this work was of too little importance to be placed under the gaze of the contemporary physicist most rich in discoveries. However, as this research was conducted with a lot of care, I would be happy if you deigned to glance at it. Lacking the originality that belongs only to the minds of the Elite, you will find there, I dare to hope, the Conscience and the love of Truth that I have drawn from most frequently re-reading your experimental researches on electricity.

Excuse, sir, the liberty that I take to draw your attention to me for an instant and believe the profound admiration of your servant,

Jules Regnauld

Professor of Physics at the School of Pharmacy | of Paris

1. Jules Antoine Regnauld (1820–1895, P3). Professor of Physics at the Ecole de Pharmacie.

2. Antoine Alexandre Brutus Bussy (1794–1882, DBF). Professor of Chemistry at the Ecole de Pharmacie.

3. Possibly Regnauld (1855).

Letter 3351

Faraday to Carlo Matteucci

30 October 1857

From an extract in Bianchi (1874), 220

Io lavoro, ma quando sono costretto a riposare, dimentico. Ma torno a lavorare. Fa del bene l'essere occupato, e ringrazio e prego la Provvidenza di lasciarmi in possesso del prezioso tesoro che è il lavoro

TRANSLATION

I work but when I am constrained to rest I forget. But I turn to work. It is good to be busy, and I thank and ask Providence to leave me in possession of the precious treasure that is work.

Letter 3352

Faraday to Harriet Elizabeth Cooke¹

4 November 1857

From the original in UU EW

[Royal Institution embossed letterhead], Albemarle St. W |
4 Nov 1857

My dear Miss Cooke,

Your kindness far exceeds what I could have hoped for & I am very grateful to you for it. I could not have thought you would have taken such interest or trouble. I hope your brother² will return home full of health & rejoicing in the results of his trip. But the idea of calling a journey to Venice a trip³[.] We really accept wonders with great coolness[.] I remember an old school copy *Familiarity breeds contempt* & am reproached by it. With grateful thanks

I am Your Very faithful Servant | M. Faraday

1. Harriet Elizabeth Cooke (d.1898, age 82, GRO). Sister of Edward William Cooke.

2. Edward William Cooke (1811–1880, ODNB). Painter.

3. See Munday (1996), 369–70 for Cooke's trip to Italy.

Letter 3353

Faraday to James Clerk Maxwell

7 November 1857

From the original in ULC Add MS 7655/II/11

[Royal Institution embossed letterhead], Albemarle St. W |
7 Nov 1857

My dear Sir,

I have just received and thank you heartily for your papers¹. I intended to send you copies of two of mine². I think I have sent them but do not find them ticked off. So I now send copies not because they are assumed as deserving your attention but as a mark of my respect & desire to thank you in the best way that I can[.]

Very Truly Yours | M. Faraday
Professor J.C. Maxwell BA | &c &c &c

1. Possibly Maxwell (1857a, b).

2. Faraday (1857c) and (1857a), Friday Evening Discourse of 27 February 1857. See letter 3354.

Letter 3354**James Clerk Maxwell to Faraday****9 November 1857****From the original in IET MS SC 2**

129 Union Street, Aberdeen | 9 Nov 1857

Dear Sir,

I have to acknowledge receipt of your papers on the Relations of Gold &c to Light and on the Conservation of Force¹. Last spring you were so kind as to send me a copy of the latter paper and to ask what I thought of it². That question silenced me at that time, but I have since heard and read various opinions on the subject which render it both easy and right for me to say what I think. And first I pass over some who have never understood the known doctrine of conservation of force and who suppose it to have something to do with the equality of action & reaction_[.] Now first I am sorry that we do not keep our words for distinct things more distinct and speak of the "Conservation of Work or of Energy" as applied to the relations between the amount of "vis viva" and of "tension" in the world; and the "Duality of Force" as referring to the equality of action and reaction.

Energy is the power a thing has of doing work arising either from its own motion or from the "tension" subsisting between it and other things.

Force is the tendency of a body to pass from one place to another and depends upon the amount of change of "tension" which that passage would produce.

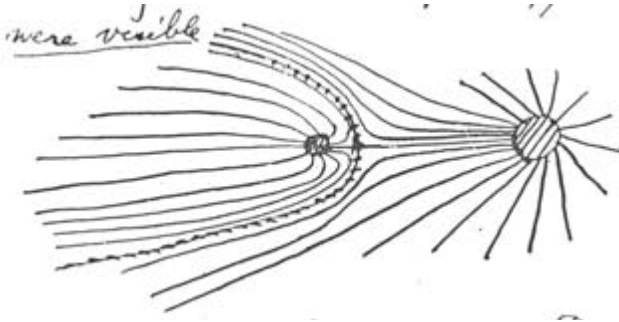
Now as far as I know you are the first person in whom the idea of bodies acting at a distance by throwing the surrounding medium into a state of constraint has arisen, as a principle to be actually believed in. We have had streams of hooks and eyes flying around magnets, and even pictures of them so beset, but nothing is clearer than your description of all sources of force keeping up a state of energy in all that surrounds them, which state by its increase or diminution measures the work done by any change in the system. You seem to see the lines of force curving round obstacles and driving plump at conductors and swerving towards certain directions in crystals, and carrying with them everywhere the same amount of attractive power spread wider or denser as the lines widen or contract.

You have also seen that the great mystery is, not how like bodies repel and unlike attract but how like bodies attract (by gravitation).

But if you can get over that difficulty, either by making gravity the residual of the two electricities or by simply admitting it, then your lines of force can "weave a web across the sky"³ and lead the stars in their courses without any necessarily immediate connection with the objects of their attraction.

The lines of Force from the Sun spread out from him and when they come near a planet *curve out from it* so that every planet diverts a number

depending on its mass from their course and substitutes a system of its own so as to become something like a comet, *if lines of force were visible*



The lines of the planet are separated from those of the Sun by the dotted line. Now conceive every one of these lines (which never interfere but proceed from sun & planet to infinity) to have a *pushing* force, instead of a *pulling* one and then sun and planet will be pushed together with a force which comes out as it ought proportional to the product of the masses & the inverse square of the distance.

The difference between this case and that of the dipolar forces is, that instead of each body catching the lines of force from the rest all the lines keep as clear of other bodies as they can and go off to the infinite sphere against which I have supposed them to push.

Here then we have conservation of energy (actual & potential) as every student of dynamics learns, and besides this we have conservation of "lines of force" as to their *number* and total strength for *every* body always sends out a number proportional to its own mass, and the pushing effect of each is the same.

All that is altered when bodies approach is the *direction* in which these lines push. When the bodies are distant the distribution of the lines near each is little disturbed. When they approach, the lines march round from between them, and come to push behind each so that their resultant action is to bring the bodies together with a *resultant* force increasing as they approach.

Now the mode of looking at Nature which belongs to those who can see the lines of force deals very little with "resultant forces" but with a network of lines of action of which these are the final results, so that I for my part cannot realise your dissatisfaction with the law of gravitation provided you conceive it according to your own principles. It may seem very different when stated by the believers in "forces at a distance", but there can be only differences in form and conception not in quantity or mechanical effect between them and those who trace force by its lines. But when we face the great questions about

gravitation Does it require time? Is it polar to the “outside of the universe” or to anything? Has it any reference to electricity? Or does it stand on the very foundation of matter – mass or inertia? then we feel the need of tests, whether they be comets or nebulae or laboratory experiments or bold questions as to the truth of received opinions.

I have now merely tried to show you why I do not think gravitation a dangerous subject to apply your methods to, and that it may be possible to throw light on it also by the *embodiment* of the same ideas which are expressed *mathematically* in the functions of Laplace⁴ and of Sir W.R. Hamilton⁵ in Planetary Theory.

But there are questions relating to the connexion between magneto-electricity and certain mechanical effects which seem to me opening up quite a new road to the establishment of principles in electricity and a possible confirmation of the physical nature of magnetic lines of force. Professor W. Thomson seems to have some new lights on this subject⁶[.]

Yours sincerely | James Clerk Maxwell
Professor Faraday | &c &c

1. Faraday (1857c) and (1857a), Friday Evening Discourse of 27 February 1857. Both sent with letter 3353.

2. Letter 3260.

3. A slight misquotation of Alfred Tennyson, *In Memoriam*, III, 6.

4. Pierre-Simon, Marquis de Laplace (1749–1827, DSB). French physicist. See Laplace (1799–1825), 1: 136–7.

5. William Rowan Hamilton (1805–1865, ODNB). Mathematician and Astronomer Royal for Ireland. See Hamilton, W.R. (1834, 1835).

6. Thomson (1856b).

Letter 3355

John Murray to Faraday

9 November 1857¹

From the original in RI MS Conybeare Album, f. 48

Albemarle St. | 9 Nov

My Dear Sir,

Will you permit me to introduce to you Mr. Muirhead² the Editor of the Works of James Watt³ – and himself of Watts kin[.] He is anxious to ascertain your willingness to allow the publication of a certain letter & I hope you will kindly listen to his request at the solicitation of

Your faithful & obedient servant | John Murray
Professor Faraday – | &c &c &c

1. Dated on the basis that this refers to a request for Faraday's permission to publish Faraday to Muirhead, 23 December 1846, letter 1939, volume 3, in Muirhead (1858), 384.

2. James Patrick Muirhead (1813–1898, ODNB). Biographer of James Watt.

3. James Watt (1736–1819, ODNB). Chemist and engineer.

Letter 3356**Faraday to Peter Henry Berthon****11 November 1857****From the original copy in GL MS 30108/2/77**

Royal Institution | 11 Nov 1857

Sir,

The errors of arrangement referred to in my letter of the 24th Ultm¹ have been corrected. I examined the lighthouse at the Trinity Wharf on Monday² & yesterday Evening the Deputy Master³ & Elder brethren again proceeded to observe the results from Hornchurch. The night was very good; dark but far clearer than before so that all the lights were well seen. According to my own impression, the 7 reflectors in one face had the pre-eminence[.] That light differed from the great lens in having a more disc like character & longer duration for the same velocity of revolution. The great lens was more star light but inferior in power[.] The Buchaness or three reflectors in face had I think the due or expected relation to the seven reflectors, possessing as well as I could judge nearly one half of their power[.] The Bishops light lenses rose considerably in value by the adjustment of the lamp – They now surpassed the Buchaness in power though their duration was not so long for the same velocity of revolution. They recur much quicker indeed as three to once⁴[.]

I have the honor to be | sir | Your Very Obedient faithful Servant |
 M. Faraday
 H.p. [sic] Berthon Esqr | &c &c &c

1. Letter 3347.

2. That is 9 November 1857.

3. John Shepherd.

4. This letter was read to Trinity House By Board, 17 November 1857, GL MS 30010/41, pp. 196–7. It was agreed to consider the matter further. At the next meeting (Trinity House By Board, 24 November 1857, GL MS 30010/41, p. 212) it was agreed that the light at Bishop Rock should be a fixed catadioptric one.

Letter 3357**Faraday to James Clerk Maxwell****13 November 1857****From the original in ULC Add MS 7655/II/14**

[Royal Institution embossed letterhead],
 Albemarle St. London W | 13 Nov 1857

My dear Sir,

If, on a former occasion, I seemed to ask you what you thought of my paper¹, it was very wrong; for I do not think anyone should be called upon for the expression of their thoughts before they are prepared, and wish to give them. I have often enough to decline giving an opinion because my mind is not ready to come to a conclusion, or does not wish to be committed to a view,

that may by further consideration be changed. But having received your last letter, I am exceedingly grateful to you for it; and rejoice that my forgetfulness of having sent the former paper on conservation, has brought about such a result. Your letter is to me the first intercommunication on the subject with one of your mode & habit of thinking. It will do me much good; and I shall read and meditate on it again & again.

I dare say I have myself greatly to blame for the vague use of expressive words. I perceive that I do not use the word "force" as you define it, "the tendency of a body to pass from one place to another" [.] What I mean by the word is the *source* or *sources* of all possible actions of the particles or materials of the universe these being often called the *powers* of nature, when spoken of in respect of the different manners in which their effects are shown. In a paper which I have received at this moment from the Phil. Mag.² by Dr. Woods³ they are called the "*forces*, such as electricity heat &c". In this way I have used the word "force" in the description of gravity which I have given as that expressing the received idea of its nature & source; and such of my remarks as express an opinion or are critical, apply only to that sense of it. You may remember I speak to labourers like myself experimentalists on force generally, who receive that description of gravity as a physical truth and believe that it expresses all, and no more than all, that concerns the nature and locality of the power. To these it limits the formation of their ideas and the direction of their exertions, and to them I have endeavoured to speak, showing how such a thought, if accepted, pledged them to a very limited and, probably, erroneous view of the cause of the force, and to ask them to consider, whether they should not look (for a time at least) to a source in part external to the particles. I send you two or three old printed lines *marked* relating to this point. To those who *disown* the definition or description as imperfect, I have nothing to urge; as there is then probably no real difference between us.

I hang on to your words because they are to me weighty; and where you say "I, for my part cannot realise your dissatisfaction with the law of gravitation provided you conceive it according to your own principles" they give me great comfort. I have nothing to say against the law of action of gravity. It is against the law which measures its total strength as an inherent force that I venture to oppose my opinion; and I must have expressed myself badly (though I do not find the weak point) or I should not have conveyed any other impression. All I wanted to do was to move men (not No.I, but No.II) from the unreserved acceptance of a principle of physical action which might be opposed to natural truth. The idea that we may possibly have to connect *repulsion* with the lines of gravitation force (which is going far beyond anything my mind would venture on at present except in private cogitation) shows how far we *may* have to depart from the view I oppose.

There is one thing I would be glad to ask you. When a mathematician engaged in investigating physical actions and results has arrived at his own conclusions, may they not be expressed in common language as fully, clearly,

and definitely as in mathematical formulae? If so would it not be a great boon to such as I to express them so?— translating them out of their hieroglyphics, that we also might work upon them by experiment. I think it must be so, because I have always found that you could convey to me a perfectly clear idea of your conclusions which, though they may give me no full understanding of the steps of your process, give me the results neither above nor below the truth;— and so clear in character that I can think and work from them. If this be possible would it not be a good thing if mathematicians, writing on these subjects, were to give us their results in this popular, useful, working state, as well as in that which is their own and proper to them[.]

Ever My dear Sir | Most truly Yours | M. Faraday
Professor | J.C. Maxwell Esqr | &c &c &c

1. Faraday (1857a), Friday Evening Discourse of 27 February 1857. See letter 3354 referring to letter 3260.

2. Woods (1857).

3. Thomas Woods (d.1905, age 89, *Med. Dir.*, 1906, p. 1815). Physician in Parsonstown.

Letter 3358

Faraday to Edwin Lankester¹

17 November 1857

From the original in the possession of George W. Platzman

[Royal Institution embossed letterhead] | 17 Nov 1857

My dear Sir,

I have not left your letter unattended to. I do not find that any results will occur *before* Easter². I cannot tell for the time after³. The F.E⁴ also I have mentioned to Mr. Barlow & the Committee[.] I am told the Evenings are all full prospectively. I shall keep & use the power you trusted to me as we agreed[.]

Ever Truly Yours | M. Faraday
Dr. Lancaster [sic] | &c &c &c

1. Edwin Lankester (1814–1874, ODNB). Physician and naturalist.

2. That is 4 April 1858.

3. Lankester delivered a course of eight lectures on the vegetable kingdom after Easter 1858. RI MS RI Le4/212.

4. Lankester (1858), Friday Evening Discourse of 5 February 1858.

Letter 3359

Charles Lock Eastlake to Faraday

23 November 1857

From the original in IET MS SC 2

7 Fitzroy Square. W | 23 Nov 1857

My dear Sir,

Can you give me the address of an operative chemist whom you can recommend. I am desired by Mr. Cope¹, who is employed on some frescoes

in the Palace of Westminster, to have some grey sand analysed. The lime on which frescoes are executed is commonly mixed with river sand of a lighter colour. The reason why Mr. Cope wishes to have this grey sand analysed is that in some experiments with it the plasterer shewed that the trowel was blackened. It is also desirable to have a piece of old plaster (of an early Italian fresco) examined so as to detect the nature & quality of the sand used.

As these analyses may require some care I shall be obliged by your giving me the address of a fit operator to whom you will perhaps allow me to mention your name.

I remain, my dear Sir | Yours faithfully | C.L. Eastlake
Professor Faraday

1. Charles West Cope (1811–1890, ODNB). Historical painter.

Letter 3360

Faraday to Christian Friedrich Schoenbein

24 November 1857

From the original in UB MS NS 433

Royal Institution | 24 Nov 1857

My dear Schoenbein,

I dare say you have plenty of letters with the London post mark now from Stamford Hill, and hardly require to have your English associations stirred up by one from me so soon after the last¹;— but we leave town on Friday² for a little renovation, & I wanted to relieve myself by writing to you before we go. I expected you would have seen much of your last in the Philosophical Magazine before now, but Dr. Francis told me, a day or two back, that he was waiting for some new type –O and +O; for that nothing they have at present will serve the same purpose:— then I trust it will appear as it ought to do³... What a wonderful thing oxygen is and so I suppose would every other element appear if our knowledge were more perfect.

Sir James Clarke applied to me the other day, to know if you had been able to draw up a set of practical directions for the observation of Ozone in the atmosphere; obviating such difficulties as arise at first, connected with the time of exposure,— the continued exposure, the moisture or dryness of the test paper, &c. He seemed well aware of the general state of the subject, but thought that you would know sooner than the world at large, of any perfectionment. His object is to consider the medical effects of Ozone in nature; where without doubt it must have some, & perhaps, very important effects. Probably when you feel that there is any improvement in the mode of observing you will let us know. I think he said that Ozone seemed to be abundant about our Queens residence; Balmoral, in the North.

I ventured to send you a paper the other day by the post. I was assured it would go free & shall be very sorry if, unaware, I have put you & other

friends to post expence: but I find that the information I obtain by enquiry is often very uncertain in its nature, though positive in its form. The paper was about Gold & the relation of it & other metals to light⁴. Many facts came out during the enquiry which surprized me greatly; especially the effects of pressure & also those relating to polarized light. Lately I have been working on the relation of *time* to actions at a distance;— as those actions which class as magnetic but the subject is very difficult,— the requisite apparatus requires to be frequently remade, each time being more perfected; & whether I shall catch the (1/200000) part of a second (if required) seems very doubtful. In the mean time I am for the present tired & must lay the research on the shelf.

Since I wrote to you we have had Miss Schoenbein here:— but since that I have not been able to see her or my old friend Miss Hornblower, either, at Stamford Hill: Probably when the Christmas Holidays come on, we shall have the opportunity; but my wifes health is so infirm and our capabilities so limited at the Institution, that I dare not think of what we should like, before the time comes: I have undertaken to give half a dozen juvenile lectures after Christmas⁵;— whether they will come off (as we say) or not is doubtful. Patience—

I hope that Madame Schoenbein is cheered by her daughters letters. Miss Schoenbein assured me that there was a great deal of correspondence going on, & from the manner in which I heard of it, I should trust that it was cheerful. Remember me in the kindest manner to the anxious mother.

I do not think we have much scientific news; at least I do not hear of much; but then I do not go within reach of the waves of sound; & so must consent to be ignorant. Indeed too much would drive me crazy in the attempt to hold it_[.]

Ever My dear Schoenbein | Yours Most truly | M. Faraday

1. Letter 3348.

2. That is 27 November 1857.

3. Schoenbein (1858a), that is the scientific part of letter 3335.

4. Faraday (1857c).

5. The notes for Faraday's Christmas lectures on 'Static Electricity' are in RI MS F4 J18.

Letter 3361

Faraday to Thomas Twining

2 December 1857

From a photocopy in RI MS

Brighton | 2 Dec 1857

My dear Sir,

I received yours here and though I could not be with you hope that you had a good & wise result last night— I had read the matter contained in the Journal of the Soc Arts¹ & was very sorry for the condition of the subject_[.] It is no doubt a difficult one & I dare say that the principle of examination can hardly be carried out by a central body for the whole country_[.] Still to change the Society suddenly from that which it has been to something to something

[sic] as yet insufficiently tried by its consequences would as it appears to me not be wise. But as I have entered very little into the subject I have no right to form any opinion[.] I trust that a good safe & useful conclusion will be arrived at²[.]

With our kindest remembrances to Mrs. Twining³ I am

My dear Sir | Most truly Yours | M. Faraday

T. Twining Jur Esqr | &c &c &c

1. *J. Soc. Arts.*, 1857, 6: 5–8 which discussed the state of the Society of Arts examinations.
2. This refers to the decision of the Society of Arts to continue examinations which was taken amidst some controversy at a special meeting of members on 1 December 1857. *J. Soc. Arts.*, 1857, 6: 31–9.
3. Victorine Twining, née von Hagen (1830–1889, Pearce (1988), 2). Married Thomas Twining in 1850.

Letter 3362

William Arthur Slade¹ to Faraday

4 December 1857

From the original in IET MS SC 2

Upper Easton, St Philips, Bristol | 4 Dec 1857

M. Faraday, Esq F.R.S. | &c &c &c | London

Dear Sir,

Your note came to hand in due course, and I beg to thank you most sincerely for the trouble you have taken. Perhaps you will oblige me by glancing over the annexed very rough sketch of a Magneto Electric Machine: I fear that my anxiety not to trespass too much on your time has, without attaining its object, prevented my giving a very clear description of its construction. Allow me to add that, in case there is any defect in the machine, I feel sure I could remove it if I only knew *where* it existed.

Your obedient servant | William Arthur Slade

AAAA are fixed pivots.

BA is a beam, to be connected by a crank with the flywheel of the engine.

BC & bc are systems of bases producing a parallel motion (as in the steam engine)

DE & de are bases to which are appended F & f pieces of iron to be attracted by GH & gh when GH & gh (alternately) become magnetic.

J & j are fixed supports through which DE & de move.

KLMNOPQ & klmnopq represent wires, coiled round the pieces of iron GH & gh, and brought to KQR & q where they terminate in small discs. These lines as well as those representing the course of the wires which bring the Electric current from the battery are in red

Rr is a fixed block on the outside of which the four discs terminating the coils of wire appear.

ST is a block carrying on its surface (in such a position as to come {alternately} in conjunction with the discs terminating each coil) the extremities of two

wires coming respectively from the poles of a battery ST is pressed against Rr by the table

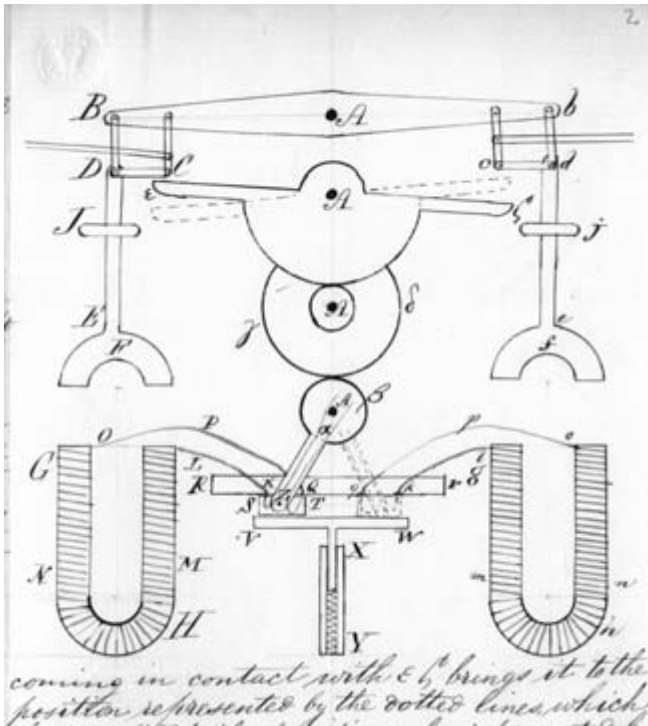
VW which is supported by a spring within

XY a tube in which the bar supporting WX moves freely. The block ST is moved from its present position to that represented by the dotted lines, by the motion of the bar

Z α , which is fixed to the cogged wheel

$\alpha\beta$, connected by the cogged double wheel $\gamma\delta$ with the semicircle fixed to the bar $\varepsilon\zeta$.

When in the position represented G.H. is magnetic and, attracting F, draws down the bar DE, thereby giving motion to the machine, till C coming in contact with $\varepsilon\zeta$ brings it to the position represented by the dotted lines which brings ST to the position also represented by dotted lines; this deprives GH of its magnetic properties conferring them on gh which in its turn draws down the bar ef till it deprives itself of its magnetic properties, by bringing c into contact with $\zeta\varepsilon$, conferring them again on GH: thus by GH & gh becoming alternately magnetic BAb is given the same motion as the beam of a steam engine.



1. William Arthur Slade (d.1900, age 61, GRO). Given as an accountant in 1861 census. TNA RG9/1734, f. 76, p. 1.

Letter 3363

Faraday to John Tyndall

9 December 1857

From the typescript in RI MS JT TS Volume 12, p. 4144

Brighton | 9 Dec 1857

My dear Tyndall,

I cannot resist the pleasure of saying I have very much enjoyed your paper¹. Every part has given me delight. It goes on from point to point beautifully. You will find many pencil marks, for I made them as I read. I let them stand, for though many of them receive their answer as the story proceeds, yet they shew how the wording impresses a mind fresh to the subject, and perhaps here and there you may like to alter it slightly, if you wish the full idea, i.e. not an inaccurate one, to be suggested at first; and yet after all I believe it is not your phrase, but the natural jumping to a conclusion, that affects, or has affected, my pencil.

We return on Friday², when I will return you the paper.

Ever truly yours | M. Faraday

1. Tyndall (1858). Received by and read to the Royal Society on 17 December 1857.

2. That is 11 December 1857.

Letter 3364

Faraday to Margery Ann Reid

12 December 1857

From the original in the possession of Elizabeth M. Milton

Royal Institution | 12 Dec 1857

My dear Maggie,

I have just learned from your aunt¹ that you have not had my letter i.e. the one I wrote immediately on the receipt of your translation² – You really must have thought me extra forgetful. Conceive the thanks I sent you for your kindness & those to Mr. Deacon & give to him those that are not for yourself[.] My letter must be lying somewhere in a post office but I cannot in any way call to mind any circumstance that will account for its misadventure[.] Now knowing your interest in me & mine & therefore in the Gravitation matter I will not try to make amends by repeating what I suppose I said to you but I will send you a letter or two that has passed betwixt myself & Mr. Maxwell the Mathematical professor at Aberdeen³:- he who wrote a mathematical paper on “Faraday’s lines of force”⁴ & proved their mathematical correctness.

I think they will interest you – take care of them & return them to me at your convenience[.] I am almost afraid now to trust the post with them.

We returned from Brighton Yesterday – in our usual health – left Jenny there & by this time I suppose she will have received her father⁵ mother⁶ & sister in our place. Mr. Boosey⁷ was so to s[t]ay with us there but I expect has returned home to day[.] Mr. Whitelaw⁸ has been very unwell this week – not out last Sabbath & Mr. Vincent has been ill but is better.

As for your coming home I wish it but do not expect it. There is a sort of lingering doubtful vis inertia fatality about Newcastle when you go there which makes one ask your intentions only to doubt the results[.] Well wherever you are my dear Girl may you be happy & at peace in that which concerns you most. My love to Your father⁹ – Caroline – & many other – to Elizabeth¹⁰, Mr. Deacon – Mr. Reid¹¹ – Mr. Paradise kind old man often on ones thoughts[.]

Your Very Affectionate Uncle | M. Faraday
Miss Reid

1. Sarah Faraday.
2. That is letter 3349.
3. Probably letters 3354 and 3357. Faraday kept a copy of the latter which is now in IET MS SC 3.
4. Maxwell (1856).
5. John Barnard.
6. Margaret Barnard.
7. Thomas Boosey (1795–1871, B1). Publisher and an Elder of the London Sandemanian Church, 1843–1860.
8. George Whitelaw (d.1872, age 68, GRO). Publisher's manager and an Elder of the London Sandemanian Church. Cantor (1991), 302.
9. William Ker Reid.
10. Elizabeth Reid (1830–1902, Reid, C.L. (1914), GRO). A niece of Sarah Faraday.
11. Unidentified.

Letter 3365

William Bowman to Faraday

15 December 1857

From the original in RI MS Conybeare Album, f. 31

5 Clifford St. | 15 Dec 1857

My dear Sir,

Thank you very much indeed for so kindly thinking of me. My little folks shall most certainly take advantage of your invitation¹.

Yours very sincerely | W. Bowman
M. Faraday Esq

1. For Faraday's Christmas lectures on 'Static Electricity'. His notes are in RI MS F4 J18.

Letter 3366**Christian Gottfried Nees von Esenbeck¹ to Faraday****15 December 1857****From the printed original in RS MS 241, f. 151**

Breslau, den | 15 Dec 1857

Sr. Hochwohlgeboren,

Herrn Profesor Dr. Michael Faraday, Esq Hon Membr. R.S. Ed., F.R.S.
 Lond, Cambr. Phil and Med. Chirurg. Soc. F.G.S., Ord. Boruss., Pour le Mérite
 Equ et Com. Leg. Honor., Inst. Imp, Par. Soc. etc.

Ew. Hochwohlgeboren | zu | London.

übersende ich hierbei das Diplom, durch welches die Kaiserliche
 Leopoldinisch-Carolinische Akademie der Naturforscher, in Anerkennung
 Ihrer Verdienste um die Wissenschaften, Sie zur Theilnahme an ihren Arbeiten
 und Bestrebungen auffordert.

Zugleich liegt mir ob, Ew. Hochwohlgeboren, insbesondere auf den
 Inhalt des dem Diplom beigefügten Blattes in deutscher Sprache aufmerksam
 zu machen, welches, ausser den Modalitäten der Mitarbeit an den Verhand-
 lungen des Instituts, den Mitgliedern auch noch den Ankauf der Nova Acta
 wenigstens von der Zeit ihres Eintritts an, angelegentlichst empfiehlt.

Wir verweisen in dieser Hinsicht ferner auf das, der Vorrede zum
 zweiten Supplement des 19. Bandes einverleibte Schreiben an die Mitglieder
 der Akademie vom 3. August 1840².

Der Präsident der Akademie | Dr. Herr Esenbeck

TRANSLATION

Breslau | 15 Dec 1857

Very honourable Sir,

Herrn Professor Dr. Michael Faraday, Esq Hon Membr. R.S. Ed., F.R.S.
 Lond, Cambr. Phil and Med. Chirurg. Soc. F.G.S., Ord. Boruss., Pour le Mérite
 Equ et Com. Leg. Honor., Inst. Imp, Par. Soc. etc.

Your honourable Sir | in | London

With this letter I send the diploma by which the Kaiserliche
 Leopoldinisch-Carolinische Akademie der Naturforscher acknowledges your
 work in science.

I am obliged to call your honourable attention to the contents of the
 paper, enclosed with the diploma, written in the German language. Except
 for the modalities relating to collaboration in meetings of the Academie, this
 paper very strongly suggests, that members acquire the Nova Acta starting
 at least with their date of admission.

In addition we point in this regard to the letter of 3 August 1840 to mem-
 bers of the Academie, that is included in the second supplement to volume
 nineteen².

The president of the academy | Dr. Herr Esenbeck

1. Christian Gottfried Nees von Esenbeck (1776–1858, DSB). Botanist and President of the Academia Caesarea Leopoldina-Carolina Naturae Curiosorum, 1818–1858.
2. *Nova Acta Academia Caesarea Leopoldina-Carolina Naturae Curiosorum*, 1841, 19 (2nd supp.): xi–xiv.

Letter 3367

Faraday to Christopher Hansteen

16 December 1857

From the original in RI MS F1 E5

Royal Institution | 16 Dec 1857

Dear & honored Sir,

General Sabine has placed in my hands a letter from you, and I make it, I hope you will not think improperly, an occasion for writing to you, for the purpose of expressing my sincere admiration and respect; hoping that, as one who has been admitted into the circle of scientific men, I might be permitted to offer a word of testimony & praise to one who is a leader in that glorious band. Let me suppose that I have not been presumptuous in thus writing to you and in any case think as kindly of me as you can.

I thought also that I could better answer your enquiries in relation to Mr. Arndtsen¹ directly than through General Sabine. I am sorry that I cannot advance the wishes of that gentleman and yourself; but I must explain to you how it is. In the first place our Institution is not like your Universities. It is a private establishment: the Government does nothing for us, and we have no opportunities of receiving students. We have but one assistant and he is an ordinary workman. I formerly gave lectures to which the public were admitted by payment; but do not deliver any now, except six, once a year, at Christmas to the juvenile connexions of our members & subscribers. So you see we have no place for such an one as Mr. Arndtsen.

For the same reason I have never had any student or pupil under me to aid me with assistance; but have always prepared and made my experiments with my own hands, working & thinking at the same time. I do not think I could work in company, or think aloud, or explain my thoughts at the same time. Sometimes I and my assistant have been in the Laboratory for hours & days together, he preparing some lecture apparatus or cleaning up, & scarcely a word has passed between us:— all this being the consequence of the *solitary & isolated* system of investigation; in contra distinction to that pursued by a Professor with his aids & pupils as in your Universities.

But besides all that, my dear Sir, I now work but little and at fitful moments; as my health will bear it. I am obliged to keep out of Society;— to avoid Societies;— and above all to forbear making new connexions and claims on my thoughts: for I find it quite difficult enough to perform my duty to the few that I do keep open, as you may suppose.

Nevertheless I esteem Mr. Arndtsen's desire as a very great compliment, and thank him & you for it. Will you say so much to him for me.

And now I must conclude, hoping you will believe me in truth & sincerely.

Your great & respectful admirer | M. Faraday
Professor C. Hansteen | &c &c &c

Address: Professor Chr. Hansteen | &c &c &c | Observatory | Christiana
| Norway

1. Adam Frederik Oluf Arndtsen (1829–1919, NBL). Norwegian physicist.

Letter 3368

Arthur-Auguste De La Rive to Faraday

19 December 1857

From the original in IET MS SC 2

Genève le | 19 Xbre 1857

Mon tres cher & excellent ami,

Je ne puis vous dire avec quel plaisir j'ai reçu votre bonne lettre du 30 Octobre¹. Parmi les jouissances que je dois à la culture de la Science; l'une des plus grandes, je vous assure, est de m'avoir procuré votre amitié, héritage précieux que j'ai reçu d'un père² qui serait heureux de penser que je vous ai pour ami. Je jouis des bonnes nouvelles que vous me donnez de vous même; vos 66 ans m'ont étonné; vous êtes si jeune d'esprit, de corps & surtout de coeur, qu'on ne peut croire que vous avez passé la soixantaine.– Mais vous avez su arranger votre vie de manière à ne pas la dépenser dans ce qui use l'âme & le corps, & par dessus tout vous avez eu le bonheur de la conformer aux sentiments chrétiens qui seuls sont un guide infaillible.

Quant à moi, si j'ai eu une vie plus agitée sous bien des rapports, plus coupée & surtout tres moins bien employée que la votre, je dois cependant bénir la Providence de tous les biens qu'elle m'a accordés. J'ai eu de grands malheurs, de grands désappointements, de grands soucis, mais j'entrevois une vieillesse calme & douce, sereine plutôt qu'heureuse, car à mon âge le bonheur complet n'est pas possible. Au reste un trop grand bonheur ne vous permettrait pas de vous détacher graduellement de cette vie pour tourner ses regards & ses espérances plus haut, & j'en suis venu à ce moment où il faut savoir mettre un intervalle entre la vie & la mort, en cherchant à se mettre à l'abri des agitations & des tracas du monde.

Enfin j'ai achevé mon ouvrage sur l'Electricité; vous allez recevoir ce 3ème & dernier volume³, & si vous daignez y jeter les yeux, vous me comprendrez quand je vous dirai qu'à lui seul il m'a coûté plus de travail & de peine que les deux autres réunis. L'électricité animale⁴ surtout m'a occasionné plus d'une année de travail perseverant. Je suis impatient de savoir ce

que vous penserez de ma théorie sur l'origine de l'Electricité atmosphérique⁵ & sur celle du Magnétisme terrestre⁶. J'ai éprouvé une grande jouissance à l'occasion de ce dernier volume dans le concours utile que m'a prêté mon fils cadet⁷; vous verrez qu'il ne s'est pas trop mal tiré de ce qu'il a fait dans ce volume⁸. Il vient de faire pour la *Bibl. Univ.* un extrait du mémoire⁹ que vous m'avez envoyé & dont je vous remercie infiniment; ce mémoire nous a vivement intéressés & mon fils a désiré en faire l'analyse. Il a commencé & poursuit un grand travail sur l'induction, sujet qui lui paraît l'un des plus curieux de la physique; j'espère qu'il parviendra à obtenir quelques résultats importants. Il prend souvent dans ma bibliothèque les volumes que vous m'avez donnés & qui renferment la collection de vos mémoires.

Quant à moi, je jouis de revenir au laboratoire & je suis certain de suivre à des recherches que m'a suggéré le travail que j'ai été obligé de faire pour composer mes trois gros volumes. Que de choses encore à faire dans ce champ inépuisable. Dès que j'aurai achevé une partie de ces recherches, j'aurai soin de vous faire part des résultats que j'aurai obtenus, si du moins ils le méritent.

Mon fils aîné¹⁰ est devenu depuis cette année le Rédacteur en chef de la *Bibliothèque Universelle*; il a trouvé là une occupation très intéressante & en même temps assez assujétissante, ce qui est assez précieux dans le temps où nous vivons. C'est surtout la partie littéraire & des Sciences morales qui est son domaine.

Voilà bien des détails intimes; mais vous me les avez demandés & je les dois à votre amitié; permettez moi d'ajouter encore que toute ma famille est bien portante, grâce à Dieu, & que la Providence m'a accordé la faveur d'avoir une compagne qui est une amie dont tous les jours j'apprécie davantage la valeur. Elle me charge de la rappeler à votre bon souvenir ainsi qu'à celui de Madame Faraday à qui je vous prie de présenter mes compliments les plus affectueux.

Votre tout dévoué & affé | Aug. de la Rive

Serait-ce indécent de vous prier de faire parvenir la lettre ci inclus à notre ami Wm Henry¹¹ dont j'ignore l'adresse

Je ne vous ai rien dit de notre séjour de sept mois en Italie qui a très bien réussi & où j'ai éprouvé des jouissances dont je ne me croyais plus susceptible.— J'ai eu un bien grand plaisir à voir un instant cet été Mr. Tyndall à qui je compte écrire incessamment; faites lui en attendant mes meilleurs compliments.

TRANSLATION

Geneva | 19 Dec 1857

My very dear and excellent friend,

I cannot tell you with what pleasure I received your good letter of 30 October¹. Among the joys that I owe to the cultivation of Science, one

of the greatest, I assure you, is for me to have acquired your friendship, a precious inheritance that I received from my father², who would be happy to think that I had you for a friend. I rejoice in the good news that you give me of yourself; your sixty six years astonished me; you are so young in spirit, in body and especially at heart, that it is difficult to believe that you are over sixty. – But you knew how to arrange your life in such a way so as not to expend it on that which uses the soul and the body and above all you have had the good fortune to model it on Christian sentiments, the only infallible guide.

As for me, if I have had a more agitated life in many respects, more cut & above all much less well employed than yours, I must however bless Providence for all the goodness that she has accorded to me. I have had great misfortunes, great disappointments and great worries, but I foresee a quiet and gentle old age, serene rather than happy, because at my age complete happiness is not possible. In any case, too much happiness would not allow you to detach yourself gradually from this life to turn your sights and your hopes higher, & I have come to the moment where one must be able to put an interval between life and death, seeking refuge from the agitations and worries of the world.

I have finally completed my work on electricity; you will receive this third & final volume³, & if you deign to glance at it, you will understand me when I tell you that it gave me more work and trouble than the two other volumes put together. Animal electricity⁴ particularly involved more than a year of painstaking work. I am impatient to know what you will think of my theory on the origin of atmospheric electricity⁵ and on terrestrial magnetism⁶. I felt great joy, in compiling this last volume, to have the useful assistance of my youngest son⁷; you will see that he did not get on too badly in what he did in this volume⁸. He has just compiled an excerpt for the *Bibliothèque Universelle* from the paper⁹ that you sent me and for which you have my infinite thanks; this paper interested us keenly and my son wanted to analyse it. He has begun and pursues a great work on induction, a subject that he feels is one of the most curious in physics; I hope that he will succeed in obtaining some important results. He often takes from my library the volumes that you have given me and which contain your collected works.

As for me, I rejoice in returning to the laboratory & I am certain to follow the research prompted by the work that I was obliged to undertake in order to write my three thick volumes. So many things still left to do in this inexhaustible field. As soon as I have completed part of this research, I shall make sure that I send you the results obtained, that is if they merit it.

My eldest son¹⁰ this year became the editor-in-chief of the *Bibliothèque Universelle*; he has found it a most interesting and at the same time exacting occupation, which is important in the times in which we live. The literary part and moral sciences, above all, are his domain.

What a lot of intimate details; but you did ask me for them and I owe them to your friendship; permit me to add also that all my family are well, thanks be to God, & that Providence has granted me the favour of having a companion who is a friend whose value I appreciate more every day. She asks to be remembered to you and also to Mrs. Faraday, to whom I ask you to present my most affectionate compliments.

Your all devoted & affectionate | Aug. de la Rive

Would it be indecent to ask you to forward the enclosed letter to our friend William Henry¹¹ whose address I do not know.

I said nothing of our seven month stay in Italy which was very successful & where I experienced joys of which no longer believed myself capable. – I had great pleasure in seeing once this summer Mr. Tyndall, to whom I intend to write shortly; please convey to him in the meanwhile my best compliments –

1. Not found.
2. Charles-Gaspard De La Rive (1770–1834, DSB). Swiss chemist.
3. De La Rive (1854–8), 3.
4. *Ibid.*, 1–88.
5. *Ibid.*, 89–199.
6. *Ibid.*, 200–307.
7. Charles-Lucien De La Rive (1834–1924, Choisy (1947), 51). Swiss physician and writer.
8. Acknowledged in De La Rive (1854–8), 3: vi.
9. Faraday (1857c) translated and excerpted as Faraday (1858a).
10. William De La Rive (1827–1900, Choisy (1947), 51). Swiss politician and writer.
11. William Charles Henry (1804–1892, ODNB). Physician and chemist.

Letter 3369

Lodovico Menin¹ and Giacinto Namias² to Faraday

21 December 1857

From the original in RS MS 241, f. 154

I.R. Istituto, di scienze, lettere ed arti | No 509
Venezia | 21 dicembre 1857

Al Chiarissimo Sigr M. Faraday | Londra

L'Istituto Veneto di scienze, lettere ed arti potendo pe' suoi statuti eleggere, in Italia e fuori, Soci corrispondenti che coll'opera gli porgano mano a raggiungere i propri scopi scienziati, o lo decorino coll'onorevole loro nome, aggregò in quel novero la Signoria Vostra Chiarissima, e la Presidenza incaricata da esso le partecipa questa deliberazione e si pregia di esprimerle i sentimenti della più sincera stima e del più profondo rispetto³.

Il Presidente | L. Menin

Il Segretario | Dr. Giacinto Namias

TRANSLATION

The Imperial Royal Institute of Science, Letters and Arts | No 509
Venice | 21 Dec 1857

To Dear Mr. M. Faraday | London

The Venetian Institute of Science, Letters and Arts, authorized by its statutes to elect, from Italy and abroad, corresponding members who with their work help it to attain its own scientific goals, or decorate it with their honourable name, elected in this category your dear self, and this Presidency has been charged to convey to you this decision and is delighted express its sentiments of the highest esteem and profoundest respect³.

The President | L. Menin

The Secretary | Dr. Giacinto Namias

1. Lodovico Menin (1783–1868, Maggiolo (1983), 199). Venetian historian.

2. Giacinto Namias (1810–1874, Maggiolo (1983), 216). Venetian physician.

3. See letter 3261.

Letter 3370

Johann Jakob Guggenbühl¹ to Faraday

24 December 1857

From the original in RI MS F1 I160

Abendberg, Interlachen | 24 Dec 1857

My dear Professor Faraday!

It is a long time since I have had the pleasure of seeing you², as I was not able last spring to make my usual turn to London in consequence of indisposition. But I have frequently spoken of you and your great discoveries, of w[h]ich I have just read an interesting chapter in the german translation of Dr. Whewell's, "history of the inductive sciences"³.

It give me a great pleasure, my dear Professor Faraday to forward you a picture of the glorious and blessed Abendberg⁴, in the agreeable hope that you will make it sometimes your home, and spend the holydays in our fine air; I should be most happy to see you there. The following Reports of distinguished writers, will shew you how much the Lord has blessed the work to w[h]ich is devoted my life and all my power. I should be very thankful, if one or the other of your literary friends, would give some accounts in the most circulated English papers, because I find that this important truth is not yet enough known and appreciated.

I propose to continue my researches about the causes of Cretinism throughout all Europe and America the next ten years (D.v.) and to bring this matter to a decided conclusion. It seems most probable that a malaria is the chief *predisposing* cause in the valleys but besides this are a great number of secondary causes, w[h]ich must be particularly studied in each country. I have dict[at]ed to one of my most advanced patients a s[c]heme of question,

w[h]ich will be printed, if once completed. I beg to look it o[v]er, that we can speak about it if I come once again to London[.]

With My heartily gratulations for the | new year | Believe me, my dear Prof Faraday | With the highest esteem | Your | Dr. Guggenbühl

Address: Via Paris | Professor Faraday F.R.S. | Royal Institution | Albemarle Street | London

1. Johann Jakob Guggenbühl (1816–1863, DHBS). Swiss physician.
2. Possibly in April 1856. See letter 3119.
3. Whewell (1840–1), 3: 108–13, 191–201.
4. This picture is mounted opposite this letter.

Letter 3371

Henry Bence Jones to Faraday

25 December 1857

From the original in IET MS SC 2

25 Dec 1857

Dear Dr. Faraday,

I send you a bad translation of a few lines from a German physicians Medical Journal. He¹ is the best physician at Berlin[.]

Yours most truly | H. Bence Jones

The cause of the violence of the attack on Faraday is because he says “Altho no mathematician & ultimately the appeal is to experiment”[.] This remark uttered as an aphorism says too much but certainly it does not say that Faraday thinks the mathematician useless & unnecessary & his opponents might well spare their advice & try to penetrate a little deeper into the meaning of the author[.]

In the knowledge of nature the mathematician can do no more than collect the conclusions of the experimental enquiry and thence abstract certain formulae which may lead to further research; but he is perfectly powerless when gaps exist in empirical knowledge. One such gap Faraday has perceived and it is not an assumption as his opponents say but simply logical that he purposes to fill up this gap by the usual course of empirical knowledge[.]

On the contrary it is illogical as some of his opponents do, to deny the existence of the gap & it is an assumption to assert that it cannot be filled up. I have clearly stated in opposition to Fechner² that our view is not closed by the simple knowledge of the laws of gravitation & by the phenomena of gravitation. We are compelled whilst we are without more perfect knowledge to assume a special force of gravitation as the cause of the phenomena & as a foundation of that law.

Such a force is only an empty formula so long as we do not know what it is, what relation it bears to other forces; whether it is adherent in matter or is communicated to it from without. The questions which Faraday throws out

have the advantage of marking out a distant path of enquiry to be followed for the solution of this problem & I hold that empirically logically philosophically it is perfectly right.

Is the force of gravitation only one of the possible forms of appearance of a universal natural force?

Can any of the known forces of nature also make themselves evident in the phenomena of Gravitation³[.]

Virchow

1. Rudolf Carl Virchow (1821–1902, DSB). Appointed Director of the Pathological Institute in Berlin in 1856.

2. Gustave Theodor Fechner (1801–1887). German psychophysicist.

3. This is a translation of the conclusion of Virchow's review of Faraday (1857a), Friday Evening Discourse of 27 February 1857, in *Archiv Pathol. Anatom. Physiol.*, 1857, **12**: 119–21.

Letter 3372

Faraday to John Murray

26 December 1857

From the original in NLS JMA

26 Dec 1857

My dear Sir,

I am very much obliged to you for your kindness & the trouble you have taken[.] All health & happiness to you is the wish at this time of Year[.]

Very Truly | M. Faraday

J. Murray Esqr | &c &c &c

Letter 3373

Julius Plücker to Faraday

27 December 1857

From the original in IET MS SC 2

Bonn | 27 Dec 1857

My dear Sir,

My best thanks for your interesting paper on the Relations of gold to light¹, which I received some time ago. I tried myself to prepare one of your purple fluids, but – you will think me a bad experimentalist – I did not succeed. I was much interested to get such a fluid, being itself a bad conductor, but containing well conducting particles in suspension, in order to examine if no particular arrangement of these particles would appear, when electric currents of different kind were sent through it. The discharge of Electricity through the tubes, exhibiting the stratified light, cannot be a transport of light, or luminous matter from one end of the tube to the other. There is, I think, within the tube a distribution of ponderable

matter produced by the discharge, that matter becoming luminous by it, while the discharge is a dark one, as you call it, from one luminous place to another.

I had the opportunity to examine a great number of tubes containing traces only of matter of a different kind. Since I showed the beautiful effect they present, at the Meeting of Bonn², several hundred of them have been sent to all countries, except till now to England. If any of my English friends had assisted to the meeting, I would have found the opportunity to send to you and to Mr. Barlow some of them. – Since that time I observed a quite new series of phenomena, which exhibit a very fine appearance. I can, in a few words give no better account of them but by saying, that I am enabled by means of the electric light, to *render luminous your lines of magnetic force*.

There is round the positive electrode (where heat is produced) a luminous atmosphere, sometimes of some inches in diameter, separated by a dark space from the Stratified light. By means of the Magnet this light is concentrated, if the electrode be a single point, to a brightly coloured line of magnetic force, passing through that point. If the electrode be a platina wire every point of it produces such a luminous curve. The system of all these curves constitute luminous surfaces of different forms, depending only upon the position of the poles. By commutating the polarity these luminous curves and surfaces of magnetic forces do *not* change.

What will do the light surrounding under different circumstances the *negative* Electrode?

In a paper sent to Poggendorff³. I gave a first account of these curious phenomena. When printed I'll send to you a copy of it.

With all my heart | Yours | Plücker

1. Faraday (1857c).

2. Of the Gesellschaft Deutscher Naturforscher.

3. Plücker (1858a).

Letter 3374

Christopher Hansteen to Faraday

30 December 1857

From the original in IET MS SC 2

Observatory Christiania | 30 Dec 1857

Dear and honored Sir,

I thank you heartily for your letter of 16 Decbr.¹ at first while you have written yourself, as you could better declare the circumstances – and secondly while I thereby have received an autographic letter from a man, which I in many years have honoured as one of the chief notabilities “in rebus

magneticis”². I preserve with delight and perhaps a little vanity letters from different English scientific notabilites, as Sir Joseph Banks³, Sir David Brewster, Professor Airy, Professor Forbes, General Sabine, Professor Barlow⁴ and others; and to this treasure I now can add yours.

Professor Oersted⁵ was a man of genius, but he was a very unhappy experimentator; he could not manipulate instruments. He must always have an assistant or one of his auditors, who had easy hands to arrange the experiment; I have often in this way assisted him as his auditor. Already in the former century there was a general thought, that there was a great conformity and perhaps identity between the electrical and magnetical force; it was only the question how to demonstrate it by experiments. Oersted had tried to place the wire of his galvanic battery perpendicular (at right angles) over the magnetical needle, but remarked no sensible motion. Once, after the end of his lecture as he had used a strong galvanic battery to other experiments, he said, “let us now once, as the battery is in activity, try to place the wire parallel with the needle”. As this was made he was quite struck with perplexity by seeing the needle making a great oscillation (almost at right angles with the magnetic meridian). Then he said: “let us now invert the direction of the current”, and the needle deviated in the contrary direction. Thus the great detection was made; and it has been said, not without reason, that “he tumbled over it by accident”. He had not before any more idea than any other person, that the force should be *transversal*. But as Lagrange⁶ has said of Newton in a similar occasion: “Such accidents only meet persons, who deserve them”.

You completed the detection by inverting the experiment, by demonstrating, that an *electrical current* can be excited by a *magnet*; and this was no accident, but a consequence of a clear idea. I pretermit your many later important detections, which will conserve your name with golden letters in the history of magnetism.

Gauss⁷ was the first, who applied your detection to give telegraphic signals from the observatory in Göttingen to the physical hall in a distance of almost an English mile from the observatory.

I very well understand your situation. I can also not work in company with other persons, and I read not much, for not to be distracted from my own way of thinking. I allow that thereby many things escape me, but I fear to be distracted upon sideways. “Non omnia possumus omnes”⁸. Every one must follow his own nature.

I have translated an extract of your letter, and sent it to Göttingen to Mr. Arndtsen⁹.

In the summer 1819 I visited in long time almost every day the library in “Royal Institution” in order to extract magnetical observations (declination and inclination) from old works, which our University was not in possession of, for instance “Hackluyt”¹⁰ and “Purchas”¹¹ his pilleggrims” etc¹²; so I am acquainted with the place of your activity.

I have in this year received your portrait from Mr. Lenoir¹³ in Vienna, as also of Sir David Brewster. They shall decorate my study on the side of Oersted, Bessel¹⁴, Gauss and Struve¹⁵.

Believe me Sir sincerely your | very respectful | Chr. Hansteen

1. Letter 3367.
2. 'in matters of magnetism'.
3. Joseph Banks (1743–1820, ODNB). Naturalist. President of the Royal Society, 1778–1820.
4. Peter Barlow (1776–1862, ODNB). Professor of Mathematics at the Royal Military Academy, Woolwich.
5. Hans Christian Oersted (1777–1851, DSB). Danish natural philosopher and Director of the Polytechnic Institute in Copenhagen, 1829–1851.
6. Joseph Louis Lagrange (1736–1813, DSB). French mathematician.
7. Carl Friedrich Gauss (1777–1855, DSB). Director of the Göttingen Observatory, 1807–1855.
8. 'we cannot all do everything'.
9. Adam Frederik Oluf Arndtsen (1829–1919, NBL). Norwegian physicist.
10. Richard Hakluyt (c.1552–1616, ODNB). Tudor and early Stuart geographer.
11. Samuel Purchas (c.1575–1626, ODNB). Writer on exploration.
12. Hakluyt (1598–1600) and Purchas (1625) are both recorded as being in the library of the Royal Institution in Burney (1821), 259.
13. George André Lenoir (1825–1909, BJDN). Chemist and instrument maker in Vienna.
14. Friedrich Wilhelm Bessel (1784–1846, DSB). German astronomer.
15. Friedrich Georg Wilhelm Struve (1793–1864, DSB). Director of the Pulkowa Observatory.

Letter 3375

Faraday to Margery Ann Reid

1 January 1858¹

From Bence Jones (1870a), 2: 397–8

Royal Institution | 1 Jan 1857 | [sic]. 5 o'clock P.M.

My very dear Girl,

Your aunt has just brought me your letter; she has just had it. *We both* write by my pen, to save the post. Poor girl! we pity you all, as you may think; it needs not to say how much. The suddenness and awful character of the case may make us all tremble in our love to each other, and that whilst, as I trust it will be with you, we look unto Him who rules all things according to the purpose of His own will, let us strive to accept the sorrow submissively, and at the same time to do what remains in our power with hope of a blessing on the intention.

How vain is life! In the midst of yours, which was not altogether smooth, still how great a trouble may be brought into it. But be composed; as far as remembrance of the hand that is over all can give composure, though it must be but partial. The Lord gave, the Lord taketh away. *Blessed be the name of the Lord*².

As for advice, I can have none to give – those only who are on the spot can tell rightly; but we feel sorry it should come in –'s way, though perhaps it may do no harm. – we should have confidence in, except that her strength is weak, "but out of weakness are made strong"³ is a comfortable thought.

Give our kindest love to your father⁴. In these heavy sorrows, I think the words of condolence shrink into my pen; the thoughts of your heart must speak for us; and we commend you in your great trouble to Him who is able to sustain you.

Your loving uncle | M. Faraday

1. Dated on the basis of the illness of Margaret Elizabeth Reid (1828–1858, GRO, Reid, C.L. (1914)), Reid's sister, at the end of 1857 which resulted in her death on 3 January 1858. Bence Jones (1870a), 2: 397 noted that this letter was occasioned by the death of Reid's mother (and Sarah Faraday's sister), Mary Reid née Barnard (1792–1845, Reid, C.L. (1914), but this was corrected in Bence Jones (1870b), 2: 392 to her sister.

2. Job 1: 21.

3. Hebrews 11: 34.

4. William Ker-Reid.

Letter 3376

Faraday to Juliet Pollock

6 January 1858

From the original in RI MS F1 E6

Royal Institution | 6 Jan 1858

My dear Mrs. Pollock,

You will not I am sure judge me unkindly for not answering your letter & act before. I have been very busy and am not able to go on actively as I was accustomed to do in former times but I thank you very much and receive the book gratefully in remembrance of one who is worthy of being kept in remembrance by all who had the pleasure & honor of knowing her¹[.]

I am glad to hear you have arrived in town safe and hope to call on you soon. If you have the opportunity call me to the remembrance of those who are at St. Juliens [sic]².

Ever My dear Mrs. Pollock | Your faithful & grateful Servant | M. Faraday

1. A reference to the death of Maria Julia Herries. See letter 3325.

2. St Julians, near Sevenoaks, the seat of the Herries family.

Letter 3377

Faraday to Sophia Romilly¹

11 January 1858

From the original in the possession of Tom Pasteur

RI | 11 Jan 1858

My dear Mrs. Romilly,

I think we use the words "to electrify" for "to charge with electricity" though I believe the words "to electrize" is also used.¹ Both words are also



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WINDOW & GROVE. 63^A BAKER ST LONDON

Plate 8. Juliet Pollock.

used to express not merely “charging with” but “treatment with”. I endeavour always to use the phrase “to charge with” when I have that meaning only².₁
 Ever Your faithful Servant | M. Faraday

1. Sophia Romilly, née Marcet (d.1877, age 67, GRO). Daughter of Jane Marcet.

2. Possibly Romilly wanted this clarification for Marcet (1858) where the section on electricity (pp. 444–98) was so revised as to be considered ‘altogether new’ (p.vii).

Letter 3378

Faraday to Charles Lock Eastlake

15 January 1858

From the original in NG MS

Royal Institution | 15 Jan 1858

My dear Sir Charles,

I should be glad to have specimens of the prepared cloth which you have used before I reply to yours – and if the pictures already back lined are accessible I should like to see them₁

Ever faithfully Yours | M. Faraday

Sir Chas. Eastlake Bart | &c &c &c

Letter 3379

John Frederick Mann¹ to Faraday

19 January 1858

From the original in RI MS F1 L/6/1

Bamfield Hill, Four Posts, Southampton | 19 Jan 1858

Sir,

Many years ago I had the pleasure of attending some lectures delivered by you at the R.M. Academy Woolwich, at one of which you informed the audience that the way in which savage nations obtained fire was by rubbing two pieces of dry stick together; at the same time, to a certain extent, illustrating the same by rubbing a piece of cedar stick up and down a piece of board of the same description, charcoal was soon formed and smoke emitted, but, if I recollect rightly you stated you had never been able to obtain a light.

Since that time, for the last fifteen or sixteen years, I have been in Australia, and have many opportunities of observing the habits and customs of the natives. I took the first opportunity of getting them to show me how they obtained a fire by the above means: one of them procuring a piece of “grass tree” readily undertook to do so.– Allow me in the first instance to describe this remarkable tree – *Xanthorrhoea* is the botanical name (though I doubt whether I am correct in the spelling). The stem varies in height from a few inches to 3, 4, or 5 feet; the diameter about the same size throughout its height, 5, 6 or 8 inches; this stem is not composed of wood but of a

series of laminated leaf like substances radiating from the centre and closely packed upon each other, the interstices being filled with an aromatic gum, which resembles coal dust; this gum is not affected by the heat of the sun, though easily melted by fire. On the top of this stem is a large tuft of long thin and sharp edged leaves, the section of which is triangular, much resembling that of a bayonet; from the centre of this tuft grows a long straight stick or reed, from 1 to 10 or 15 feet in length having a top something like a bulrush: this stick contains a pith but the outer shell or case is very hard and strong. These trees grow on poor sandy soil, principally on the ranges near the sea coast.

To procure a fire, as I have already said, one of these sticks was procured (of course a perfectly dry one) and broken in two; a notch was made in the side of one of the pieces, a small slice of the outer shell being cut off; this piece was laid on the ground and held in that position by another black, the end of the other piece was then inserted in the notch and the operator rolled it backwards and forwards quickly between the palms of his hands.

In the first instance a small quantity of very dry charcoal was procured from the burnt stump of a tree, this was enclosed in a handful of dry bark well beaten so as to resemble oakum, this was placed in an indentation in the ground immediately beneath the notched part of the stick. When the horizontal stick was half bored through by the drilling process of the other, a small *pinch* of very dry and fine charcoal was inserted into the hole, the operation was then renewed with increased vigour until the horizontal stick was bored through and the ignited charcoal mingled at once with that previously placed underneath to catch it, the spark was then most carefully blown until the whole burst into a blaze.

This operation requires the greatest care and practice, for the piece of stick between the hands requires to be pressed upon the other whilst rolled, this causes the hands to slide down, and in taking a fresh hold it is likely to come out of the notch, when the charcoal therein immediately becomes cooled, and the operation has to be commenced again, – In damp weather it often takes hours to procure a light.

During several journeys in that Colony particularly one made into the interior with the late Dr. Leichardt², I did not notice the “grass tree” growing, and am therefore not aware how the natives at those places procure fire, but that it is a most precious article is evident from the care they took in always carrying a “fire stick” whenever they moved from one place to another.

I may however add that amongst those natives who are at all in contact with the white population, the lucifer match has entirely superseded all other modes of theirs of obtaining fire.

Knowing how fully your time must be occupied I beg to apologise for the length of this letter and to remain

Yours most obediently | John F. Mann
 Professor Faraday FRS | etc etc etc | London

P.S. I have also endeavoured to illustrate the above subject and take the liberty of enclosing a rough sketch, but I fear it does not throw much light on the subject | J.F.M



1. John Frederick Mann (1819–1907, AuDB). After leaving the army in 1838, worked for the Ordnance Survey, before going to Australia as an explorer and surveyor.
2. Friedrich Wilhelm Ludwig Leichhardt (1813–1848, ODNB). German-born explorer of Australia.

Letter 3380

Antoine Thompson d'Abbadie¹ to Faraday

21 January 1858

From the original in IET MS SC 2

Paris, rue Bellechasse 31 | 21 Jan 1858

My dear Sir,

Some time ago you were kind enough to inform me that your heavy glass is not to be had in England. As I am in great want of it I have tried to persuade several glass manufacturers to undertake the making of it, and at last Messrs Maes² & Clémandot³ of Paris have consented if I can be fortunate enough to obtain from you answers to the following queries for they do not like to make several trials.

1. What are the ingredients? 2[.] their proportions. 3. Are they melted at a low or a high heat? 4. is there any particular process (in French, *tour de main*) necessary to prevent the formation of Striae and the Separation of ingredients according to their density? 5. What sort of crucible was employed by you? 6. How much of the glass did you find it convenient to make at once? 7. what was the Size of the largest *pure* bit of your heavy glass.

I do not wish to encroach on your valuable time by requesting an answer in your own hand, but I hope that you may be kind enough to direct one of your assistants to pen the answers to the foregoing questions and send them, *not* prepaid, to me at rue Bellechasse 31 Paris.

It is with much reluctance that I here venture to trespass on your time, but the only frenchman who ever made glass with an index of refraction equal to 2, & even then in small quantities & impure, is now dead, & if you do not come to my help I am afraid that I must give up my plan of improvement in Astronomical instruments. I hope at all events that I may some day be able to return your preceeding kindness towards me, & remain

ever most truly yours | Antoine d'Abbadie | Correspt. de l'Institut
Professor Faraday, F.R.S. membre de l'Institut | etc etc

1. Antoine Thompson D'Abbadie (1810–1897, DBF). Irish-born French explorer.

2. Louis Joseph Maës (1815–1898, Dufrenne *et al.* (2005), 407–9). Founder of the Clichy glass factory.

3. Louis Clémandot (1815–1891, Dufrenne *et al.* (2005), 79, 82). Chemist and Director of the Clichy glass factory.

Letter 3381

Faraday to Charles Lock Eastlake

23 January 1858

From the original in NG MS

Royal Institution | 23 Jan 1858

My dear Sir Charles,

In addition to your letter of the 13th instant¹ you sent me three specimens of cloth, steeped respectively in solutions of Microcosmic salt,– silicate of soda, and chloride of zinc.– the first burns freely;– the second nearly as well and not merely with flame, but also as tinder:– the third may be considered as incombustible. All of these contain salts in the *soluble state*; they are removable by a slight washing, and then the specimens are left as combustible as if they had never been prepared. The insoluble silicate remaining in the second, did not interfere with its burning.

Yesterday we looked at certain pictures which have been backed with silicate and chloride of zinc cloths². The application of the tongue, shews at once the presence, and the solubility, of the respective salts. In my letter of February 1857³ I objected to *soluble salts* in the back lining; and I do so still:– for though I cannot say that I am sure evil would result, I think it possible that it might. A soluble salt has an amount of diffusibility such, that, under the influence of varying circumstances and *of time*; portions may find their way into the frame, and even into the picture. I therefore prefer *fixed* matters that cannot thus travel. Nevertheless the pictures that have been lined might be left safely for six or twelve months, being examined from time to time, to see if there is any travelling of the salts in the lining, or any other recognizable effect produced, good or bad.

You yesterday shewed me a piece of animal parchment paper; which I think a very excellent material, though it is more combustible than the chloride of zinc cloth. Woollen fabrics, of which there are some, light and cheap enough (see the accompanying samples); are more combustible than the parchment paper, but will not catch fire from sparks. With a coat of earthy matter, analogous to white wash, I believe they would resist both sparks and small flames.

I am | My dear Sir Charles | Your Ever faithful Servant | M. Faraday
Sir Charles Eastlake Bart | PRA | &c &c &c

1. Not found, but clearly the letter to which 3378 was the reply.

2. For an account of this visit to the National Gallery see the journal of Ralph Nicholson Wornum, NG MS 32/67.

3. Not found.

Letter 3382**Faraday to Charles Lock Eastlake****25 January 1858****From the original in NG MS**

Royal Institution | 25 Jan 1858

My dear Sir Charles,

I think well of prepared canvass:– it is strong, smooth, impervious & will last a long while. I have been trying to prepare some cloths with jelly & then to tan them but they do not give any promise:– they are stiff, irregular, & not so incombustible as they should be[.]

I am My dear Sir Charles | Ever Your faithful Servant | M. Faraday
Sir Chas. Eastlake Bart | PRA | &c &c &c

Letter 3383**Faraday to Blackwood and sons¹****25 January 1858²****From the original in NLS MS 4131, f. 29**

Royal Institution | 25 Jan 1857 [sic]

Gentlemen,

I have received from some friend to whom I desire to return my best thanks a copy of the *Scenes of Clerical life*³. Can you aid me in directing them to the right quarter & if so will you do me that favour⁴[.]

I am Gentlemen | Your faithful Servant | M. Faraday
Messrs Blackwood & Sons

1. Blackwood & Son Scottish publishing firm. See Tredrey (1954).

2. Dated on the basis of the publication of the book.

3. Eliot (1858), published by Blackwood. See Eliot, *Diary*, 8 January 1858, p. 292 for her instruction that Faraday be sent a copy.

4. See letter 3384.

Letter 3384**Faraday to George Eliot****28 January 1858****From Cross (1885), 2: 12.**

Royal Institution | 28 Jan 1858

Sir,

I cannot resist the pleasure of thanking you for what I esteem a great kindness: the present of your thoughts embodied in the two volumes you have sent me¹. They have been, and will be again, a very pleasant relief from mental occupation among my own pursuits. Such rest I find at times not merely agreeable, but essential.– Again thanking you, I beg to remain, your very obliged servant²,

M. Faraday
George Eliot, Esq., &c &c

1. Eliot (1858). See letter 3383.

2. Eliot, *Diary*, 30 January 1858, p. 294 noted 'Received a letter from Faraday, thanking me very gracefully for the present of the "Scenes"'.

Letter 3385

Peter Henry Berthon to Faraday

28 January 1858

From the original in GL MS 30108/2/78

Trinity House, London, E.C. | 28 Jan 1858

Sir,

I am directed to transmit to you the accompanying sample of Red Lead which Messrs. Pilcher propose to provide for the Service of this Corporation, and as it is important that the best Red Lead should be supplied, I am directed to request you will subject it to such analysis as shall determine the genuineness of the Article.—

I am | Sir | Your most humble Servant | P.H. Berthon
Michael Faraday Esq. D.C.L. | &c &c &c

Letter 3386

John Peter Cassiot to Faraday

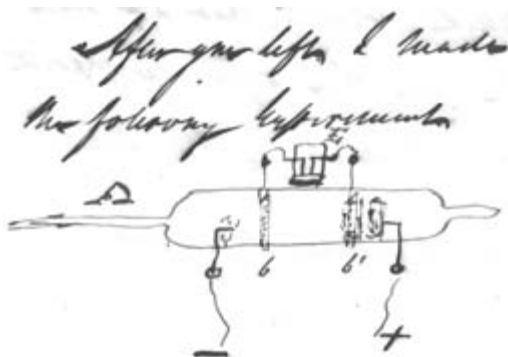
30 January 1858

From the original in RI MS F2 J225

Saturday | 10 o'Clock

My dear Faraday,

After you left I made the following experiment¹:



A No 5 Tube
 b, b' 2 Clips attached to E
 E D[ou]ble leaf Electroscope

Whether the clips *were as in figure* or *separated to their fullest extent or nearly in contact* the leaves of the Electroscope always *repelled*, shewing that at no time were there signs of opposite states of Electricity on the *Tube*.

When Wheatstone saw the Experiment in this (or a similar tube) he considered the striae as mere cases of induction, alternating + & -, if the experiment is correct this cannot be the case – at all events I give you the fact as I observed it,

Believe me | truly yours | J.P. Gassiot

Dr. Faraday

Endorsed by Faraday: 30 Jan 1858

1. See Faraday, *Diary*, 30 January 1858, 7, pp. 414–23 for his and Gassiot's experiments on the electric discharge.

Letter 3387

Faraday to Peter Henry Berthon

2 February 1858

From the original copy in GL MS 30108/2/78

Royal Institution | 2 Feb 1858

Sir,

I have carefully analyzed the specimen of red lead from Messrs Pilcher & sons received from you on the 28 January 1858¹. It contains no impurities mixed with it as commercial adulterations – no red oxide of iron or other red substances – nor any thing intended to make weight. Beyond that it is as far as I can judge a well manufactured & prepared article².

I am Sir | Your Very faithful Servant | M. Faraday
 P.H. Berthon Esqr | &c &c &c

1. Letter 3385.

2. This letter was noted by Trinity House Wardens Committee, 9 February 1858, GL MS 30025/25, p. 282. The order for red lead was placed with Pilcher.

Letter 3388

Faraday to Charles William Pasley

2 February 1858

From the original in RI MS F1 A25

Royal Institution | 2 Feb 1858

Dear Sir,

I have received your packet of bark & your letter respecting it & in the name of the Royal Institution beg to thank you for both. The specimen is a

very good illustration of the power of lightning: it will be a great addition to those we have for though like them in general effect it differs in several minor points. Your note on the back of the drawing says July 1858. I conclude I may alter the year to 1857.

I am | Dear Sir | Yours Very Truly | M. Faraday
Chas. Pasley, Esq., | &c &c &c

Letter 3389

John Peter Gassiot to Faraday

3 February 1858

From the original in RI MS F2 J229

Wednesday Evening

My dear Faraday,

You left your pencil case on the Table which I return I am afraid you were a little fagged today¹[.]

I avail myself of the opportunity to enclose a note of two facts², both of which I will shew you when we next meet.

If there are experiments which occur to you & which I can make I will do my best. Never mind your *reasons*, merely say try *so* & *so*, and I will then endeavour to vary the conditions, and not take any thing for *granted*[.]

Truly yours | J.P. Gassiot
M. Faraday Esq

Endorsed by Faraday: 3 Feb 1858

1. See Faraday, *Diary*, 30 January 1858, 7, pp. 423–30 for his and Gassiot's experiments on the electric discharge.

2. This note is in RI MS F2 J230.

Letter 3390

Faraday to Fourth Duke of Northumberland¹

10 February 1858

From the original copy in IET MS SC 3

To his Grace | The Duke of Northumberland K.G. | &c &c &c &c

Royal Institution | 10 Feb 1858

My Lord Duke,

According to your Grace's kind wish Mr. Barlow has shewn me the letter he received from you and therefore I take the liberty of expressing an opinion upon *one point*, being sure from the kindness Your Grace shewed me when on a former occasion the matter of the Presidentship was in consideration that I shall not by doing so give offence. I think it would be a very serious

thing for the *President* & the *Secretary* to resign at the same time. It would be sure to give occasion to the thought that there was some reason touching the character of the Institution which united the two in the act; and the thought would be the more *inconvenient* because, as no open reason would appear, an unpleasant one according to the common course of human nature would be assigned. It grieves me to think that either President or Secretary should ever leave the Institution but as such events must occur in the course of nature I do hope most earnestly that the resignation of the one may overpass the other by a year or two, that the present policy which seems so good & prosperous may not be suddenly interrupted but transmitted through the gradual change.

Again begging for your Graces kind reception of my free thoughts on this occasion as on others I sign myself most truly Your Grace's free, faithful, & humble servant

M. Faraday

1. Algernon Percy, 4th Duke of Northumberland (1792–1865, ODNB). President of the Royal Institution, 1842–1865.

Letter 3391

Peter Henry Berthon to Faraday

10 February 1858

From the original in GL MS 30108/2/79

Trinity House, London, E.C | 10 Feb 1858

Sir,

I am directed to transmit to you the accompanying Four Samples of White Lead, and to request you will be so good as to analyze the same, and favor me with the result for the Board's information¹.—

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq: | &c &c &c

1. Faraday's notes of his analysis are on the back of this letter.

Letter 3392

Fourth Duke of Northumberland¹ to Faraday

12 February 1858

From the original in IET MS SC 2

Alnwick Castle | 12 Feb 1858

My dear Sir,

I am obliged to you for your letter²: it was for the purpose of having the advantage of your opinion, & that of Mr. Pole³ that I requested Mr. Barlow to consult you & him. I have just one object, namely to act for the advantage

of the Royal Institution; and I shall be guided in this matter by the opinions I have elicited, & when in London I shall speak to you upon it.

Yours truly | Northumberland
Professor Faraday

Endorsed by Faraday: Nb 1858. | About resigning &c &c | Mine &c

Address: Professor Faraday | Royal Institution | Albemarle Street | London W.

1. Algernon Percy, 4th Duke of Northumberland (1792–1865, ODNB). President of the Royal Institution, 1842–1865.

2. Letter 3390.

3. William Pole (1798–1884, AO). Treasurer of the Royal Institution, 1849–1865.

Letter 3393

George Gabriel Stokes to Faraday

12 February 1858

From the original in RI MS Conybeare Album, f. 7

69 Albert St. Regents Park, NW | 12 Feb 1858

My dear Faraday,

Dr. Tyndall promised me a Lady's ticket for your lecture¹ but as it has not come I suppose he has forgotten it. Could you spare me one as my wife² would like greatly to hear your lecture? I am sorry to disturb you in your preparations[.]

Yours very truly | G.G. Stokes

1. Faraday (1858b), Friday Evening Discourse of 12 February 1858.

2. Mary Susanna Stokes, née Robinson (c.1823–1899, ODNB under G.G. Stokes). Married Stokes in 1857.

Letter 3394

Christian Friedrich Schoenbein to Faraday

15 February 1858

From the original in UB MS NS 435

Bâsle | 15 Feb 1858

My dear Faraday,

I am afraid you will be dissatisfied with Mr. Schoenbein and think him to be a very lukewarm friend, if not even a forgetful one, and I must allow, appearances are strongly speaking against him; but I can assure you at the same time, that coldness of feelings has nothing to do with the silence he has been keeping these many months. You know perhaps that in our commonwealth I have become a sort of political and public character i.e. a member of

our little parliament and as such I have got duties to fulfill. Now, of late, a party having sprung up amongst us, that attempted to change some fundamental principles of our constitution and your friend being a staunch conservative, he conjointly with his political friends of course opposed that tendency and the consequence was that in our senate we had some battles to fight, in which I could not help taking some active part, both within and out of doors. You will no doubt smile at Mr. Schoenbein's acting a political part and you are quite right in doing so, for I will and cannot deny myself, that he is by no means the proper man for dealing in politics and may add, that the bias of this mind does not go that way neither. You must be aware, however, that the citizens of a small republic, such as our's is, are not always allowed indulging their private taste; they are now and then, as it were, forced to handle things, which they have not the slightest mind to touch, and such is Mr. Schoenbein's case. Our many-headed sovereign (the people) proves in general to be more imperious and exacting than your monarchs even, so that very often little choice is left us between following and disobeying his pleasure and commands¹.

I tell you all these things, indeed very insignificant in themselves, to account for and justify my long taciturnity, for you may easily imagine, that the mind being seriously occupied with such matters, is little apt for any thing else, even not for letter-writing. Now, after having carried a most signal and decisive victory over our antagonists, we belong again to ourselves, so that nothing prevents us from reassuming our wonted peaceful work and I hasten to make use of the very first moment of the leisure-time regained to pay old debts to my friends.

First of all permit me to express you my most grateful thanks for the very numerous proofs of kindness and benevolence which you have of late been pleased to bestow upon my daughter. She was really overhappy in having been favoured so much as to enjoy the enviable privilege of passing some days at the Royal Institution and getting introduced into the amiable family of your near relations. And I need not add, that the juvenile lectures², which you kindly allowed her to attend, highly pleased and interested the girl. I do not wonder at all the great pleasure and gratifications she has derived from such favors and in reading the girl's lively descriptions of what she saw, heard and felt on the occasion, I could not help envying Miss Schoenbein and being a little jealous of her. The girl looks on the new world of wonders, in which she has been placed, with open eyes and all the freshness of youth, and, even at the risk of being taxed with partiality, I tell you, that the young maid now and then surprises me by the justness of remarks, which she makes upon men and things.

According to her often repeated assurances my daughter feels quite happy in England and has (to me the most important point) become so exceedingly fond of her sphere of activity there, that the Idea of soon returning to Bâsle is far from being a flattering one to her. Her truly filial attachment

to Miss Hornblower is daily growing stronger and deeper and every letter of her's bears ample evidence of the feelings both of the deepest affection and highest veneration she entertains towards your excellent friend. You may easily conceive, how much gratifying such news must prove to myself as well as to Mrs. Schoenbein and as it was by your kind interference, that our beloved Child has been so happily placed, both of us feel ourselves laid under the deepest obligations to you and you may rest assured, that this great piece of friendship will never be forgotten by us. Mrs. Schoenbein charges me to offer you in her name the most heart-felt thanks for Your Kindness.

From the very same reasons, that forced me to be neglectful to my friends as a correspondent, I have for some time very little worked, though it would not be quite true if I said to have been entirely idle. Now and then I took up some little piece of work, but without doing any thing being worth while of speaking about. I entertain however the hopes, that the forthcoming spring in renewing all Nature around us, will also call forth some dormant powers of my mind and stir me up again to scientific activity. At this present moment there is some dullness, I had almost said, sleepiness about me and it is full time to get rid of that drowsy disposition of mind. I saw the other day my last letter to you in the *Philosophical Magazine*³; the epistolary production hardly merited the honor of being printed, be that however as it may, there is at any rate no harm in publishing such trifles and queer Ideas. I will not let pass unnoticed a little misprint, which is singular enough. Whilst from several reasons I have made it a point never to communicate any thing to the french Academy, the printer has put "Academy of Paris" instead of Munic⁴. Or have I perhaps made the mistake myself in my letter? Errare humanum est; I do not think it however worth while, that the error should be corrected. From what you told me in your last letter, it appears that you are at present engaged in researches of the highest importance, for the problem to be solved is really of a transcendent nature. You only could think of undertaking such a bold enterprize and I wish you from all my heart full success. How does Mrs. Faraday fare? I confidently hope and ardently wish, that she is going better. Pray present my best compliments and kindest regards to her. In begging you kindly to excuse the emptiness of my letter I am, my dear Faraday for ever

Yours | most faithfully | C.F. Schoenbein

Address: Doctor Michael Faraday | &c &c &c | Royal Institution | Albemarl Street | London

1. A reference to continuing French interference in the internal affairs of Switzerland. See Imlah (1966), 89–90.

2. For Faraday's Christmas lectures on 'Static Electricity' see RI MS F4 J18.

3. Schoenbein (1858a), the scientific part of letter 3335.

4. *Ibid.*, 26.

Letter 3395**Faraday to Peter Henry Berthon****16 February 1858****From the original copy in GL MS 30108/2/79**

Royal Institution | 16 Feb 1858

Sir,

I beg to report to you the results of the Analyses of the ground White leads received on the 10th instant¹. I have had to delay these & partly in consequence of my desire to be quite sure in respect of the adulterated sample. No. I. (Pilcher & Sons 8 Feb 1858).

White lead	65.5
Oil	14.7
Sulphate of baryta	19.8
	100.0

The Sul baryta was very white – it appears to enable the mixture to take up more oil without settling out at the top
No. II. H. Grace² Bethnal green (Feb 1858)

White lead	88.4
Oil	10.8
Sulphate of baryta	0.8 – very small proportion
	100.0

No. III. Tudor³ (Feb 1858)

White lead	87.3
Oil	12.7
Insoluble the smallest trace – not Sul Bartya	
	100.0

Clear oil. settled out at the top of this specimen

No. IV. Pontifex Wood

White lead	91.1
Oil	8.9
Insoluble – a mere trace not Sul baryta	
	100.0

There was no other impurity in any of the samples except the mere traces which are inevitable in all manufactured articles. I do not like to see Sulphate of baryta even in so small a quantity as 0.8 per cent because it seems to show that the adulteration is sometimes present in the mills. I suppose however that some instances require an adulterated article occasionally. The quantity of oil held by the sample No. I is retained by the presence of the baryta. The next large quantity in the pure preparation No. III separated partly in small clear portions at the surface⁴[.]

I am Sir | Your Obedient humble Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letter 3391.

2. Henry Grace. White lead supplier of Hollybush Gardens, Bethnal Green Road. POD.

3. Samuel and William Tudor. White lead suppliers of 166 and 167 Upper Thames Street. POD.

4. This letter was read to Trinity House Wardens Committee, 23 February 1858, GL MS 30025/25, p. 299. It was agreed to place the order with Pontifex.

Letter 3396

Faraday to Charles Vincent Walker¹

19 February 1858

From the original in the National Academy of Sciences, Washington

Royal Institution 1858 | 19 Feb 1858

My dear Sir,

We accept your present of a graphite plate² with many thanks. It seems to be an excellent plate in strength & according to your account in action & every other point[.] How wonderfully electricity in its apparatus applications services & students indeed in all points is advancing over both matter & mind – the earth & the human being on it. I am delighted to find that your system of telegraphing is justifying itself in real practice for that is the end we look for in all these things:– it is just when the matter as a novelty is fading out of the minds of men generally that the full & confirming proof is obtained of its real & permanent value[.]

I am My dear Sir | Very Truly Yours | M. Faraday
C.V. Walker Esq | &c &c &c

1. Charles Vincent Walker (1812–1882, ODNB). Electrician.

2. On this see Walker (1857, 1859).

Letter 3397**John Tyndall to Faraday****19 February 1858****From the typescript in RI MS JT TS Volume 12, pp. 4077–81**

Friday, 19 Feb 1858

My dear Mr. Faraday,

Will you grant me your patience while I endeavour to lay before you a few reflections which arise out of my recent conversation with you regarding the shortening of my course of lectures¹. At the time of our conversation I was so deeply sensible of the kindly feelings which, as you informed me, the Managers had manifested towards me, that I thought of nothing else². Perhaps I now labour under a misconception, and if so, I am sure that you will have the goodness to set me right.

The matter, as I understand it, stands thus:– I have agreed to perform certain duties, for which the Institution grants me a certain annual salary, and which the Managers have the right to require of me. My friend Dr. Bence Jones is to give his opinion that my health is impaired by these duties, and the Managers, acting on his report, propose to continue my salary at its present amount, while they diminish the quantity of work which I have to perform.

I feel that your own natural disposition enables you to solve mine so thoroughly that I have no fear of your misunderstanding what I have to say upon this proposition. I ask you, therefore, would you not in my place consider that such an arrangement would place you in a doubtful position as regards the Institution? The acceptance of the change, under the proposed conditions, would, I fear, materially hamper a feeling of independence which, throughout my life, I have endeavoured to maintain. If an alteration is to be made at all I certainly should not like to see it made on the grounds of my being unable to discharge the duties which the Managers regard as merely a reasonable return for the terms which they grant me.

Will you in your kindness permit me to draw your attention to the exact circumstances connected with my joining the Institution? I had three offers before me at the time³. The R.I. offered me £200 a year for 19 lectures. Another institution would have given me the same sum⁴, an assistant, a laboratory and funds for experiments, for 6 lectures annually; and I was afterwards given to understand that this offer would be made still better.– At the conclusion of one of my first lectures at the R.I. a gentleman connected officially with a Government establishment, who had heard of my probable appointment at the R.I. took me aside and advised me strongly not to accept the professorship, telling me that I should regret it if I did, for another and a better post was open to me.

Subsequent to this, as you are aware, the late Sir Henry DelaBeche⁵ sought to induce me to accept an appointment in the Government School of Mines⁶. Before me lies a letter from poor Edward Forbes⁷, in which he communicated to me the strong desire of himself and his colleagues to have me among them⁸. There was nothing in my agreement with the R.I. to prevent me from holding this post in connexion with my professorship, but the fear that such a connexion might interfere with my duties at the Royal Institution caused me to refuse a position which, in many respects, would have been extremely agreeable to me.

This however occurred subsequent to my appointment at the R.I. and therefore could not influence my decision when the professorship of physics was offered to me. I might refer to other cases which occurred still later – to offers from Woolwich⁹ and elsewhere, one of which referred to a position, not however in England, worth £600 a year¹⁰. I refer to these instances on account of their bearing up on a passage of a letter which I shall cite presently, and to show that since I came to the Institution I have exhibited no disposition to be unfaithful towards it.

But when I made my choice in 1853 the two offers first mentioned were before me, and side by side with these was the offer from the Royal Institution. A short time before the official communication reached me I received a letter from which I extract as follows:– “Mr. Faraday proposed you as Professor of Physics, to receive £200 a year and to give 19 lectures. To show what we might, and ought to do, he read a statement of what the Managers had done for Davy, and he said he saw no reason why we should not do the same now. Davy had £100 and rooms the first year, the second year £200; the third £300; the fourth £400; the fifth an extra hundred for an excursion; the sixth it was repeated, and he was allowed to take a travelling assistant and have his expenses paid; the seventh he had a multitude of other appointments. . . . I have no doubt that a scale sliding upwards will be the result, and you will be allowed any sum however great, for experiments”¹¹.

On Thursday the 26th of May 1853 a letter reached me from which I will make one or two extracts:– “The Managers met to day, and I am requested to communicate to you officially, that in consequence of a recommendation from Professor Faraday the Managers are desirous of proposing you for election as Professor of Natural philosophy, with £200 a year”. . . . “The £200 may and will be increased *in a year*, and there is no reason why after a time you should not have £400 (or more) if you devote yourself to the R.I. – Davy had, and Mr. Faraday has quoted him as a precedent for this proceeding. We had a very full meeting, and all were for you, and some for offering you more at first, while all agreed that it was very poor pay”¹².

These are the inducements which I had to weigh against the two other offers to which I have referred. I accepted the professorship at the R.I., but I think it will be granted that some discrepancy exists between the above extracts and my actual position after the lapse of nearly five years. I confess that this has often been a source of dissatisfaction to me.—Dissatisfaction perhaps as much with myself as with others. For had I not reason to infer that I had not come up to the Managers' expectations regarding me, and that the discrepancy to which I have referred was to be attributed, in part at least, to failure of my own?

I have been repeatedly urged by two or three friends who are acquainted with the above circumstances to bring them under the notice of the Managers. But my repugnance to such a step has been hitherto insurmountable. Urged by a friend, I wrote a letter upon the subject last autumn; but when finished I put it aside, and it has lain in my drawer ever since. At present, however, I think perfect frankness on my part is better than remaining silent as I have hitherto done.

Thus far I have looked at my connexion with the R.I. from a commercial point of view merely. Need I say that from first to last this is the consideration which has had least influence with me. The extracts which I have quoted indicated a feeling towards me which I regarded as a thousand times more precious than any pecuniary emolument that the Royal Institution could bestow. This was my feeling at the time, as my letters if consulted, would shew. It is my feeling now. The Institution has surrounded me with relations which to me are above all price. Indeed my feelings towards the Institution are such as would induce me, were I sufficiently independent, to give it my best services without any pecuniary return whatever; for no return of the kind which it could make is to be put in comparison with the happiness which I have derived from the personal relationships, for which I have to thank my connexion with the Institution.

If I were permitted to give utterance to my feelings on the point in question I would express the hope that should the Managers legislate upon the subject, they will not suffer themselves to be influenced in any degree by considerations regarding my health; but be simply guided by what they consider to be just towards me and advantageous to the Institution and to science. Were I relieved to some extent from lecturing I should not become an idler. Other, and perhaps higher work, would be before me. This, and not my health, is with me the point of chief importance, but as things stand at present I find my power of following up such work far more limited than I could wish it to be. This is the point of view from which I should like the question to be regarded. For my health I have no fears, for it is always in my power to repair its injuries by giving myself rest, and I know the reliance which I can place upon a constitution naturally tough and unshaken by physical intemperance.

Believe me dear Mr. Faraday | Ever yours most faithfully | John Tyndall

Endorsed: To be opened when Mr. Faraday has abundance of leisure, but not before.

Address: Professor Faraday.

1. See letter 3272 and Tyndall, *Diary*, 19 February 1858, 7: 273–5.
2. See RI MM, 1 March 1858, 11: 220 where it was agreed that Tyndall should deliver no more than twelve lectures each year.
3. See Eve and Creasey (1945), 44–5.
4. This was the London Institution. See Tyndall, *Diary*, 20 February 1853, 5: 194–5.
5. Henry Thomas De La Beche (1796–1855, ODNB). Director of the Geological Survey from 1835.
6. See Eve and Creasey (1945), 45.
7. Edward Forbes (1815–1854, ODNB). Palaeontologist at the Geological Survey.
8. Forbes to Tyndall, 27 December 1853, RI MS JT/1/F/25.
9. Tyndall, *Diary*, 23 November 1854, 5: 391.
10. At Toronto. Tyndall, *Diary*, 26 April 1855, 6a: 46–52.
11. Bence Jones to Tyndall, 9 May 1853, RI MS JT/1/J/50.
12. Bence Jones to Tyndall, 23 May 1853, RI MS JT/1/J/51. See RI MM, 23 May 1853, 11: 13–14.

Letter 3398

Faraday to John Tyndall

20 February 1858

From the typescript in RI MS JT TS Volume 12, pp. 4145–6

Royal Institution | 20 Feb 1858

Dear Tyndall,

Your letter of yesterday¹ has taken me by surprise in this respect:— that it seems to imply that you had reason to believe that I was conscious of an understood engagement, rising year by year £100, according to the terms of the first quotations from a letter by a third person which you have sent me:— or of an engagement at £200, which was to be increased in a year, and should rise up to £400 or more after a time, according to your second quotation. I was not conscious of any such understood engagement, and you may be quite sure that if I had felt myself answerable in any way, either by expression or implication, for such an understanding, I would have seen it carried out or else the whole matter rearranged. At the same time it is also true that I quoted Davy's case to the Managers as a good precedent and an example for their departing from the course which, through circumstances, they had been following for many years. I should have been very glad indeed if I could have felt at the time that they had the means of doing it; and you must not suppose that I would not in that case have urged some such course between that time and this. I have often considered whether I should not aid such facility, and do more good to the Institution by retiring from the place I hold, than by keeping it. Whilst waiting, however, I hoped that other engagements would rise up, that could be held by you conjointly with the one here, and give you

that return on the part of science which you so thoroughly deserve for your labours in its cause.

Your letter to me of last April² I brought (according to your request) before the Managers at the time, and again last Monday³. They shewed the kindest feeling. They could know nothing of the sense of the extracts which yours to me of the 19th inst. contains, but only of the case of Mr. Brande, to which you refer in that of April, and they seemed quite ready to go beyond that, and will meet on the 1st of next month to settle that matter⁴. Your last letter puts the subject altogether on new grounds, and will take them unawares perhaps more than it did me, for though they may know your worth to the Institution and to science, they cannot be aware of the latter point to the same extent as I am. They could not be conscious of the understood engagement, and they have not the power to be liberal patrons of science, or to permit themselves to reward it in proportion to their admiration of it.

And now, my dear Tyndall, let me say that my chief care in this note had been to write only that which would be necessary to clear away misapprehensions and remove the implied charge of injustice or forgetfulness from either myself or the Managers or any other person. We know your value to the cause of science, and therefore to us; but we also know our incompetency to express the proper sense of it. However on that I must not speak too hastily for others. I know you agree with me in the matter of open dealing, and therefore am sure that if you have entertained the impressions above referred to you will let me make the true state of the case known to the board of Managers at its next meeting.

Ever, my dear Tyndall | Truly yours | M. Faraday

1. Letter 3397. This reply of Faraday's was agreed after Faraday and Tyndall had met in the morning. Tyndall, *Diary*, 20 February 1858, 7: 275–7.

2. Letter 3272.

3. Not minuted in RI MM, 15 February 1858, 11: 215–7.

4. See RI MM, 1 March 1858, 11: 220 when the number of Tyndall's lectures was reduced to twelve.

Letter 3399

John Tyndall to Faraday

22 February 1858

From the typescript¹ in RI MS JT TS Volume 12, pp. 4082–3

22 Feb 1858

My dear Mr. Faraday,

Though deeply sensible of the kindness which suggested the arrangement, I did not quite like the idea of having my duties lessened on the ground that I was incompetent through ill health, to perform them, my

salary at the same time remaining intact. I therefore sent to you, on the 19th², a statement of facts which I thought calculated to shew, that I might reasonably expect to see these duties diminished on grounds which should relieve me from a feeling of a weighty personal obligation. My conversation with you on Saturday³ removes all uncertainty from my mind as to the real origin of the extracts which I laid before you. They doubtless expressed the private views of that member of their body whom the Managers had deputed to communicate with me, and did not commit the Managers generally. Still, standing as they did, in such close juxtaposition with an avowedly official communication, it was natural that I should place, as I did place, perfect reliance upon them. At the time referred to I was a comparative stranger to the writer of those letters, but he is now an intimate friend, the generosity of whose character, and the kindness of whose heart, I appreciate so highly that it would give me pain to see him reminded, in a manner which must be unpleasant to him, of the inducements which he held out to me. It is solely through a desire to spare him this unpleasantness that I now ask you not to communicate my letter of the 19th. to the managers. That *you* were "conscious of an understood engagement", and had neglected to see it carried out, is an idea which I never entertained.

With regard to the precise matter in hand I would say, that if the Managers are disposed to regard me purely in the light of scientific lecturer to the Institution, the terms which they now give me do no credit to their liberality. I call, for example, the terms of the London Institution liberal, and they are about the same, per lecture, as I receive from the Royal Institution. True the labour in both cases, is widely different, but that is beyond the question. I have therefore not the slightest fault to find with the terms as a lecturer – They are good terms – and if the Managers would have the kindness to shorten my course⁴, and reduce my salary in the same proportion I shall retain a position which I consider it an honour to be called upon to fill. But it is my misfortune to desire to do something beyond lecturing. This desire, existing side by side with the demand for lectures, often, it is true, produces weariness of brain; but for this I am myself accountable, and I should shrink from the idea of becoming on this account, a burden to the Royal Institution. I am sure you will enter into my feelings, and will not ascribe to it any want of appreciation, on my part, of the great kindness of the Managers, when I say, that any change which diminishes my duties, while it leaves me in possession of my present salary, could only be agreeable to me under the condition that it is made upon scientific grounds alone, and without any reference to the state of my health.

Believe me dear Mr. Faraday | Ever yours most faithfully | John Tyndall

1. There is a draft of this letter in RI MS JT/1/T/432.
2. Letter 3397.

3. That is 20 February 1858. See note 1, letter 3398.

4. See RI MM, 1 March 1858, 11: 220 when the number of Tyndall's lectures was reduced to twelve.

Letter 3400

Charles Frederick Winslow to Faraday

1 March 1858

From the original in IET MS SC 2

West Newton | 1 Mar 1858

Dear Sir,

For a long time I have thought of taking the liberty to address you a few lines but knowing your time must be fully occupied with your scientific experiments & physical discussions I have hesitated to ask a moment even for the perusal of a letter. But having travelled & observed much & occupied much time in physical researches not wholly disconnected with the generalizations springing from your inquiries & discoveries I have ventured to ask your attention to facts & deductions embraced in the foregoing sketch¹. It is but a mere physical sketch, still quite sufficient for you to discern its bearing on the results of your own experiments & thought.

I have sent a copy of this paper to Professor Airy² & Gen. Sabine with a request that they may confer with you on its new direction of inquiry herein developed, & if there be any fact or suggestion in it worthy of application to your discussions in different departments of Science I shall be most happy for you to avail yourselves of them. I present this subject to you in no spirit of egotism, but with a hope & belief that a pregnant field of discovery is concealed in this germ. I trust you will give it the attention you & the other eminent minds around you may consider it to deserve. I beg you not to be too hasty in rejecting the general idea because it may conflict with theories of past or present thinkers or philosophers.— So far as my own observations extend in *geographical & geological* directions (& they have been large) I know of no facts which conflict, but all coincide, as you will see hereafter if I live to bring out my conclusions on these points which indeed are wholly subordinate to the fundamental laws here laid down. These, together with *astronomical* conclusions, I scarcely hint at here; but if you should detect or suspect anything of importance in either direction & wish to compare thoughts or conclusions on doubtful points with my own facts I should be most happy to communicate with you & aid you (if I could hope to be of so much service) in attaining to ultimate generalizations.— I have much that I would like to say to you — I have great store of facts & details which I shall endeavour to arrange & bring out systematically in some way hereafter. I date my first taste for physical inquiry from listening to Arago in 1835 in his lectures at the Paris observatory, when I was a student of Medicine in France. I was in London in 1838 [sic] & was then honored with most kindly attentions by Sir Astley Cooper³ who one

morning after I had breakfasted with him (the morning after King Wm 4th died at Windsor⁴) detained me in his study & gave me a *whole hour* (one of the most precious of my life) & on parting said he “Now Dr. Winslow I wish you well & accept my parting words. Do not ever be married.” I was surprised & begged him to give me his reason for such advice. His reply was “If you are never married you will be a very useful man. If you are married you will be engrossed in the cares of your family & will never accomplish the work God has intended for you to do.” He shook hands with me & dismissed me saying “I meant to have given you another hour, but William died last night & I must go to Windsor.” I have always thought of his remarks. But I was disobedient to his suggestion – I was married in a year afterwards & have found things a good deal as he prophesied; although from possessing nothing I have acquired some property by my profession, travelled over the globe with my family & alone considerably & have found leisure to cultivate myself in directions of which I had scarcely dreamed when Sir Astley helped me.–

I mention these things only for the purpose of introducing myself to you as I am a very humble person, & so far have made no impression upon or name in the world & probably never shall. But I enjoy all your researches & all knowledge obtained in all departments of Science & I trust you will honor me by expressing your objections to my deductions or referring me to facts which may tend to controvert them₁.]

With great respect | My dear Sir, I have | the honor to be your |
humble Servant | C.F. Winslow
Professor M. Faraday | London

1. Winslow began this letter at the end of the offprint of Winslow (1858). On p. 369 he referred to Faraday (1857a), Friday Evening Discourse of 27 February 1857.

2. See Winslow to Airy, 20 March 1858, RGO6/471, f. 214 which also enclosed a copy of Winslow (1858), f. 211–4. See also the press copy of Airy to Winslow, 12 April 1858, RGO6/471, f. 215–6.

3. Astley Paston Cooper (1768–1841, ODNB). Surgeon.

4. William IV (1765–1837, ODNB). King of England, 1830–1837. Died on 20 June 1837.

Letter 3401

Samuel Elkins Phillips¹ to Christian Friedrich Schoenbein

8 March 1858

Faraday to Christian Friedrich Schoenbein

c.9 March 1858

From the original in UB MS NS I A 761

8 Bexley Place, Greenwich | 8 Mar 1858

My dear Sir,

Are you sufficiently democratic to receive the congratulations of a self-taught working man on your recent Ozone researches – I should not seek thus to intrude myself, but that I have been for some years (tho with slenderest means) wading deeply in very kindred abstractions – I think the truth

will come out more simply than you anticipate and more in the direction of Electro-polarity but you are working *successfully* & well & I would fain encourage rather than divert[.]

I have for some years past uttered (in private) the strongest denunciations against modern chemistry in general and the doctrine of types & substitutions in particular but I begin to think there may be *a little* truth in it. And however indignant may still be my feelings against the idea of 3 atoms of Cyanogen combining together (as such) to form a tribasic acid (Cyanuric) and generally against the modern notions of bi and tribasic acids, yet I cannot divest myself of the conviction that herein also there is a *little* substratum of truth which dimly perceived has led to such outlandish theories[.]

In studying the amide ureide carbydrogen and other forms of Ammoniacal type I have apparently gone far beyond Hoffman² & others and must admit herein some *very few cases* which old *Electro* chemistry may peradventure prove unable to explain, or at any rate for the time being[.]

And just so I feel in regard to the idea of double or multiple molecules[.]

Any thing more disgraceful than the current methods of halving or doubling atoms in order to bolster up preconceived hypotheses I cannot well imagine, yet still there are peculiarities that might extenuate much of hypothetical latitudinarianism— (a) (CO) is probably the radical of carbonic acid (CO)O b 2(CO) c in like manner that of oxalic acid (C₂O₂)O d I claim to have discovered the law by which compound uses are constituted (inic substances being those wh. act & react like chlorine B. I and other so called simple bodies) but your field of investigation involves a *narrow* sphere where I cannot clearly discern the way in[.]

it will not suffice to say that CO₂ is a binary compound of a + O' and oxalic acid is another binary compound of b + a because chemical evidence leads to the idea that both acids are simple oxides (or protoxides) of their respective radicals, in other words that CO has a perfect equivalence to C₂O₂!

Again these radicals in common with numerous carbydrogens, severally replace the H elements of Ammonia, to the preservation of a standard type and a generic series of properties & reactions – And in doing so it seems yet more marvellous that while (C₂O₂) or (C₁₂H₅) may in the Ammonia, each possess its own independant H equivalence, yet in another and *perfect analogue*, the 2 atoms are merged in one and both together replace only one H so that the radicals of oxalic and Benzoic acids similarly exemplify the same principle or anomaly. One is CO = 1 atom

CO

the other is C₁₂H₅ = 1 atom

C₂O₂

With a given ammonia we might eliminate the C_2O_2 & replace the same by H to the preservation &c &c &c With another we might eliminate ($C_{12}H_5$) in like manner, but with a third containing both C_2O_2 and $C_{12}H_5$ we can eliminate neither their individuality is lost nor can we any how reproduce them without a destruction of the type &c.

With many other deficiencies it so happens that my experimental abilities are of the very meanest character, otherwise in this age of Rhumkorff coils I have long wished to look out upon the Ozone horizon of Atmospheric electricity somewhat thus

A B C
a b c represent three rooms. B. coil

If a b c represent three rooms, B containing a powerful coil, so strong as freely to give sparks from a single pole, then if an expanded pole be placed in A and a similar expansion of the other pole of the secondary wire be placed in C in connection with an arrangement to secure a similar directive influence in A & C it does thence appear to me that A might be made very minus E while C might be correspondingly plus[.]

What would an electrician say of the A or C atmospheres knowing nothing of the intermediate conditions and what would the ozone test papers say?

With a reverence in great men who act as well as think increased by the conviction of my own practical littleness

I remain | Yours very truly | Saml E. Phillips
Schoenbein Esq

My dear Schoenbein,

I do *not know* the person above – but he asks me to send the accompanying to you & I do so not for what they may be worth but as giving the [two words illegible][.]

Ever Yours | M. Faraday

1. Samuel Elkins Phillips (d.1893, age 79, GRO). Given as an electrician in 1861 census. TNA RG9/403, f. 46, p. 29.

2. August Wilhelm Hofmann (1818–1892, ODNB). Professor of Chemistry at the Royal College of Chemistry, 1845–1865.

Letter 3402**Faraday to Juliet Pollock****11 March 1858****From the original in RI MS F1 E7**

Royal Institution | 11 Mar 1858

My dear Mrs. Pollock,

I return with many thanks & carefully the pages you lent me. I have read them with great interest. It is wonderful to me to see the enlarged & meditative mind possessed by the writer in fields so different; it produces the conviction that in other fields the power must have been equally great. Her departure was a great loss to others but not to herself¹[.]

Ever Your Grateful Servant | M. Faraday

1. A reference to the death of Maria Julia Herries. See letters 3325 and 3376.

Letter 3403**Henry Richard Williams¹ to Faraday****13 March 1858****From the original press copy in TNA MT4/41, p. 1427**

13 Mar 1858

Sir,

to acquaint you that Her Majesty's Paymaster General has been requested to pay to you the sum of One hundred Pounds £100 being the amount of remuneration for services in Inspecting &c the Lighting Apparatus for Colonial Lighthouses, during the year ended 1st March Instant.

Enclosed is a form of receipt which upon being stamped and signed by you can be presented for payment at the Office of her Majesty's Paymaster General through your Banker[.]

I am | Sir | Your obedient Servant | H.R. Williams
Professor Faraday F.R.S. | &c &c &c | Albemarle Street

1. Henry Richard Williams (d.1884, age 85, *The Times*, 1 December 1884, p. 1, col. a). Accountant at the Board of Trade, 1853–1867, *Royal Kalendar*.

Letter 3404**John Tyndall to Faraday****19 March 1858****From the original in RI MS RI CG3/1/4**

19 Mar 1858

My dear Mr. Faraday,

The change recently made in the number of my lectures, and the manner in which it has been made¹, are both so gratifying to me, that I feel

very desirous to express to the Managers how highly I appreciate this proof of their confidence. Would you have the goodness to convey to them my warmest thanks for the consideration they have shewn me? I know not, of course, what the future may bring forth; but I cherish the hope that the alteration which has been made in such a kindly spirit, will eventually be found to harmonize both with the interests of the Institution [and] of Science².

Believe me | dear Mr. Faraday | most truly yours | John Tyndall

1. See letters 3397, 3398, 3399 and RI MM, 1 March 1858, 11: 220 when the number of Tyndall's lectures was reduced to twelve.

2. This letter was read to the managers. RI MM, 22 March 1858, 11: 222.

Letter 3405

William Snow Harris to Faraday

27 March 1858

From the original in IET MS SC 2

6 Windsor Villas, Plymouth | 27 Mar 1858

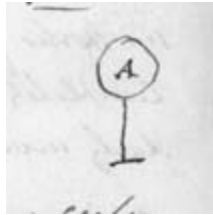
My dear Faraday,

You have always been so kind and considerate toward me that I have really very conscientious scruples, in venturing upon this communication which I fear must to some extent intrude on your valuable time and attention but the fact is I am now seriously engaged on what I hope may be at least a useful Work to be entitled "Statical and Dynamical Electricity"¹ and which will comprise I hope the existing state of Electrical Philosophy to the present time under an original form & containing something more than is constantly repeated over & over again in Works of this kind – I have now a very completely fitted apartment with Instruments for daily investigation – some good may be done in this way and future times will judge of the real merits of my Work. This also is much better than vanishing discussions of a controversial kind[.]

– Now it is most important to me to be assured that I fully & clearly comprehend all your advances in your immortal Work "Experimental Researches" which I am ever studying – it is really very difficult to say what questions bearing on Electrical force it does not treat.–

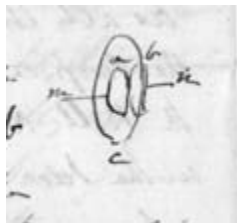
When you say 1177 "that it is impossible to charge a portion of matter with one Electrical force independently of the other"² I suppose you mean to say that the opposite force always appears somewhere – or is always called up: now I want to avoid collision with this view & the common notion that an insulated conductor may be electrified either

positively or negatively[.] When I throw sparks from the positive Conductor of the



Elec. Machine upon the Insulated metal Ball A may it not be said to remain in a certain sense charged with one of the Electrical forces? – can we say that the opposite force is at the same time also called up on this positively electrified Ball A?

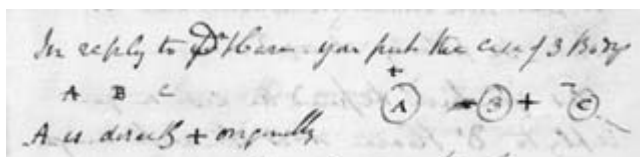
As all charge is necessarily determined to the Surface – it seems a delusion to talk of charging the Ball A itself at all. I believe you agree in the Idea that the Ball is the mere terminating or beginning of that peculiar condition of the Intervening dielectric medium external to it. – I should imagine that the depth of the charge whatever it be upon the surface of the Ball is infinitely small yet it has certainly possession of the metallic particles – and there is a stratum of dielectric particles also outside it charged with the same Electricity – this we see by Franklins³ Experiment with the Leyden Jar⁴ and moveable coatings moreover you can not easily deprive the Metal of this charge for if an Electrified Ball be whirled round in the Air with any Velocity by means of a silk line – still the charge remains – so I think the Metal must hold some of the Charge as Metal – and probably by attractive force for the Electrical agency[.] Then again we have the outer stratum acting sensibly through the medium around it. Thus if we apply metallic discs ab



to a dry Glass disc abc one on each side and charge a in the usual way b being for the time connected with the Earth – remove the charged discs ab by the insulators mn – each will be found Electrified – but one + the other

minus – now the glass disc itself shows the Electricity of the charged side a every where the charge left under disc a upon the glass acts freely upon an Electrometer through the Glass and is radiated as it were from the opposite surface – what should we say – supposing an objector were to remark upon the Theory that metal disc a charged positively ought to induce *negative* Electricity upon the Glass beneath it – and so we should find negative not positive Elec. upon the glass – In any new work on this subject (which is greatly wanted) the whole form of expression as to language requires to be altered and we must study to adapt description by approximate terms to the real state of things taken in all their generality[.] I should never for example talk of Electrical Fluids but rather of Electrical force – or of the charging of Metallic Balls or other Conductors with Electricity as if the whole mass were independently affected &c &c

In reply to Dr. Hare⁵ you put the case of 3 Bodys A B C⁶

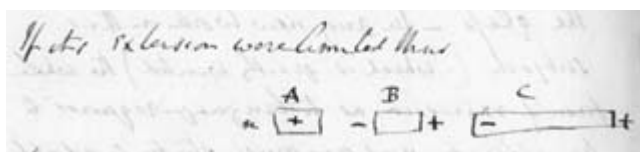


A is directly + originally

B + on one surface and – on the other

C – you say.

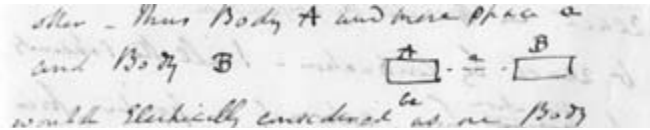
I think we must consider C as a polar charge also in a certain sense – If connected with the Ground the whole is but another Conductor and what may be called the positive force must exist in C at an infinite distance as If its extension were limited thus



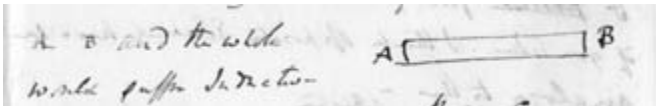
but the + of C is with unlimited extension a vanishing quantity[.]

So again the charge upon A is disturbed by reaction of B placing its two extremitys on somewhat dissimilar plates – we might have an arrangement such as to bring the distant force of n into a neutrality or even to pass that limit in which case A would be polarly charged.

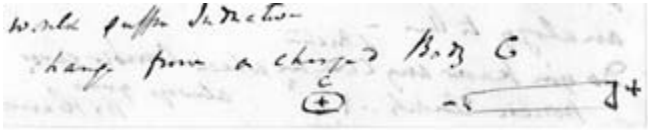
As you have defined the case in your reply to Dr. Hare it is all well enough. I had much correspondence with some French Philosophers relative to Conduction across a vacuum & your notion of considering forces only as mere force without taking into the account solid indivisible Atoms – you are aware that my little unpretending Work has been Translated into French by M. Garnault⁷ Professor of Physics à l'Ecole navale à Brest – under the Title of "Lecons Elementaire D'Electricite"⁸ – They have much difficulty in conceiving all this on the other side of the Water and I had hard work to explain to them what we were to really understand by your Theory[.] It may appear silly to many persons – but I can not see what mere space or distance has to do with the question. If two Bodys are so circumstanced as to have *nothing* between them – they must be considered for all practical purposes as touching each other. Thus Body A and mere space a and Body B



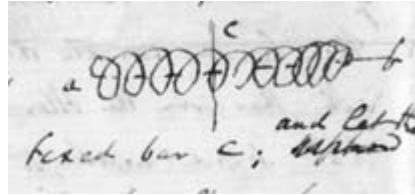
would Electrically considered be as one Body AB and the whole



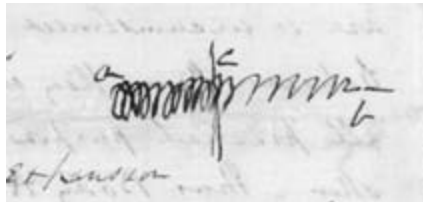
would suffer Inductive charge from a charged Body C



in each case alike.— With respect to positive & negative Electricity – I believe the real difference to consist in the operation of the same force in different ways. Take a Spiral Spring ab



at rest with a central fixed bar c; and let the ends ab be connected through the Spring by a cord ba – extend the cord at b in direction ab the result will be the extension of the Spring in direction cb and compression in ac thus



If we allow the Spring to react or operate by expansion b reacts by contraction – I call the extremity b negative force, extremity a positive force if you like. I think opposite Electricities have some analogy to this—

Do you know any case of friction in which Amber gives positive Electricity?

always yours | W.S. Harris

Michael Faraday Esq. LL.D. FRS &c

1. Faraday (1838a), ERE11.
2. Eventually published posthumously as Harris (1867).
3. Benjamin Franklin (1706–1790, ODNB). American natural philosopher.
4. See Heilbron (1979), 330–4.
5. Robert Hare (1781–1858, ANB). Professor of Chemistry in the University of Pennsylvania Medical School, 1818–1847.
6. Faraday (1840).
7. Eugène Garnault, otherwise unidentified.
8. Harris (1857).

Letter 3406**John Dickson¹ to Faraday****27 March 1858****From the original in IET MS SC 2**

Sir,

I would esteem it as a very great kindness if you would favor me with your opinion on the enclosed paper. "On the cause of Gravity". I have no apology to offer for the liberty I take, except a strong desire to obtain the opinion of some authority on part of a subject which has occupied the greater part of my leisure hours for many years, and a hope that the contents of the paper may prove not altogether uninteresting to you, as, according to it, the general attraction of matter for matter is a simple and almost obvious consequence of the doctrine of a conservation of force.

In the enclosed the centers of force are supposed to act on each other at a distance by supposing them close to each other another series of phenomena are obtained, as identical with Polarity, as those now described are with Gravity, and by these may be explained in a simple, and as I think in a common sense manner, the principal phenomena of Heat Light and Electricity_[.]

Again apologizing for my intrusion²_[.]

I very respectfully remain | Sir | Your Most Obedient Servant |
J. Dickson

Lea Terrace – Gainsborough | 27 March 1858

To Professor Faraday | London

1. John Dickson (b.c.1823, TNA RG9/2409, f. 11, p. 15). Customs collector.

2. For his extended view of the subject see Dickson (1859).

Letter 3407**Faraday to Ughtred James Kay-Shuttleworth¹****29 March 1858****From the original in the possession of D. Walker**

[Royal Institution embossed letterhead], Albemarle St. W |
29 Mar 1858

My dear Master Shuttleworth,

I am very glad that you escaped from serious injury on Saturday², and also thankful to yourself for letting me know assuredly of your escape. How soon in life we begin to learn that there are many deliverances we have to be thankful for which no amount of tender care in those around us could have provided; although they may make that care, & the love which causes it, abundantly manifest_[.]

I am My dear Master Shuttleworth | Very truly Yours | M. Faraday

1. Ughtred James Kay-Shuttleworth (1844–1939, WWW3). Later a politician.
2. That is 27 March 1858. Shuttleworth was injured by the explosion of glass apparatus being used in a lecture at the Royal Institution by the Professor of Practical Chemistry at King's College, London between 1856 and 1870, Charles Loudon Bloxam (1831–1887, ODNB). Faraday administered first aid to Shuttleworth in the Ante-room. See Shuttleworth to The Times, *The Times*, 31 August 1931, p. 6, col. f.

Letter 3408**Michael and Sarah Faraday to Mary Barnard****29 March 1858****From the original in the possession of Alison Giles**

[Royal Institution embossed letterhead], Albemarle St. W |
29 Mar 1858

My dear Mary,

We have heard of your thoughts and decision¹ – and in the strong persuasion that they are happy both at present and in their future results we long to rejoice with you. Such thoughts bring with them certain troublous excitements;– hopes never come without fears. But these are necessary during the transition of the young mind into a more mature and responsible state. May you be guided by him to whom you are about to commit yourself and far more by Him who knoweth all things, in such manner as to have cause to be thankful and also the thankful heart which acknowledges the many mercies poured upon us. Give our kindest remembrances and thoughts to Mr. Deacon and consider how earnestly we are moved upon the present occasion to express ourselves[.]

Your very affectionate uncle & Aunt | M.S. Faraday

1. This letter, which is entirely in Faraday's hand, refers to Barnard's engagement to William Matheson Deacon. They married on 4 October 1860. See letter 3866.

Letter 3409**Faraday to John Barlow****30 March 1858****From the original in Lone Star Autographs**

30 Mar 1858

My dear Barlow,

I hoped to have seen you this morning before I left for Brighton but have nothing particular to say[.] I have arranged all in my department & expect to be at home at the end of next week.

Ever Yours | M. Faraday

Letter 3410**Faraday to John Barlow****1 April 1858****From the original in RI MS F1 E8**[Royal Institution embossed letterhead],
118 King's Road, Brighton | 1 Apr 1858

My dear Barlow,

I have received the enclosed. It is from the Engraver Burnet¹ I think an associate of the Academy perhaps but my memory is sadly tenuous. I have written to tell him arrangements are all made as I believe but that I shall send his letter to you as the directing spirit. I do not suppose you will see any thing in the profer to take advantage of. Perhaps you would not mind dropping him a line – I think he has published a good work on art².

Ever Yours | M. Faraday

1. John Burnet (1784–1868, ODNB). Painter, engraver and writer on art.

2. Burnet had published a number of books on art.

Letter 3411**Henry William Pickersgill¹ to Faraday****1 April 1858****From the original in RI MS**

14 Stratford Place | 1 Apr 1858

To my old & much esteemed friend Ml. Faraday D.C.L. FRS. | &c &c &c

I address this Letter to you as the Head of the Royal Institution of Gt. Britain for the purpose of enabling the Managers in case of any thing happening to me before the return of the Pictures from the Exhibition at the Royal Academy to claim as their Property my Picture of the Revd. John Barlow² which I hope they will accept as a slight testimony in consideration for the great privilege [sic] I have long enjoyed through their liberality³.

I have the honor to be | My dear Dr. | Ever yours faithfully |
H.W. Pickersgill

PS The Picture can be seen any time before the 6 Inst.

1. Henry William Pickersgill (1782–1875, ODNB). Portrait painter.

2. This portrait of Barlow was exhibited at the Royal Academy in 1858. See Graves (1905–6), 6: 147.

3. For the background to this presentation see Prescott (2002), 65–6. This letter was read to the managers. RI MM, 5 April 1858, 11: 227.

Letter 3412

Faraday to Charles Vincent Walker¹

3 April 1858

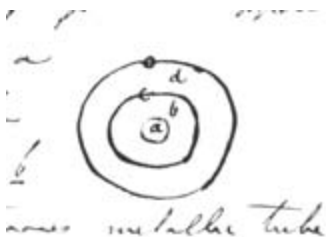
From the original in BL Bennet Woodcroft Collection, Faraday file

[Royal Institution embossed letterhead], Brighton |
3 Apr 1858

My dear Sir,

I have an impression that the specific inductive capacity of Gutta Percha is nearly 2 but I cannot remember the exact experiments:- as to Trinidad bitumen I cannot say any thing.

When an electric current is sent into a submerged insulated wire the effect may for the moment be considered as two fold. one part *dynamic* dependant on the current;- the other *static*, dependant on the momentary charge of the wire: Both may be resolved into one law of action acting *along & across* the wire, but with infinite degrees of difference in the two directions; the difference being as great as that of *insulation & conduction*. Your question refers to the *static* part i.e. the induction of the wire through the gutta percha towards the iron coating or towards the water outside. Now the true principles of static induction (as I have understood them & endeavoured to promulgate them for many years past) offer no advantage from the expedient you propose. Suppose the diagram to represent a



section of a telegraph cable *a* being the central wire,- *b* gutta percha,- *c* a continuous metallic tube- *d* gutta percha,- and *o* either the outer iron wire or the water. On sending a positive current through *a* it, for the moment, induces laterally across *b* *c* & *d* the action terminating at *o* where it raises up the negative state. If you attempt to charge *c* positive at the same time with *a* - indeed pos *a* will induce a *negative* state on the inside of *c* & a positive state on its outside; which, with that given to it purposely, will act with increased force towards *o* rendering its inside equivalently negative.- As regards pure static action you will see these principles stated in the last paper I sent you²; and I should expect as concerning *static* action, that *a* would be in a better state with *c* away i.e. replaced by gutta percha.

Then there come the dynamic inductions[.] If a momentary current be sent through either *a* or *c* it tends to send a reverse currents through the other; and therefore if like short currents were sent though *a* & *c* at once, they would affect each other.— You may think it worth while making experiments of this kind. Only I may observe, that, as the two states must be kept up throughout the whole length of the cable there seems no reason theoretically, why *a* & *c* should not coalesce that is be made into one central wire & all the electric current thrown into it.

I am My dear Sir | Very Truly Yours | M. Faraday
C.V. Walker Esqr | &c &c &c

1. Charles Vincent Walker (1812–1882, ODNB). Electrician.
2. Faraday (1858b), Friday Evening Discourse of 12 February 1858.

Letter 3413

Julius Plücker to Faraday

6 April 1858¹

From the original in IET MS SC 2

21 Norfolk Street. 21, Strand. London | 6 Apr

My dear Sir,

Not having found you in town I write to you viâ Royal Institution. I should happy to see you either in town or the country, if you would be so kind to give me any direction whatever.

Yours | Plücker | of Bonn

1. Dated on the basis that Faraday was in Brighton at this time and that letter 3414 is the reply. See also letter 3447.

Letter 3414

Faraday to Julius Plücker

7 April 1858¹

From the original in NRCC ISTI

Brighton | Evening, 7 Apr

My dear Plucker,

I shall return to London the day after tomorrow in the Evening and will meet you on Saturday² at any hour from 10 o'clk and either at Norfolk Street or the Institution or wherever you like. Will you let me find a note from you at the Royal Institution telling me what you wish me to do?

Ever Yours | M. Faraday

1. Dated on the basis that Faraday was at Brighton and that this is the reply to letter 3413. See also letter 3447.

2. That is 10 April 1858.

Letter 3415

Faraday to Joseph John Jenkins¹

10 April 1858

From the original in RI MS F1 A26

[Royal Institution embossed letterhead], Albemarle St. W |
10 Apr 1858

Sir,

I hasten to return my thanks for a card to view the Annual Exhibition²
on the 26th instant[.]

I have the honor to be | Sir | Your Very faithful Servant | M. Faraday
Jos J. Jenkins Esqr. | &c &c &c

1. Joseph John Jenkins (1811–1885, ODNB). Painter and Secretary of the Old Watercolour Society of Painters, 1854–1864.

2. Of the Society of Painters in Watercolours. See *Athenaeum*, 1 May 1858, pp. 567–8.

Letter 3416

Richard Owen to Faraday

12 April 1858

From the original in IET MS SC 2

British Museum | 12 Apr 1858

My dear Faraday,

I am induced to depart from a rule of not trespassing on such time
as yours, on account of the sterling merit, and excellent character of the
bearer, Mr. Julius Sax¹. Who is most industrious and ingenious in the man-
ufacture of philosophical instruments, & most docile in efforts to apply
his skill in carrying out any ideas suggested to him for new or special
ends, in such apparatus. Mr. J. Sax deserves to be known, as having these
qualities.

Forgive me this trespass, & believe me
sincerely your's | Richard Owen

1. Julius Sax (1826–1890, GRO). Russian-born telegraph engineer. See 1881 census return TNA RG11/325, f. 86, p. 29.

Letter 3417**Charles Beaumont Phipps to Faraday****19 April 1858****From the original in IET MS SC 2****Faraday number 1**

Buckingham Palace | 19 Apr 1858

My dear Professor Faraday,

By the death of Lady Wheatley¹ there has become vacant a small house belonging to The Queen, upon Hampton Court Green – (not in the Palace) and Her Majesty thinks that it might be agreeable to you, and advantageous to Your health to have a Suburban retreat, to which you could retire after the labour of your lectures, and quietly pursue your Scientific researches.

The Queen has therefore commanded me to say, that, not only appreciating the Services which, in your lectures, you have rendered to the Prince of Wales² – but the far more important benefit you have conferred upon the World of Science – it would give Her Majesty much pleasure to place this house at your disposal as a residence for the rest of your life – if it would be agreeable to you to live there.

It would be probably desirable that you should see the house before you return any Answer[.]

Sincerely Yours | C.B. Phipps

Endorsed by Faraday: Offer of the house by Coll. Phipps from the Queen

Address: Professor Faraday | Royal Institution

1. Louisa Wheatley, née Hawkins (d.1858, age 77, *Gent. Mag.*, 1858, 4: 568). Widow of Henry Wheatley (1777–1852, B3), army officer.

2. See letter 3071.

Letter 3418**Faraday to Ernst Becker****20 April 1858****From Bence Jones (1870a), 2: 398–9**

Albemarle Street, W | 20 Apr 1858

My dear Dr. Becker,

I believe you know all about the extreme kindness shown to me in respect of one of Her Majesty's houses at Hampton Court¹. I am in a little difficulty about either accepting or declining it. The manner in which it is

offered to me is such as would make it grievous to me to decline it, and yet, if it is not improper, I should like to have a few words with you before I finally settle. At what hour could I call to see you, and where?

Ever your obliged | M. Faraday

1. See letter 3417.

Letter 3419

Faraday to Charles Frederick Winslow

26 April 1858

**From a typescript in Nantucket Historical Association Research Library,
MS 166/3/3**

Royal Institution | London | 26 Apr 1858

Dear Sir,

I write at once to acknowledge your letter¹ and your paper² on the relations of the sun and the earth. I am very glad to see you occupied on the subject, and certainly have been very much surprised by your monthly table of earthquakes and volcanic eruptions. The relation of these mighty terrestrial phenomena to the distance at which our earth is placed from the sun at the time seems as if it could only be referred to the amount of heat received at the time from the sun or else to some function of the force of gravity varying as it does in accordance as to time with the phenomena³.

I sincerely wish you may establish your view i.e. may by further examination of the phenomena prove it to be the truth. We none of us want anything but the truth, but when the truth is in a new direction and aside from our preconceived notions we are often very slow to receive it. I should be very glad to find some effect of gravity that might be considered complementary to the variation of that force by change of distance but the thought is very coldly received here⁴. I see no reason to change my own impression but I should rejoice to find them receiving either expansion or correction by such views as yours.

You amuse me rather by your account of your interview with Sir Astley Cooper⁵, and I am glad you did not take his advice. If you have found a good wife you have found sources of happiness very different but quite equal to those which science could have given you and I trust far more abundant. I believe that the work God intended you for has been as well carried out as it would have been by any monkish devotion to science alone. It is in the power of man to perform duties of both kinds and I suspect his moral and religious nature will be best subserved by a wife and family.

I do not imagine that I shall hear from either Airy or Sabine on the subject of your paper. I doubt whether either are as yet favorable to such views as ours. If the views be truth it will require time for them to make their view. Still they are announced and I am persuaded will progress though probably not much in the present generation.

I am Sir | Your Very Obligated Servant | M. Faraday
Dr. C.F. Winslow

1. Letter 3400.
2. Winslow (1858).
3. For Faraday's continuing interest in Winslow's views see Faraday, *Diary*, 10 February 1859, 7: 15810.
4. See the anonymous and hostile review of Faraday (1857a), Friday Evening Discourse of 27 February 1857, in *Athenaeum*, 28 March 1857, pp. 397–9 by Augustus De Morgan (1806–1871, ODNB), Professor of Mathematics at University College, London.
5. Astley Paston Cooper (1768–1841, ODNB). Surgeon.

Letter 3420

Faraday to Charles Beaumont Phipps

27 April 1858

From the original copy in IET MS SC 2

Faraday number 3

Royal Institution | 27 Apr 1858

Sir,

It grieves me much that I did not acknowledge your letter of the 19th¹ immediately upon its arrival but I understood the last paragraph in it as expressing a wish that I shd not reply until I could give a decisive answer[.] I believe you have been made aware that circumstances oblige me to hesitate until I receive some promised information from the Lord Chamberlains² office. I hoped for this on Saturday³ & Yesterday:– but I cannot any longer delay the acknowledgment in some manner of your communication & the expression of my deep thanks for the kindness & consideration with which it was made[.]

I have the honor to be | Sir | Your Very faithful Servant | M. Faraday
Coll | The Honble. C.B. Phipps CB. | &c &c &c &c

Endorsed by Faraday: From me to Coll. Phipps

1. Letter 3417.
2. De La Warr.
3. That is 24 April 1858.

Letter 3421**Charles Beaumont Phipps to Faraday****3 May 1858****From the original in IET MS SC 2****Faraday number 5**

Buckingham Palace | 3 May 1858

My dear Professor Faraday,

The Queen has directed me to request that in your consideration as to the acceptance of the House at Hampton Court, you will put entirely on one side all thought of the expence of the repairs¹, as Her Majesty has made arrangements by which, should you take the house, it shall be put in to thorough repair inside and out – without any expence to you.

Sincerely Your's | C.B. Phipps

1. See letter 3420.

Letter 3422**Faraday to Margery Ann Reid****3 May 1858****From the original in the possession of Rosalind Brennand**

Royal Institution | 3 May 1858

My dear Maggie,

The case is settled. The Queen has desired me to dismiss all thoughts of the repairs as the house is to be put into thorough repair both inside and out. The letter from Sir C. Phipps¹ is most kind. I am about to answer it².

Ever your affectionate uncle | M. Faraday

Address: Miss Reid | 26 Claremont Square | Pentonville | N

1. Letter 3421.

2. Letter 3424.

Letter 3423**Faraday to Henry Bence Jones****3 May 1858****From the original in RI MS F1 E9**

[Royal Institution embossed letterhead] | Monday Evening

My dear friend,

I have just received a most kind note from Coll. Phipps¹ in which he says for the Queen that her Majesty will give orders for the thorough repair

of the house inside & out. I am about to accept^[1] in the terms which I think I ought to use^[1] this most gracious offer²^[1]

Ever Truly Yours | M. Faraday

Dr. B. Jones | &c &c &c

Endorsement: 3d May. 1858

1. Letter 3421.

2. Letter 3424.

Letter 3424

Faraday to Charles Beaumont Phipps

3 May 1858

From the original in RAW MS M54/100

[Royal Institution embossed letterhead], Albemarle St. | 3 May 1858

Sir,

I find it difficult to write my thanks or express my sense of the gratitude I owe to Her Majesty; first, for the extreme kindness which is offered to me in the use of the house at Hampton Court, but far more, for that condescension and consideration which, in respect of personal rest and health, was the moving cause of the offer. I feared that I might not be able properly to accept Her Majesty's most gracious favour. I could not bring myself to decline so honorable an offer;— and yet I was constrained carefully to consider, whether its acceptance was consistent with my own particular and peculiar circumstances. The enlargement of Her Majesty's favour¹ has removed all difficulty.— I accept with deep gratitude:— and I hope that you will help me to express fitly to Her Majesty my thanks and feelings on this occasion.

I have the honor to remain | Sir | Your very obliged & grateful
Servant | M. Faraday

Coll. | The Honble C.B. Phipps C.B. | &c &c &c &c

1. Letter 3421.

Letter 3425

Faraday to Ernst Becker

5 May 1858

From Bence Jones (1870a), 2: 399–400

Royal Institution | 5 May 1858

My dear Dr. Becker,

I had a most kind letter from Col. Phipps¹, who, speaking in the name of Her Majesty, removed all my difficulty, and so yesterday I could accept

the favour offered me². I dare say you know these things, but I felt as if I must either call or write to you on the occasion; and as I had troubled you too much already by calls, I take the latter course. I am surprised by the kindness I have received on this occasion, which, in the case of Her Majesty's unsought condescension, astonishes me. I know that your good wishes are with me in this matter, and they are of the greater value to me, as they are free and unsolicited – the spontaneous result of your own kind thought. Whilst enjoying Hampton for a year or two, as I hope to do, pleasant remembrances will be called up on every side.

Ever, my dear Dr. Becker, yours most truly | M. Faraday

1. Letter 3421.

2. Letter 3424.

Letter 3426

James South to Faraday

5 May 1858

From the original in IET MS SC 2

Observatory Kensington |
Wednesday, 5 May 1858 | 3h. P.M.

My Dear Faraday,

Ecstasied with Joy at the News which Your Letter of Yesterday¹ (this Moment only received) has almost overwhelmed me, I write this to thank You for the friendly act – In the rural domicile, which the Queen has done honor to herself by Giving You, and I hope Your wife in case she should survive you, May God “endue you plenteously with heavenly gifts” – “grant you in health” and happiness “long to live” – “strengthen you that you may vanquish and overcome” all the inconveniences of age – and “finally after this life, may you attain everlasting joy and felicity” with those to whom you have been attached in this world, never more to be separated from them in that which is to come!²

Remember me most kindly to Mrs. Faraday and believe me that no one living feels a deeper interest in the health and happiness of you both, than does

My Dear Faraday | Yours most affectionately | J. South

1. Not found.

2. All these quotations come from ‘A Prayer for the Queen’s Majesty’ of the *Book of Common Prayer* with ‘her’ replaced by ‘you’.

Letter 3427**Peter Henry Berthon to Faraday****5 May 1858****From the original in GL MS 30108/2/80**

Trinity House, London, EC |

5 May 1858

Sir,

I am directed by the Elder Brethren to forward to you the enclosed copy of a Letter¹ from Mr. Cope² of Frankfort O.M. to Sir John Pakington³, relative to the improvement of the effect of Light by the use of Jets of Steam thrown thereon, and I am to request you will give the subject your consideration and favor the Elder Brethren by stating your opinion as to the applicability of Steam to the purpose suggested by Mr. Cope.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq.– | &c &c &c | Royal Institution

1. Cope to Pakington, 20 April 1858, GL MS 30108/2/80.

2. Unidentified.

3. John Somerset Pakington (1799–1880, ODNB). Conservative MP for Droitwich, 1837–1874.

Letter 3428**Peter Henry Berthon to Faraday****5 May 1858****From the original in GL MS 30108/2/82**

Trinity House, London, EC |

5 May 1858

Sir,

The Board's attention having been directed to the alleged superiority of the Red Light recently exhibited by the French Authorities, in the Malakoff Light House at the Entrance of the River Garonne, over other Lights of that colour, – I am directed to request you will favor the Elder Brethren by stating whether any recent improvements have come under your observation whereby Lights of the colour alluded to, have been rendered more effective, and visible at a greater distance than they have hitherto been considered capable of being.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq | &c &c &c | Royal Institution

Letter 3429**David Brewster to Faraday****6 May 1858****From the original in IET MS SC 2**

6 May 1858

My dear Mr. Faraday

The enclosed letter will explain the reason of my writing you. It is from a distinguished advocate Prof of Scots Law in the university of Edinr, Mr. Shank More¹, who takes the deepest interest in the honour of the University of Edinr.

We have got Adams² to gild our ancient university. If Edinr could allure you from the Metropolis³, I need not say how we shall be delighted. I am,

My Dear Mr. Faraday | Ever Most Truly Yours | D. Brewster
St. Leonards College, St. Andrews

1. John Shank More (1784–1861, B2). Professor of Scots Law, University of Edinburgh, 1843–1861.

2. John Couch Adams (1819–1892, ODNB). Astronomer and Fellow of Pembroke College, Cambridge, 1853 to 1892. He was also Professor of Mathematics at the University of St. Andrews in 1858, a position he held for a single session only.

3. This is the suggestion that Faraday should be appointed Professor of Chemistry at the University of Edinburgh in succession to William Gregory (1803–1858, ODNB) who had died on 24 April 1858, having held the position since 1844. Faraday's decline of this offer was noted in *The Times*, 20 May 1858, p. 12, col. d and *Lit. Gaz.*, 22 May 1858, p. 497. Lyon Playfair was appointed.

Letter 3430**Faraday to Peter Henry Berthon****7 May 1858****From the original copy in GL MS 30108/2/80**

Royal Institution | 7 May 1858

Sir,

I have given much consideration to the letter from Mr. Cope¹ to Sir J. Pakington² (dated 20 April 1858³). I cannot find out its meaning i.e. I cannot find even a crude idea of the manner in which steam is to be employed in association with a strong light for the service of lighthouses. That steam applied to flame lowers the intensity of the light, and that it much disturbs & ultimately extinguishes it is within my knowledge: but I am not aware of any principle or method by which it can be applied with advantage to increase its light⁴[.]

I am | Sir | Your Very faithful Servant | M. Faraday
P.H. Berthon Esq | &c &c &c | Trinity House

1. Unidentified.

2. John Somerset Pakington (1799–1880, ODNB). Conservative MP for Droitwich, 1837–1874.

3. Cope to Pakington, 20 April 1858, GL MS 30108/2/80, sent with letter 3427.

4. This letter was read to Trinity House By Board, 11 May 1858, GL MS 30010/41, pp. 391–2. It was agreed to send a copy to the Board of Trade endorsing Faraday's opinion.

Letter 3431

Ernst Becker to Faraday

7 May 1858

From the original in IET MS SC 2

Buckingham Palace | 7 May 1858

My dear Mr. Faraday,

On Monday morning¹ I heard that the question as to the repairs of the house had been decided on. My first idea was, to run to Albemarle Street to tell you,— I could not, my duties kept me here, then I thought of sending you a line, but for an hour or more I could not find a single moment & then I knew that Sir Charles Phipps's letter must have reached you meanwhile.— On Tuesday, I heard that you had accepted the Queen's offer; again, I felt, as if I *must* come to you to tell you how glad I was, & hear from you that you have really in accepting the offer, considered it to be what Her Majesty intended it to be, viz: a boon to you & *only* a boon;— but again I could not find time. Then I received your very kind letter², which has given me more pleasure than you can imagine— and again I had to let two days pass before I was able to answer it. My consolation is: your very letter shows me that I need not be afraid of your taking my silence for want of interest.—

And now, as you have decided, let me express the hope, that when the first time of inconvenience & trouble is over, you will find that an occasional stay at Hampton will as much refresh your mind & improve Mrs. Faraday's health, as, I am sure, it will suit Miss Barnard's taste.

I hope I shall be able to see you soon; meanwhile believe me

Yours truly | E. Becker

1. That is 3 May 1858.

2. Letter 3425.

Letter 3432

Faraday to Peter Henry Berthon

8 May 1858

From the original copy in GL MS 30108/2/82

Royal Institution | 8 May 1858

Sir,

In reference to your letter of the 5th instant¹ regarding the red light at the entrance of the river Garonne I have received no account of that light nor of its superiority over others. Not being aware of its construction either

as regards the source of the light or the means employed to render the ray, red;— having indeed no data upon which to found any consideration, — I am unable to form an opinion respecting it relative to other red lights or indeed of the light itself².

I am Sir | Your very faithful Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c | Trinity House

1. Letter 3428.

2. This letter was read to Trinity House By Board, 18 May 1858, GL MS 30010/41, pp. 401–2. The minute suggests that the letter Faraday sent underwent considerable revision from this text. It was agreed to write to the French lighthouse board for further details.

Letter 3433

Faraday to an unidentified Royal Academician

10 May 1858

From Bence Jones (1870a), 2: 406–7

Royal Institution | 10 May 1858

My dear Friend,

I am puzzled how to answer your note of the 8th, and our meeting in the evening¹ only adds to my difficulty. The much occupation I have here, the continual delay of the pursuit of my own researches (a delay now extending over two years), and the weariness of head and health resulting from this continued occupation, had made me resolve never to sit again.

There are two or three who claim the first right if I break through the determination, but they are content with such an answer. Nevertheless, if I sit to you now, having done so very frequently, on two former separate occasions, I think they will have a just right to complain. I think you said that I had promised this to you. I do not remember any distinct promise, but as you have understood it so, I will endeavour to arrange for six sittings; I trust that they will be sufficient to complete the last picture (which was left off very suddenly), and then, if I have health and strength, I must go to my research. I think, as far as I can see, that Tuesday mornings, early, would suit my arrangements.

I understood you to say on Saturday², that it was for my sake that you desired to paint another picture. Notwithstanding the high compliment which this implies, all the reasons having relation to me are against it. I should give offence to others, whom I esteem most highly. I want my time, if well enough, for thought and research; exhausting as these are to me, I want time for rest and health. Twice before have I for long periods together been a burden to your genius; and I have arrived at such a time of life as to be no longer vain of what may well be considered as a distinction. I am quite prepared to do what I have said for *your sake*, but find no motive in any circumstance that is connected with *my own*.

Ever yours faithfully | M. Faraday

1. Presumably at the soirée of the Royal Society at Burlington House. See *The Times*, 10 May 1858, p. 12, col. e.
2. That is 8 May 1858.

Letter 3434**Peter Henry Berthon to Faraday****10 May 1858****From the original in GL MS 30108/2/82**

Trinity House | 10 May 1858

My dear Sir,

Your reply¹ to my Enquiry respecting improvements in the Exhibition of Lights *Coloured red*, being limited to your unacquaintance with the “Malakoff” Light only,— leads me to think that I did not make myself understood in my official Letter², and that you took my enquiry as applying to that Light House only.—

The opinion the Board desire is of a mere general Character and I am therefore to trouble you again to ask whether any improvements have come under your observation in the means or appliances for rendering Lights coloured red, by shades or otherwise more powerful and visible at a greater distance than those of this colour in use at this Corporations Light Houses.

Believe me to be | My dear Sir | Yours Very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. Letter 3432.

2. Letter 3428.

Letter 3435**Arthur-Auguste De La Rive to Faraday****10 May 1858¹****From the original in IET MS SC 2**

Mon cher & excellent ami,

Marcel² qui arrive de Londres me dit que vous n’avez pas réussi à reproduire mon expérience de la rotation de la lumière électrique; or, comme à mes yeux cette expérience a une grande importance soit en elle même, soit pour la théorie de l’Aurore Boréale & que je viens de la faire à Berlin avec le plus grand succès, je vous envoie sous forme de lettre, quelques détails à ce sujet que vous me feriez un vrai plaisir, si cela est possible, de faire insérer dans le *Phil. Mag*³. J’y tiens d’autant plus que Mr. Plucker à l’air de s’attribuer le mérite d’avoir trouvé le premier l’influence du magnétisme sur les décharges, tandis que je l’avais déjà fait connaître en 1849 dans ma lettre à M. Regnault⁴.—

Au reste ce n'est pas la première fois que Mr. Plucker laisse quelque chose à désirer dans ses rapports avec les autres savants; on le sait bien à Berlin & demandez le à Tyndall & à vous même.

Je vous écrirai plus en long incessamment, j'ai fait un petit voyage des plus intéressants en Allemagne & à Berlin; j'espère que vous êtes bien ainsi que Madame Faraday; mes meilleures amitiés à M. Tyndall &c. – M'autorisez-vous à vous appeler dans ma lettre: *Mon cher ami*? Ce titre m'est bien doux, comme vous le savez.

Marcet m'a parlé des belles expériences qu'il a vues à Londres avec les tubes de Geissler; je les ai aussi vues & répétées à Berlin soit chez Riess, soit chez Dove⁵ avec qui j'ai analysé la lumière de ces décharges au moyen de puissan[te] lumière qui présente une disposition particulière des raies du spectre. C'est un sujet bien curieux & intéressant dont le mérite revient plus à Mr. Geissler qui a eu l'idée de faire ces tubes & qui y a réussi, qu'à Mr. Plucker.

Vous me trouverez bien mauvais, vous qui êtes toujours si bon, à l'endroit de Mr. Plucker, mais cela vient de ce que j'ai entendu Marcet. Il semblait que le physicien de Bonn avait tout fait dans ce sujet. – Permettez-moi de vous prier de demander en grâce à Mr. Gasiott en lui présentant mes compliments les plus affectueux, de répéter mon expérience sur une grande échelle, en prenant un fort électro-aimant & une puissante machine de Ruhmkorff, montée avec 3 ou 4 couples de Grove ou de Breuget⁶ [sic]; je suis sur qu'il sera récompensé de sa peine, car cette rotation est bien belle à voir.

Votre tout devoue & affe. | A. de la Rive

Je ne vous ai pas remercié, je crois, de votre bonne lettre du 26 mars⁷ & de la notice qu'elle renfermait qui m'a fort intéressé. – Vous savez combien vos lettres me font toujours plaisir. –

TRANSLATION

My dear and excellent friend,

Marcet² who arrives from London tells me that you have not succeeded in reproducing my experiment on the rotation of electric light; now, since in my opinion, this experiment is of great importance both in itself, and for the theory of the Aurora Borealis, & I have just done it in Berlin with the greatest success, I am sending you in the form of a letter, some details on this subject which it would give me great pleasure, if it were at all possible, if you could insert in the *Philosophical Magazine*³. It is particularly important for me as Mr. Plücker gives the impression of attributing to himself the merit of being the first to find the influence of magnetism on discharges, whilst I had already made this known in 1849 in my letter in Mr. Regnault⁴. – Besides, it is not the first time that Mr. Plücker leaves something to be desired in his dealings with other savants; it is well known in Berlin and ask Tyndall and yourself.

I shall write at length in due course, I made a short and most interesting journey to Germany & Berlin; I hope that you are well and also Mrs. Faraday;

my best regards to Mr. Tyndall &c. – Will you allow me to call you *My dear friend?* in my letter. This title is very dear to me, as you know.

Marcet told me of the beautiful experiments that he saw in London with Geissler's tubes; I also saw them & repeated them in Berlin both with Reiss and with Dove⁵; with the latter I analyzed the light of these discharges using a powerful light which presented a particular arrangement of the line spectra. It is a most curious & interesting subject for which the merit is due more to Mr. Geissler, who had the idea of making these tubes & who succeeded, than to Mr. Plücker.

You will find me wicked, you who are always so good, concerning Mr. Plücker, but this comes from what Marcet heard. It seemed as if the physicist of Bonn had done everything on this subject. – Permit me to ask you kindly to beg Mr. Gassiot, presenting to him my most affectionate compliments, to repeat my experiment on a grand scale, using a strong electromagnet & a powerful Rühmkorff machine, powered with 3 or 4 Grove or Breguet⁶ cells; I am sure that he will be rewarded for his trouble, because this rotation is very beautiful to see.

Your devoted and affectionate | A. de la Rive
I did not thank you, I believe, for your kind letter of 26 March⁷ & for the note that was enclosed, which interested me greatly. – You know how much pleasure your letters always give me.

Address: Monsieur Faraday | Associé Etranger de le Institut de | France &c
&c | Royal Institution | Albemarle St. | Londres

1. Dated on the basis of the postmark and that it follows on from the text of a letter of the same date (mentioned in the first paragraph) as intended for translation and publication in the *Philosophical Magazine* which was done as De La Rive (1858).

2. François Marcet (1803–1883, *Ann. Reg.*, 1883, p. 142). Anglo-Swiss man of science.

3. De La Rive (1858).

4. De La Rive (1849).

5. Heinrich Wilhelm Dove (1803–1879, DSB). Professor of Physics at Berlin.

6. Louis François Clément Breguet, (1804–1883, DSB). French instrument maker.

7. Not found.

Letter 3436

Faraday to Maria Drummond¹

11 May 1858

From the original in APS Misc MS Collection

Royal Institution | 11 May 1858

My dear Mrs. Drummond,

The temptations you offered me and the kindness in which they were wrapped up were such that I made up my mind to be with you this evening; – but I cannot I really am not well enough and my appearance would discredit

both you & me. What can I say but thanks to you for inviting me & more thanks for the forgiveness you will extend to me[.] With kindest thoughts of you & yours believe me to be

Very faithfully Your Servant | M. Faraday

1. Maria Drummond, née Kinnauld (1810–1891, P[aul] (1891)). Widow of the Under Secretary in Ireland, 1835–1840, Thomas Drummond (1797–1840, ODNB). She was a member of the Royal Institution from 1848.

Letter 3437

Faraday to Peter Henry Berthon

11 May 1858

From the original copy in GL MS 30108/2/82

Royal Institution | 11 May 1858

My dear Sir,

No improvements or propositions for improvements of the red lights have recently come under my observation¹. – I am not likely to receive such subjects except through you – On the 11 July 1857, I reported on a proposition by Capt. Denham² to employ red glass chimneys instead of the usual plates of red glass³ – On the 17 March 1843 I reported on some proposed combinations of red & yellow glass and also respecting the Ruby glass⁴ – On the 26 Decr. 1845, I reported on a proposition to line the silver reflectors with red glass⁵. – On another occasion of which I cannot recall the date I reported to the Trinity board on a proposition to use red serge in place of red glass – In none of these cases was the light improved – The use of the ruby glass being excepted – by the plans & suggestions brought forward[.]

I am My dear Sir | Very truly Yours | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. See letter 3434.

2. Henry Mangles Denham (1800–1887, B1). Royal Navy captain and Younger Brother of Trinity House, 1841–1887.

3. Not found.

4. Faraday to Herbert, 17 March 1843, letter 1479, volume 3.

5. Faraday to Herbert, 26 December 1845, letter 1810, volume 3.

Letter 3438

Faraday to George Biddell Airy

c.14 May 1858¹

From the original in RGO6/169, f. 260

My dear Sir,

Mr. Barlow asked me about glass silvering for you. I could not remember at the time who had taken up Pettitjeans [sic]² Patent³, but have gradually remembered that it was Swinburnes (house) RW & Co. of 93 Upper

Thames Street⁴. I do not think they could silver solid forms of glass so as to give you good reflecting surfaces, but as to concave & convex surface I send you two watch glass [sic]. They are bad now & were done in haste long ago. Do not trouble yourself to return them_[.]

Ever faithfully Yours | M. Faraday
The Astronomer Royal | &c &c &c
I send the glasses in a separate letter MF

1. Dated on the basis that letter 3439 is the reply.
2. Tony Petitjean (d.1873, age 49, GRO under Petitjeax). Invented a process for silvering glass.
3. Petitjean was granted patent 1855-1681 for his process for silvering glass on 24 July 1855.
4. R.W. Swinburne & Co Plate glass manufacturers of Red Bull Wharf, 93 Upper Thames Street. POD. See letter 3230.

Letter 3439

George Biddell Airy to Faraday

15 May 1858

From the original press copy in RGO6/169, f. 261

15 May 1858

My dear Sir,

I am much obliged by your sending me the specimens of silver watch glasses¹. I suppose that this process has something in common with that about which I was inquiring, but it is not in all respects the same. As I understand, in the process applied to telescopes, in order to form a *concave* mirror (for instance) the silver is deposited on the *concave* side of the glass. It is, as I understand, pure metallic silver; and its polish is given, not by the polish of the glass, but by subsequently rubbing the exposed silver.

I am happy to hear that you have received a suburban location which pleases you.

I am, my dear Sir | Yours very truly | G.B. Airy
Professor Faraday | &c &c &c

1. See letter 3438.

Letter 3440

Spencer Cecil Brabazon Ponsenby¹ to Faraday

19 May 1858

From the original in IET MS SC 2/3/7

Faraday number 8

19 May 1858

Dear Sir,

I have received Instructions to make out a warrant for you for the House at Hampton Court given to you by the Queen. I find on looking into the matter

that there is a pew in the Chapel in the Palace which has hitherto belonged to the House and I would be obliged by your informing me whether you would wish to use it, as in that case I would ascertain, if it is intended that it should be included in your warrant².

Faithfully yours | Spencer Ponsenby

Endorsed by Faraday: From Mr. Ponsenby about *Chapel Pews*

1. Spencer Cecil Brabazon Ponsenby (1824–1915, *Ann. Reg.*, 1915, p. 175). Gentleman Usher to the Queen and Comptroller of Accounts in the Lord Chamberlain's Department.
2. Letter 3457.

Letter 3441

Arthur-Auguste De La Rive to Faraday

19 May 1858

From the original in IET MS SC 2

Genève le | 19 Mai 1858

Monsieur & très cher ami,

Si par hasard la lettre que je vous ai adressée, il y a quelques jours¹, n'a pas encore parue dans le *Phil. Mag.*, auriez-vous l'extrême bonté d'y faire ajouter le *post-scriptum* ci joint; vous m'obligeriez infiniment. (Voyez la 3ème page)².

J'ai répété ces jours derniers l'expérience dont je vous ai parlé & j'espère ne pas tarder à publier les résultats assez curieux que j'ai observés, dans un mémoire spécial.

J'ai eu un peu de remords de vous avoir parlé, comme je l'ai fait, de M. Plucker; j'étais peut-être encore un peu trop sous l'impression de ce que j'en avais entendu dire; j'espère que vous m'avez pardonné.–

J'aimerais bien aller vous faire une visite cet été; mais cela m'est impossible; il faut remettre ce plaisir à une autre année, si Dieu me prête vie. Si vous saviez tout le plaisir que j'aurais à vous voir, à vous serrer la main, vous comprendriez mes regrets de ne pouvoir réaliser cette année mon désir. D'autant plus que nous devenons vieux & que nous n'avons pas de temps à perdre; nous n'avons probablement pas jusqu'aux 89 ans comme l'illustre Humboldt que j'ai trouvé encore avec toutes ses facultés & son entrain d'autrefois.– Mais il lui manque malheureusement toujours quelque chose que la grâce de Dieu pourrait seule lui donner & dont l'absence est même plus pénible à son âge–

Ma femme me charge de ses compliments les plus affectueux; elle ne vous oublie point pas plus que votre tout dévoué & affectueux

A. de la Rive

TRANSLATION

Geneva | 19 May 1858

Sir and very dear friend,

If by chance the letter that I addressed you a few days ago¹, has not yet appeared in *Philosophical Magazine*, would you have the extreme kindness to add to it the attached *post-script*; you would oblige me infinitely. (See the third page)².

I have repeated in these last few days the experiment of which I spoke to you and I hope not to delay publishing the curious results that I observed, in a special paper.

I felt slight remorse to have spoken of Plücker to you as I did; I was perhaps still a little too much under the impression of what I had heard said; I hope that you have forgiven me.—

I should have liked to visit you this summer; but it is impossible; it is necessary to postpone this pleasure to another year, if God grants me life. If you knew how much pleasure seeing you and shaking you by the hand would give me, you would understand my regret at not being able to achieve my desire this year. All the more as we become old & do not have of time to lose; we probably do not have eighty nine years like the illustrious Humboldt, whom I once again found with all his faculties and liveliness intact.— But he still unfortunately lacks something which only God's grace can give him & of which the absence is more painful at his age.

My wife asks me to send her most affectionate compliments; she does not forget you any more than your devoted & affectionate

A. de la Rive

1. Letter 3435.

2. This post-script was added to De La Rive (1858), 466.

Letter 3442

John Percy to Faraday

20 May 1858¹

From Bence Jones (1870a), 2: 394

My dear Dr. Faraday,

Forgive my presumption in addressing a few lines to you respecting the interview which has just taken place². One man can speak with more freedom than three men. I wish I could adequately convey to you the earnest desire which prevails as to your acceptance of the office of President of the Royal Society. Your acceptance of it would be conducive to the best interests of science, and establish an important principle in reference to the society for which many have, as you know, so long and arduously struggled Forgive, I again say, my presumption. That after deliberation you may decide upon a yes, is the earnest prayer of,

Yours sincerely, with the highest respect | John Percy



Plate 9. Royal Society delegation to Michael Faraday, 20 May 1858, offering him the nomination of the presidency of the Royal Society.

1. Dated on the basis of the reference to the presidency of the Royal Society.
2. This was a deputation (depicted in Plate 9) consisting of the President of the Royal Society, from 1854 to 1858, the astronomer John Wrottesley (1798–1867, ODNB), Grove and Gassiot to ask if Faraday would agree to be nominated for the presidency of the Royal Society. For the discussion at the Royal Society Council (of which Percy was a member) surrounding this, which occurred earlier in the day, see White (1898), 117–8.

Letter 3443

Faraday to John Percy

21 May 1858¹

From Bence Jones (1870a), 2: 395

Royal Institution | 21 May 1857 [sic]

My dear Percy,

Your letter is very kind and earnest, and I thank you heartily for it, but I may not change my conclusion. None can know but myself how unfit it would be².

Ever affectionately yours | M. Faraday

1. Dated on the basis that this is the reply to letter 3442.
2. That is Faraday declined to be nominated as President of the Royal Society. Tyndall recollected Faraday expressing a similar sentiment at greater length and detail. Tyndall (1868), 266–7.

Letter 3444

William Benjamin Carpenter¹ to Faraday

21 May 1858

From the original in RI MS Conybeare Album, f. 23

University of London, Burlington House, W | 21 May 1858

Dear Professor Faraday,

I enclose you some proofs of Evidence for Sir C. Lyell, which I cannot venture to forward to him myself, from fear of the awful consequences of disobedience to Mr. Warburton's² dictum³.

I am much wanting Dr. Tyndall's corrections⁴; having had Dr. Miller's⁵ & Professor Graham's⁶ a week since[.]

Believe me | yours faithfully | William B. Carpenter
Professor Faraday

1. William Benjamin Carpenter (1813–1885, ODNB). Registrar of University College, London, 1856–1879.
2. Henry Warburton (1785–1858, ODNB). Philosophical radical.
3. This refers to the collection of evidence for the establishment of science degrees in the University of London. A committee, of which Faraday and Warburton were members, was established

by the Senate on 14 April 1858 to consider the matter. Lyell's evidence was given on 25 May 1858. *Report of the Committee appointed to consider the propriety of establishing a degree or degrees in Science*, London, 1858, pp. 73–85.

4. Tyndall, *Diary*, 27 April 1858, 7: 336. Tyndall's evidence is in *Report*, pp. 1–9.

5. William Allen Miller (1817–1870, ODNB). Professor of Chemistry at King's College, London, 1845–1870. His evidence, given on 27 April 1858, is in *ibid.*, pp. 10–15.

6. Graham's evidence, also given on 27 April 1858, is in *ibid.*, pp. 16–19.

Letter 3445

Faraday to Arthur-Auguste De La Rive

24 May 1858

From the original in BPUG MS 2361, f. 87–8

Royal Institution | 24 May 1858

My dear De la Rive,

I have received both yours¹; and sent them both to the Philosophical Magazine² to have the parts which are *intended* for publication published. I have no doubt they will appear in the next Number. I began to translate them; but they were taken from me that they might be done at once. I gave Tyndall your message who returns his own remembrances. The tubes which Gassiot & I worked³ with were those of *his own construction* not those of Geissler. Plucker brought some of Geisslers to London & shewed us some effects⁴. I have not, & cannot read, Plucker's note⁵; but I did not understand from him that he claimed any merit in the observations of the ordinary effect of a magnet over the electric discharge through air, but for a special effect which occurs at the Negative metallic termination. When that (the discharge) takes place in a globe there is a diffused light in the globe or part of the globe, having its seat on the negative wire:– not the brighter; light – but another feebler one – When this is held between the strong poles of a very large & powerful magnet, they being about an inch apart,– all that kind of light collects itself into a plate, leaving the other parts of the globe *dark*.– Further, this plate has the Negative wire *as a base or section* & in fact is formed upon it; the breadth of the plate of light is formed by the length of the wire left exposed in the vacuum;– the thickness of the plate is coincident with the thickness of the wire; & the length of the plate is given by the globe, for its ends abut suddenly against the glass. This plate of light is always coincident with the *lines of magnetic force* and makes them visible as iron filings render them visible;– with this restriction, that no light is visible except for *those lines of force which pass through the negative wire* i.e. though the discharging part of the negative wire for if half the wire be coated with an insulator no light lines of magnetic force pass though the coated part.

Plucker has shewn me this phenomenon not to its full extent but very decidedly & I understand that, that, was what he claimed.

I have not your letters at present; but I can understand your feelings about Plucker. Do not think that they surprize me. I profess to be a peacemaker & therefore say little about such matters, unless circumstances call for it:– but scient[if]ic morality is not altogether satisfactory.

Your kind expressions my dear friend delight me. I speak to very few friends (and to no other philosopher) as I do to you:– and why? because I have a trust in you in respect of matters beyond science. My kindest thoughts & remembrances (as also my wife's) to Madame De la Rive. Others, still very kind, as they ought to be, to Your Son⁶, Marcet⁷, & other friends.

Ever My dear De la Rive | Yours | M. Faraday

1. Letters 3435 and 3441.

2. De La Rive (1858).

3. See Faraday, *Diary*, 23, 27, 30 January, 3, 20 27 February, 5, 13, 18 March 1858, 7, pp. 412–51.

4. In April 1858. See letter 3447.

5. De La Rive (1858), 464 referred to Plücker (1858a).

6. William De La Rive (1827–1900, Choisy (1947), 51). Swiss politician and writer.

7. François Marcet (1803–1883, *Ann. Reg.*, 1883, p. 142). Anglo-Swiss man of science.

Letter 3446

Faraday to Lyon Playfair

25 May 1858

From Reid, T.W. (1899), 176

Royal Institution | 25 May 1858

My dear Playfair,

That such a question should arise! I cannot give – i.e. volunteer – a certificate for circulation; it is against my rule; but if any one asks me whether you are able to expound the truths of experimental science to a large number of persons in a clear, logical, audible, and, to me, satisfactory manner, I should have no need to consider, but must from my own experience say “Yes” at once. Anyone concerned in the matter may write to me, or you may show them this note, which contains what would be my answer¹.

Ever truly yours | M. Faraday

1. Playfair needed testimonials as part of the process for his application to be Professor of Chemistry at the University of Edinburgh.

Letter 3447

Julius Plücker to Faraday

28 May 1858

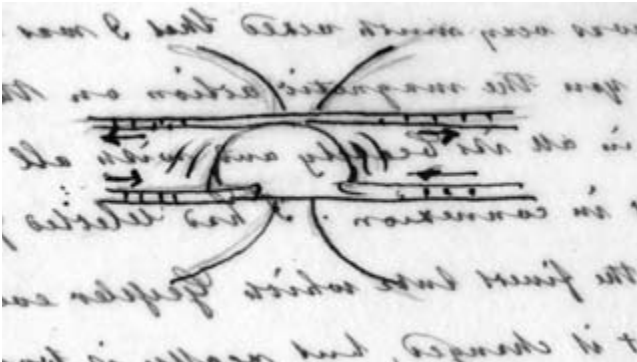
From the original in IET MS SC 2

Bonn | 28 May 1858

My dear Sir,

At first my most sincere thank for all your kindness to me during my last stay in London¹. I never will forget the hearty reception I received, without any exception.

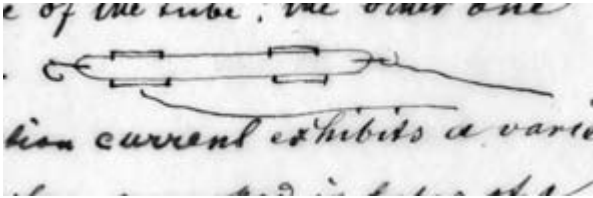
I was very much vexed that I was not able to show you the magnetic action on the electric current in all its beauty and with all the singular facts in connexion. I had selected for this purpose the finest tube which Geissler ever made; I thought it changed, but really it was broken by Geissler when put into the box and exchanged by an ordinary one of a similar shape. Thus I was able only to give a general idea of the curious phenomenon.



Being returned home I was most anxious to examine the *induction* current within the tube, I am indebted for to Mr. Gassiot's kindness. There is indeed between the two tin covers a double current of *equal intensity* and opposite direction, as indicated already by Mr. Gassiot's experiment. When put equatorially between the two iron pieces one of the two currents is thrown upwards, the other one downwards; over a single pole the two currents, separated by the Magnet, are deflected within the horizontal plane. When put axially upon the two iron pieces, the two currents, obliged by the Magnet to *cross* each other, present a beautiful phenomenon difficult to be reproduced by a drawing. When the tube is put in an oblique direction upon the iron pieces I got two *separated* bright spirals, the one dextrorsum, the other

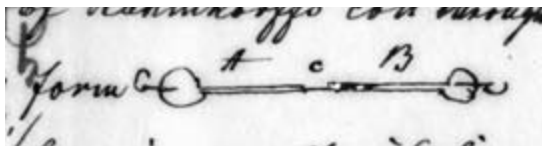
one sinistrorsum. In some distance from the Magnet (when the action on both spirals is less different) we get two similar spirals; the lower one is interrupted, when the tube touches the iron pieces.

It was quite unexpected to me, to get essentially the same double current, when I connected one of the wires of Ruhmkorff's coil with the platina wire of the tube, the other one with the tin cover.



The double induction current exhibits a variety of phenomena when provoked in tubes of a different shape and containing traces of different gazes.—

After closer examination of the Torricellian vacuum, made with the greatest care, I am persuaded now that our best vacuum does not at all transmit an electric current and therefore not offer a luminous phenomenon. The double induction current seems to be even than transmitted when, for want of *sufficient* ponderable matter within the tube, the ordinary current between the platina wires is stopped. (Perhaps the double induction current may be transmitted by traces of ponderable matter deposited in the interior surface of the tube). I used tubes of the shape of Mr. Gassiot's. In one instance the double induction current having passed though the tube for a short time with a constant intensity, I interrupted the current, and connected the two wires of Ruhmkorff's coil to the two platina wires in the extremities of the tube; I was struck to get a very bright ordinary electric discharge which did not exist before; but, after some time the discharge became an interrupted one and finally entirely stopped. This experiment may be repeated as often as you like. In order to verify the general law, according to which to the same colour corresponds the same refrangibility of rays, in the case of electric spectra produced in different most dilated gazes, I conducted the discharge of Ruhmkorff's coil though a double tube of this form.



The two narrow tubes A, B, forming one straight line were connected to a platina wire c. One of the tubes contained traces of carbonic acid, the other one of hydrogene. Whatever might be the difference between the two spectra, the deflexion of the same colour in both spectra was the same. The strange appearances of the spectra are partly produced by contrast; I think also that some effect is to be attributed to fluorescent light.

There is, I think, no doubt that the blue colour in English tubes, corresponding to the green one in ours, is produced by imixed lead. I procured German glass, containing lead: tubes made of it gave a most intensive blue colour.



Lately I got a new case of magnetic light. Putting a tube of the shape A, in the equatorial position, with one of its narrow parts between the two poles, its larger middle part touching laterally the two iron pieces, I observed in the horizontal plane a fine magnetic luminous arch (a) completely seperated from the electric current. The two extremities of the arch touched the interior surface of the glass, as the magnetic arches do, which pass through the negative electrode. But in the new case the arch in the midst of the tube *is not at all directed by any conducting wire.*

You would oblige me very much, Sir, by communicating my observations, as you did in a former case, to Mr. Gassiot and also to Mr. Tyndall,

who kindly intends to give an abstract² of my papers, (the third³ did not yet appear) in the Philosophical Magazine.

I beg you, dear Sir, to present my respects to Mad. Faraday, and to Rvd. John Barlow, the amiable Secretary of Royal Institution.

Ever yours | Plücker

1. In April 1858. See letters 3413 and 3414.

2. Plücker (1858b) translated the entire paper; there was no abstract.

3. Plücker (1858c).

Letter 3448

Faraday to Angela Georgina Burdett Coutts

2 June 1858

From the original in BL Burdett-Coutts papers

[Royal Institution embossed letterhead],

Albemarle St. W | 2 Jun 1858

Dear Miss Coutts,

Your offer is very kind & I thank you heartily for it¹. I know the difficulty of getting tickets near the reading day¹. Unfortunately I cannot avail myself of it for my wife is out of town & my niece & I expect to be listening to Grisi². With thanks on all our parts I am

Very Truly Yours | M. Faraday

Address: Miss Coutts | Holly Lodge | Highgate

1. This probably refers to a reading from his own work by the journalist and novelist Charles John Huffam Dickens (1812–1870, ODNB). Dickens, who was close to Burdett Coutts, often read to the public in these years. For his reading on 3 June 1858 at St. Martin's Hall and the black market created by the popularity of his readings see *The Times*, 2 June 1858, p. 1, col. d.

2. Giulia Grisi (1811–1869, GDM). Italian soprano, who sang in many performances in London at this time. On 3 June 1858 she sang at Covent Garden in *Lucrezia Borgia* by the Italian composer Gaetano Donizetti (1797–1848, GDM). *The Times*, 1 June 1858, p. 8, col. f.

Letter 3449

Faraday to R. Lane¹

3 June 1858

From the original in RI MS F1 A27

[Royal Institution embossed letterhead],

Albemarle St. W | 3 Jun 1858

My dear Sir,

Remembering your request for F.E tickets I send you two for the 11th instant². I am not able to inclose more for my privilege is both limited & strained.

Ever Truly Yours | M. Faraday

R. Lane Esqr | &c &c &c

1. Unidentified.
2. Faraday (1858c), Friday Evening Discourse of 11 June 1858.

Letter 3450**Charles Wheatstone to Faraday****4 June 1858****From the original in IET MS SC 2**

Lower Mall, Hammersmith | 4 Jun 1858

Dear Faraday,

I shall have all my telegraphs at work tomorrow (Saturday) from 12 to 3, and I shall be glad to see you if you think it necessary to take another lesson. I will bring you on Monday the rough drafts of my provisional specifications and some other papers which will enable you to make notes for your lecture¹; and if you can spare an hour after the meeting we will go over them together. I will also bring a dial for experiments with the electric light. It may be as well for you to read the description of my dial telegraph as it existed in 1840 published in the 2nd edition of Daniell's Chemical Philosophy p. 578².

Yours very truly | C. Wheatstone

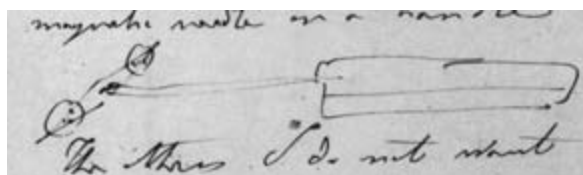
1. Faraday (1858c), Friday Evening Discourse of 11 June 1858.
2. Daniell (1843), 578.

Letter 3451**Faraday to John Zephaniah Bell¹****9 June 1858****From the original in SI D MS 554 A**

R Institution | 9 Jun 1858

My dear Sir,

Could you oblige me with the piece of apparatus, a magnetic needle on a handle



The others I do not want until you have done with them².

Very Truly Yours | M. Faraday

J.Z Bell Esqr | &c &c &c

1. John Zephaniah Bell (1794–1883, AIKL). Scottish artist living in London with strong Sandemanian connections. Cantor (1991), 51.
2. This was for Faraday (1858c), Friday Evening Discourse of 11 June 1858. Bell is mentioned in Faraday's notes RI MS F4 G54.

Letter 3452**Faraday to Henry Cole¹****10 June 1858****From the original in SI D MS 554 A**

[Royal Institution embossed letterhead] | 10 Jun 1858

My dear Sir,

I am much obliged by your note of the 7 May² – but finding that I can make an illustration that will serve my purpose better even than a Jaquard loom I will not trespass on your kindness for tomorrow³[.]

Ever Truly Yours | M. Faraday

H. Cole Esqr | &c &c &c

1. Henry Cole (1808–1882, ODNB). Secretary of the Department of Science and Art.
2. Not found.
3. Faraday (1858c), Friday Evening Discourse of 11 June 1858.

Letter 3453**Faraday to Edward William Cooke¹****15 June 1858****From the original in the possession of Kurt Wolfgang Vincentz**[Royal Institution embossed letterhead], Albemarle St. W |
15 Jun 1858

My dear Mr. Cooke,

You know generally how I am obliged to shut myself out of Society[.] I often grieve that it should be so. For Wednesdays I have no power to break a rule even though the temptations be very strong – for I am engaged in the City every Wednesday Afternoon & Evening² – The most sincere thanks for your kind invitation[.]

Ever Truly Yours | M. Faraday

1. Edward William Cooke (1811–1880, ODNB). Painter.
2. At the Sandemanian Church. See Cantor (1991), 65.

Letter 3454**Peter Henry Berthon to Faraday****18 June 1858****From the original in GL MS 30108/2/81**

Trinity House | 18 Jun 1858

My dear Sir,

A Committee of the Elder Brethren who have recently visited the Bishop Rock Light House have been much disappointed at the very queer appearance of the Glass of the Lantern put in by Mr. Wilkins, a specimen of which I send herewith, – it was order[e]d of an unusual thickness $\frac{7}{8}$ ths of an Inch, on account of the exposed position of the Tower, but the Board were assured that this circumstance would not interfere with its purity or Colour, this being the Case, the Deputy Master¹ who is much annoyed about it, has desired me to send the Specimen to you with a request that you will favor him by saying whether the glass is as free from colour as is usual or practicable, & whether you consider that its use will in any degree impair the brilliancy or power of the Light when seen through it, or the distance at which it will be visible.

Believe me to be | My dear Sir | Yours very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. John Shepherd.

Letter 3455**Faraday to Frederick Gye¹****19 June 1858****From the original in RI MS F1 A28**

[Royal Institution embossed letterhead], Albemarle St. W |
19 Jun 1858

My dear Sir,

My thanks for last night – stalls are equal to my enjoyment & both were & are great. I am happy to say that my wife was able to go – which I hardly expect again[.]

Ever Truly Yours | M. Faraday
F. Gye Esqr | &c &c &c

1. Frederick Gye (1810–1878, ODNB). Manager of Covent Garden Opera, 1848–1878.

Letter 3456**Faraday to Peter Henry Berthon****21 June 1858****From the original copy in GL MS 30108/2/81**

Royal Institution | 21 Jun 1858

My dear Sir,

I have examined the block of glass sent to me with your letter of the 18th¹ and compared it in respect of colour with numerous other specimens of plate glass. I find it to come out exceedingly well. It will not compare with flint glass nor should that be expected. When a thick block like that you sent me is looked at in the ordinary way much of the light which traverses its length or breadth comes to the eye. These parts are here 5 & 3 inches & the effect is as if glass 5 or 3 inches in thickness were looked through[.] I have in the present case blocked that out & the Deputy master² will now see the colour belonging to a thickness of $\frac{7}{8}$ of an inch³[.]

I am My dear Sir | Yours Very Truly | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letter 3454.

2. John Shepherd.

3. This letter was read to Trinity House By Board, 29 June 1858, GL MS 30010/41, p. 456.

Letter 3457**Fifth Earl De La Warr to Faraday****21 June 1858****From the original in IET MS SC 2/3/7****Faraday number 9**

These are to Certify that in Obedience to Her Majesty's Command I have placed Michael Faraday Esqr. D.C.L. F.R.S. in Possession of the House on Hampton Court Green, late in the occupation of Lady Wheatley¹; deceased, together with the Coach Houses Green House, *Summer House* and other outbuildings, thereunto belonging[.]

Given under my Hand and Seal this 21st day of June 1858 In the Twenty Second year of Her Majesty's Reign.

De La Warr. | Lord Chamberlain

21st June 1858 | Michael Faraday Esq | D.C.L. – F.R.S. Warrant for the House on Hampton Court Green lately occupied by Lady Wheatley, deceased.

Envelope endorsed by Faraday: Hampton Court 12 & 4? Wanting

1. Louisa Wheatley, née Hawkins (d.1858, age 77, *Gent. Mag.*, 1858, 4: 568). Widow of Henry Wheatley (1777–1852, B3), army officer.

Letter 3458**Faraday to Maria Sarah Hooker¹****22 June 1858****From the original in Hargrett Library, University of Georgia, MS 2153/5/58**

[Royal Institution embossed letterhead], Albemarle St. W |

22 Jun 1858

My dear Lady Hooker,

Your note was very kind & gave us great pleasure[.] We both thank you heartily for it. The Queen's gift was made in a most gracious & considerate manner but as to when we shall be able to enjoy it I do not know for there are no signs of the repairs &c being taken in hand by the Board of Works as yet. I trust your visit to Scotland will be consoling to others & in many points a pleasure to yourself – the power of helping other gives pleasure to a mind properly constituted[.] When I saw Sir William² the other day he looked so well that I think he will be able to spare you[.] My wife joins me in kindest remembrances to him & to you[.]

Believe me to be | My dear Lady Hooker | Your Most faithful
Servant | M. Faraday

1. Maria Sarah Hooker, née Turner (1797–1872, ODNB under W.J. Hooker). Married W.J. Hooker in 1815.

2. William Jackson Hooker (1785–1865, ODNB). Botanist and Director of the Royal Botanic Gardens, Kew, 1841–1865.

Letter 3459**Circular from Lord Chamberlains's¹ Office****23 June 1858****From the original in IET MS SC 2/3/7****Faraday number 10**

Circular

Lord Chamberlain's Office | 23 Jun 1858

In order to prevent the possible recurrence of misunderstanding on the part of those Persons occupying Apartments in Hampton Court Palace or detached Buildings in the neighbourhood The Lord Chamberlain feels it to be his Duty to point out that the Grant is limited to those named in the Warrants, and is strictly Personal to them respectively, and that, in no case, can the Expenditure of Money upon any Apartment in the Palace or adjacent Buildings be admitted as constituting any thing like a claim either for pecuniary remuneration or for the benefit of Survivorship to the Relations of any Person, to whom a Warrant for Apartments may have been issued by The Lord Chamberlain.

Endorsed by Faraday: Circular from Lord Chamberlain's Office

1. De La Warr.

Letter 3460**Peter Henry Berthon to Faraday****23 June 1858****From the original in GL MS 30108/2/84**

Trinity House, London | 23 Jun 1858

Dear Sir,

A New Catadioptric Light apparatus of the 1st Order having been provided by Messrs. Wilkins & Co. for the Bishop Rock Light (Scilly) I beg to apprise you that the same is now ready for Inspection at their premises in Long Acre, and I am to request that you will favor the Elder Brethren by making an examination thereof & furnishing me with a Report of the result, for their information.— They will be obliged by as early an Inspection being made as may accord with your convenience.—

I am | Dear Sir | Your's faithfully | P.H. Berthon
M. Faraday Esq D.C.L. | &c &c &c

Letter 3461**Peter Henry Berthon to Faraday****25 June 1858****From the original in GL MS 30108/2/83**

Trinity House | 25 Jun 1858

My dear Sir,

The Deputy Master¹ will be obliged by your letting me know what reply you think should be given to Messrs Chance on the question put in their enclosed note,— i.e. whether we should require the Whitby Apparatus to be put up for your inspection *here* or at Birmingham[.]

Pray return the enclosed[.]

Believe me | My dear Sir | Yours very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. John Shepherd.

Letter 3462**Christian Friedrich Schoenbein to Faraday****25 June 1858****From Schoenbein (1858c)**

Bâsle | 25 Jun 1858

My dear Faraday,

These last six months I have been rather busily working on oxygen, and flatter myself not to have quite in vain maltreated my favourite; for I think I can now prove the correctness of that old idea of mine, according

to which there are two kinds or allotropic modifications of active oxygen, standing to each other in the relation of + to -, i.e. that there is a positively-active and a negatively-active oxygen, - an ozone and an antozone, which on being brought together neutralize each other into common or inactive oxygen, according to the equation $\oplus + \ominus = \text{O}$.

The space allotted to a letter being so small, I cannot enter into the details of my late researches, and must confine myself to some general statements, which I hope, however, will give you a clear notion of the nature of my recent doings. Having written a paper on the subject, that will before long be published in the transactions of the Academy of Munich¹, I shall not fail to send it to you as soon as possible.

Ozonized oxygen, as produced from common oxygen by the electrical spark or phosphorus, is identical with that contained in a number of oxy-compounds, the principal ones of which are the oxides of the precious metals, the peroxides of manganese, lead, cobalt, nickel and bismuth, - permanganic, chromic and vanadic acids; and even the peroxides of iron and copper may be numbered amongst them.

The whole of the oxygen of the oxides of the precious metals exists in the ozonic state, whilst in the rest of the oxy-compounds named, only part of their oxygen is in that condition. I call that oxygen negatively-active, or ozone *par excellence*, and give it the sign \ominus , on account of its electromotive bearing. Though generally disinclined to coin new terms, I think it convenient to denominate the whole class of the oxy-compounds containing \ominus "ozonides". There is another less numerous series of oxy-compounds in which part of their oxygen exists in an opposite active state, i.e. as \oplus or antozone, wherefore I have christened them "antozonides". This class is composed of the peroxides of hydrogen, barium, strontium, and the rest of the alkaline metals; and on this occasion I must not omit to add, that what I have hitherto called ozonized oil of turpentine, aether, &c., contain their active oxygen in the \oplus state, and belong therefore to the class of the "antozonides".

Now, on bringing together (under proper circumstances) any ozonide with any antozonide, reciprocal catalysis results, the \ominus of the one and the \oplus of the other neutralizing each other into O, which, as such, cannot be retained by the substances with which it had been previously associated in the \ominus or \oplus condition. The proximate cause of the mutual catalysis of so many oxy-compounds depends therefore upon the opposite states of the active oxygen contained in those compounds.

I will now give you some details on the subject.

1. Free ozonized oxygen = \ominus , and peroxide of hydrogen = $\text{HO} + \oplus$, or peroxide of barium = $\text{BaO} + \oplus$ (the latter suspended in water), on being shaken together destroy each other, $\text{HO} + \oplus$ or $\text{BaO} + \oplus$ being reduced to HO or BaO, and \oplus and \ominus transformed into O.

2. Aqueous permanganic acid = $\text{Mn}^2\text{O}^2 + 5\ominus$, or a solution of permanganate of potash mixed with some dilute nitric acid, is almost instantaneously discoloured by peroxide of hydrogen or peroxide of barium, the nitrate of the protoxide of manganese being formed in the first case, and in the second, besides this salt, the nitrate of baryta. It is hardly necessary to state, that in both cases the \ominus of the permanganic acid and the \oplus of the peroxides of hydrogen or barium are disengaged as O.

3. An aqueous solution of chromic acid containing some nitric or sulphuric acid and peroxide of hydrogen, are rapidly transformed into the nitrate or sulphate of oxide of chromium, HO, and inactive oxygen, which is of course disengaged. A solution of chromic acid mixed with some nitric acid and BaO^2 gives a similar result, nitrate of baryta and oxide of chromium being formed, and O disengaged.

4. If you add to a mixture of any peroxide salt of iron and the red ferro-sesquicyanuret of potassium (both substances dissolved in water) some peroxide of hydrogen, prussian blue will be thrown down and inactive oxygen set free. On introducing into a mixture of nitrate of peroxide of iron and the ferro-sesquicyanuret of potassium the peroxide of barium, a similar reaction takes place, prussian blue, hydrate of baryta, &c. being formed, and inactive oxygen eliminated. From these facts it appears that, under certain conditions, even peroxide of iron and HO^2 or BaO^2 are capable of catalyzing each other into FeO and HO, or BaO and O.

5. Under certain circumstances PbO^2 or MnO^2 are soluble in strong acetic acid, as you may see in one of my papers joined to this letter; now if you add to such a solution HO^2 or BaO^2 , the peroxides will be reduced to HO or BaO, and PbO or MnO, inactive oxygen being disengaged.

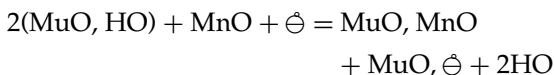
6. It is a well known fact that the oxide of silver = $\text{Ag}\ominus$, or the peroxide of that metal $\text{Ag}\oplus^2$, and the peroxide of hydrogen = $\text{HO} + \oplus$, catalyze each other into metallic silver, water and inactive oxygen. Other ozonides, such as $\text{PbO} + \ominus$ or $\text{MnO} + \ominus$, on being brought in contact with $\text{HO} + \oplus$, are transformed into PbO or MnO, HO and O. Now the peroxide of barium = $\text{BaO} + \oplus$, acts like $\text{HO} + \oplus$. If you pour water an intimate mixture of AgO, or AgO^2 and BaO^2 , a lively disengagement of inactive oxygen will ensue, AgO, AgO^2 and BaO^2 being reduced to metallic silver and baryta. In concluding the first part of my letter, I must not omit to state the general fact, that the oxygen disengaged in all cases of reciprocal catalysis of oxy-compounds, behaves in every respect like inactive oxygen.

There is another set of chemical phenomena, in my opinion, closely connected with the polar states of the active oxygen contained in the two opposite classes of peroxides. You know that a certain number of oxy-compounds, for instance the peroxides of manganese, lead, nickel, cobalt, bismuth, silver, and also permanganic, chromic, and vanadic acids, furnish with muriatic acid chlorine, whilst another set, such as the peroxides of barium, strontium, potassium &c., are not capable of eliminating chlorine

either out of the said acid or any other chloride. This second class of oxy-compounds produces, however, with muriatic acid, the peroxide of hydrogen; and it is quite impossible in any way to obtain from the first class of the peroxides HO^2 , or from the second chlorine.

You are aware that, from reasons of analogy, I do not believe in the doctrine of chlorine, bromine, &c. being simple bodies, but consider those substances as oxy-compounds, analogous to the peroxides of manganese, lead, &c., in other terms, as "ozonides". Chlorine is therefore to me the peroxide of murium = $\text{MuO} + \ominus$, hydrochloric acid = $\text{MuO} + \text{HO}$, and, as already mentioned, the peroxide of barium = $\text{BaO} + \oplus$, that of hydrogen = $\text{HO} + \oplus$, and the peroxide of manganese = $\text{MnO} + \ominus$. Proceeding from these suppositions, it is very easy to account for the different way in which the two sets of peroxides are acted upon by muriatic acid.

From reasons as yet entirely unknown to us, HO can be chemically associated only with \oplus , and with no other modification of oxygen, to constitute what is called the peroxide of hydrogen; and in a similar way MuO (the hypothetically anhydrous muriatic acid of older times) is capable of being united only to \ominus to form the so-called chlorine, which I denominate peroxide of murium. If we cause $\text{MuO} + \text{HO}$ to react upon $\text{BaO} + \oplus$, MuO unites with BaO , and HO with \oplus ; but if you bring together $\text{MuO} + \text{HO}$ with $\text{Mn} + \ominus$, part of MuO is associated to MnO , another part to \ominus , water being eliminated, according to the equation



As you will easily perceive, from these views it would follow that, under proper circumstances, two opposite peroxides, on being intimately and in the right proportion mixed together and acted upon by muriatic acid, could yield neither chlorine nor peroxide of hydrogen, but mere inactive oxygen. If somewhat dilute muriatic acid be poured upon an intimate mixture of five parts of peroxide of barium and two parts of peroxide of manganese, the whole will be rapidly transformed into the muriates of baryta and protoxide of manganese, the active oxygen of both the peroxides being disengaged in the inactive condition, and not a trace of free chlorine making its appearance. The same result is obtained from dilute hydrobromic acid.

Another consequence of my hypothesis is this: that an intimate and correctly proportioned mixture of two opposite peroxides, such as the peroxide of barium and that of lead, on being acted upon by any oxy-acid, cannot produce the peroxide of hydrogen; or, to express the same thing in other terms, muriatic acid must act upon the said mixture exactly in the same way as the oxy-acids do; and that indeed is the case. Mixtures of the peroxides just mentioned and acetic or nitric acids, are readily converted into

the acetates or nitrates of baryta and protoxide of manganese, the active oxygen of both the peroxides being of course disengaged in the inactive condition.

Before I close my long story I must mention one fact more, which, in my opinion, is certainly a very curious one. If you mix an aqueous and concentrated solution of bromine with a sufficient quantity of peroxide of hydrogen, what happens? A very lively disengagement of inactive oxygen takes place, the colour and the odour of the bromine solution disappear, the liquid becomes sour, and on adding some aqueous chlorine to it, bromine reappears. From hence we are allowed to conclude, that, on bringing bromine in contact with peroxide of hydrogen, some so-called hydrobromic acid is produced. The hypothesis at present prevailing cannot account for the formation of that acid otherwise than by admitting that bromine takes up the hydrogen of HO^2 , eliminating the two equivalents of oxygen united to H. I, of course, take another view of the case, bromine is to me an ozonide like peroxide of lead, &c., i.e. the peroxide of bromium = $\text{BrO} + \ominus$. Now $\text{HO} + \oplus$ and $\text{BrO} + \ominus$ catalyze each other into HO , BrO , and inactive oxygen, $\text{BrO} + \text{HO}$ forming hydrobromic acid, or what might more properly be called hydrate of bromiatic acid.

You see that I am growing more and more hardened in my heretical notions, or to speak more correctly, in my orthodox views; for it was Davy who acted the part of a heretic in overthrowing the old, venerable, true creed². Indeed the longer I compare the new and old doctrine on the nature of chlorine, &c. with the whole material of chemical facts bearing upon them, the less I am able to conceive how Davy could so lightly and slightly handle the heavy weight of analogies which, in my opinion, speak so very strongly and decisively in favour of Berthollet's³ views⁴. There is no doubt Sir Humphry was a man of great genius, and consequently very imaginative; but I am almost inclined to believe that, by a certain wantonness, or by dint of that transcendent faculty of his mind, he was seduced to conjure up a theory intended to be as much out of the way and "*invraisemblable*" as possible, and serve nevertheless certain theoretical purposes; and certainly, if he entertained the intention of solving such a problem, he has wonderfully succeeded. But what I still more wonder at is both the sudden and general success which that far-fetched and strained hypothesis met with, and the tenacity with which the whole chemical world has been sticking to it ever since its imaginative author pleased to divulge it: and all this could happen in spite of the fact that the new doctrine, in removing from the field of chemistry a couple of hypothetical bodies, was, for analogy's sake, forced to introduce fictitious compounds, not by dozens only, but by hundreds, – the oxy-sulphion, oxy-nitron, and the rest of those "nonentia". But enough of this subject, upon which I am apt to grow warm and even angry. Although the results I have obtained from my recent investigations cannot but induce me to begin another, and, I am afraid,

endless series of researches, I shall for the present cut short the matter and indulge for some time in absolute idleness.

I am, my dear Faraday | Yours most truly | C.F. Schönbein

1. Schoenbein (1858e).
2. See Knight (1992), 86–7.
3. Claude Louis Berthollet (1748–1822, DSB). French chemist.
4. See Knight (1992), 81–2, 87.

Letter 3463

Faraday to Benjamin Cheverton

26 June 1858

From the original in Royal Botanic Gardens, Kew, Hooker Collection

Misc. Letters, f. 82

[Royal Institution embossed letterhead], R Institution |
26 Jun 1858

My dear Sir,

I have no practice in telegraphy and may not be able to answer your questions – I cannot give you any opinion to be used only a private one – that I shall be happy to do if you think it of any value – & I will keep your secret[.]

Ever Truly Yours | M. Faraday

Benjn. Cheverton Esqr

Letter 3464

James Timmins Chance to Faraday

28 June 1858

From the original in GL MS 30108/2/83

Birmingham | 28 Jun 1858

My dear Sir,

The two Lights for the Trinity Board will be ready on Wednesday next; & we shall be glad to see you next *Thursday*¹ at any hour in the day-time at which you can conveniently arrive, as well as in the Evening, or towards night.

Mr. R.L. Chance purposes writing to you² to offer you hospitalities while you are in this neighbourhood.

I will enclose *tomorrow* a memorandum of the railway times.

Believe me | Yours most truly | James T: Chance

Professor Faraday | &c &c | London

1. That is 1 July 1858.
2. Letter 3465.

Letter 3465**Robert Lucas Chance to Faraday****28 June 1858****From the original in GL MS 30108/2/83**

Summerfield House near Birmingham | 28 Jun 1858

My dear Sir,

If you come by the train which leaves Euston Square at Ten on Thursday morning next the 1st of July I will meet you at 1.40 PM at the Birmm. Station in New St. & (DV) drive you over to Summerfield, & after luncheon we will go over to the works to see the Lighthouse apparatus—.

We have return'd your letter to Mr. Berthon & named the time to him – Mrs. Chance¹ unites with me in respectful Compts to Mrs. Faraday & kind regards to yourself &c

I am My dear Sir | Yours very faithfully | R.L. Chance
M. Faraday Esq | Royal Instn | Albermarle St.

1. Louisa Chance (d.1873, age 86, GRO). Wife of R.L. Chance.

Letter 3466**Julius Plücker to Faraday****28 June 1858****From the original in IET MS SC 2**

Bonn | 28 Jun 1858

Dear Sir!

I got the last number of the philosophical Magazine with de la Rive's letter¹, who characterizes my experiments as his own under a different form. Allow me a few words. Davy's² and de la Rive's beautifull experiments are inscribed into the annals of science. In both cases the free luminous arch is substituted to the moveable copper wire, to which in your original experiments the current is bound. In that sense de la Rive's experiment of 1849³ is that of "Faraday's pendulum"⁴. I published a series of experiments of the same kind which in like manner emanate immediately from yours and may be easily predicted.

But there is a second series of experiments, which I described in my first paper⁵, of quite a different description. Such is the rupture of the current too. A copper-wire cant change its form under the influence of the Magnet, an electric beam, starting from a fixed point, can. The new class of phenomena show this change of form of the luminous current in a most splendid way. I am enabled now to explain the connection of these phenomena with the experiments you made about thirty [sic] years ago and particularly with the mathematical law deduced from them by M. Biot and Laplace⁶.

The form which a flexible current assumes when in equilibrium under the influence of the Magnet is given by the following laws.

1°. When the current is free only to move on a given surface (that arrives, for instance, in the case of the double helix in Gassiot's induction tube) each moveable element of the current, acted upon by the magnet, is to be, by this action, compelled perpendicularly against the given surface: therefore the line perpendicular to the element and the line of force passing through it, must be a normal to the surface.

2°. When the flexible current, starting from a fixed point, is free to move in any direction whatever, each of its elements must be directed thus, that there is on it no action not at all emanating from the Magnet; id est it must be directed along a line of magnetic force. Whence you may immediately conclude that the whole current, when in equilibrium, is bent along the line of magnetic force passing through the fixed point.

I hope, my dear Sir, you will be satisfied by the simplicity of these new laws.

Most truly yours | Plücker

PS. Since my last letter⁷ I observed a great variety of new facts. I am preparing a fourth paper⁸. My third one is printed since a month⁹ but I could not yet obtain the copies. With regard to the two spirals, I mentioned in my last letter, by a "lapsus pennae" I said one "dextrorsum the other one sinistrorsum". But both are either dextrorsum or sinistrorsum (independently of the direction of the primitive current of Ruhmkorff's coil and of the polarity of the Magnet) the tube in its oblique position being inclined either +45° or -45° to the equatorial plane.

1. De La Rive (1858).

2. Davy (1821), 427.

3. De La Rive (1849).

4. That is Faraday's discovery of electro-magnetic rotations. Faraday (1821) which was referred to in Plücker (1858d), 622.

5. Plücker (1858a, b).

6. Pierre-Simon, Marquis de Laplace (1749–1827, DSB). French physicist. See Plücker (1858d), 622 and Faraday (1838b), ERE12, 1318–1429. For the relation of Plücker's work to Faraday's see James (1983b), 153–6.

7. Letter 3447.

8. Plücker (1858d).

9. Plücker (1858c).

Letter 3467

Christian Friedrich Schoenbein to Faraday

28 June 1858

From the original in UB MS NS 436

Bâsle | 28 Jun 1858

My dear Faraday,

I take the liberty to introduce to you Professor Vischer¹ of Bâsle, an intimate friend and Colle[a]gue of mine, who intends to make a stay at London

for some time and is kind enough as to take charge of a parcel containing voluminous letters, scientific papers and something else destined for the Sovereign of the Royal Institution. It will perhaps interest you to learn on this occasion, that my friend, being an excellent greek scholar, acted the part of a god-father, when I christened my Child "Ozone" 19 years ago.

Mr. Vischer does, of course, not meddle in any way with Chemistry, but is in every other respect a true "savant", whose personal acquaintance, I trust, will afford you much pleasure.

I am my dear Faraday | Your's most sincerely | C.F. Schoenbein
Dr. M. Faraday | &c &c &c

1. Wilhelm Vischer (1808–1874, ADB). Professor of Greek at Basle.

Letter 3468

Peter Henry Berthon to Faraday

29 June 1858

From the original in GL MS 30108/2/83

Trinity House | 29 Jun 1858

My dear Sir,

I presume you have heard from Messrs Chance appointing Thursday Evening¹ for the trial of the Whitby Apparatus. I write you a few lines to say that a Committee of the Elder Brethren have been appointed to accompany you in the Examination, – they intend leaving for Birmingham by the 1.50 PM Train on Thursday and will meet you at Messrs Chances at any time you may appoint.

Believe me | My dear Sir | Yours very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. That is 1 July 1858. See letters 3464 and 3465.

Letter 3469

Robert Lucas Chance to Faraday

30 June 1858

From the original in GL MS 30108/2/83

Summerfield House near Birmingham | 30 Jun 1858

My dear Sir,

I shall (DV) be at the New St. Station at 1-40 PM tomorrow.

This morning we had advice from Mr. Berthon that a Committee of the Trinity directors will come with you by the train leaving Paddington at 1-50 PM and arriving at Spon Hill at 5.30 PM, of which you have, no doubt, received advice¹.

I shall, no doubt, receive a letter from you tomorrow morning stating whether you will come by the one or the other. I beg to say that it would afford me great pleasure to see the Committee at dinner at 6 for half past but we don't know whether that would be agreeable to them. I leave therefore the arrangement entirely to you— It also happens that I had invited your friend Joseph Chater² & his eldest son³ to meet you on this occasion, & they will be here at 1-40 PM or at least I hope so – I shall not know until tomorrow morning[.]

I shall be much obliged by your telegraphing me tomorrow morning at Summerfield House nr Birmm. saying, whether it would be expedient that I should meet the Committee at the Station or otherwise.

Of course if you come by the train to arrive at 1-40 PM, this will be unnecessary.

I am | My dear Sir | Yours very faithfully | R.L. Chance
M. Faraday Esqr

1. See letter 3468.

2. Joseph Chater (1797–1875, Chater (1977), 18). London glass merchant.

3. Henry Chater (1825–1895, Chater (1977), 18). London glass merchant.

Letter 3470

Faraday to Peter Henry Berthon

3 July 1858

From the original copy in GL MS 30108/2/83

Royal Institution | 3 Jul 1858

Sir,

I have been to Birmingham according to your instructions¹ (accompanied as you are aware by a committee of the Elder Brethren²) and have examined carefully by day & by night the two catadioptric apparatus intended for Whitby. They offer much improvement as compared with the apparatus I last saw there, and I am satisfied with them.

As to *colour* the glass is excellent[.] There is very little variation in the different pieces. In some of the reflecting prisms the ray has to travel through six *inches* of glass, and yet these parts compared with a scale constructed from window glass, did not give a deeper colour than *one inch* of the lightest window glass that I could find, out of some hundred specimens. I do not expect that this colour will be surpassed by any future exertions[.]

As to *bubbles*; these were present in some of the pieces of glass, and irregular:– but by far the great majority of pieces were all that could be desired – Bubbles do harm according to their superficial extent but not so much as is sometimes supposed.

Striae – were present in some of the pieces and in a few were very strong indeed. In this point improvement is desirable and as in the worst cases striae

occurred at one end of a piece of glass whilst the other was nearly clear it is to be hoped that such improvement will be finally obtained_[.]

The mechanical working of the glass & the form was very good, being considerably in advance of the former apparatus. The work was examined closely in the day time and also at night at distances of 190, 317 and 421 feet. The rays of light issued in their proper order & right course and though the wind at night disturbed the lamps and made the light irregular still the *direction* of the light was always good_[.]

The apparatus illumine 190° of the horizon – one of them has three large metallic reflectors in the spare space which throws the rays proceeding in their direction back towards the focus & by it (in part) on towards the refractors. Their effect in giving a superiority to that apparatus above the one without reflectors could not be judged at night because of the wind & irregularity of the lamp flame – but from the day light investigation I have no doubt that they do return useful rays of light towards the sea horizon; however I am not prepared to say in what degree_[.]

The general construction of the metal frame seemed to me to be good & sound, but of that I do not profess to be a sufficient judge³.

I am Sir | Your Very faithful & Obedient Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letters 3461 and 3468.

2. Faraday was there on 1 and 2 July 1858. The committee was composed of Mark Currie Close and Gabriel Jemmet Redman (d.1878, age 80, GRO), an Elder Brother of Trinity House, 1847–1878, Chaplin [1950], 209.

3. This letter was read to Trinity House Court, 6 July 1857, GL MS 30004/27, pp. 218–9. It was agreed to install Chance's apparatus in Whitby.

Letter 3471

William Edward Fitzmaurice to Faraday

3 July 1858

From the original in GL MS 30108/3/90.3

Hamilton Lodge | 3 Jul

Dear Sir,

I am going to shew the *new light* to Lord Lansdowne¹ and a few friends on Friday next² at half past ten when I should very happy to see you. I understand that you will probably have had [two words illegible] before you in an *official* manner before then. I remain

Dear Sir | Yours faithfully | W.E. Fitzmaurice

Endorsed by Faraday: 9 Kensington Gore | /58 | Honble | Fitzmaurice

1. Henry Petty-Fitzmaurice, 3rd Marquis of Lansdowne (1780–1863, ODNB). Prominent Whig politician.

2. That is 9 July 1858.

Letter 3472**William Edward Fitzmaurice to Faraday****6 July 1858****From the original in GL MS 30108/3/90.4**

Hamilton Lodge, Kensington Gore | Tuesday, 6 Jul 1858

Dear Sir,

I fear that I must have made some mistake in my note. I have asked Lord Lansdowne¹ and a few other friends here on *Friday* evng. next the *9th* July at half past ten, and I wished you to have such time here *on that Evng.* From what I had heard I expected that the trial of the Trinity Board would have taken place tonight at Blackwall or wherever their *official* trials come off. Whether it be so or not I have not heard and I am uncertain whether they would send to me about it as all the apparatus is at Mr. Wilkins[.]

Yours faithfully | W.E. Fitzmaurice

I send this by hand fearing my note of this mornng may not come in time[.]

1. Henry Petty-Fitzmaurice, 3rd Marquis of Lansdowne (1780–1863, ODNB). Prominent Whig politician.

Letter 3473**Charles Barry to Faraday****6 July 1858****From the original in IET MS SC 2***Private*

Old Palace Yard | 6 Jul 1858

My dear Faraday,

Mr. Gurney¹ the ventilator and worthy successor of Dr. Reid² at the New Palace at Westminster who boasts of not having looked into a chemical book for the last 25 years has assumed with reference to my two great towers at the New Palace, that the fumes of a coke fire at the bottom of a shaft in such of them will not affect either iron, or gold at the top of these towers although the said fumes which are there most suffocating are in constant contact with the metals to which I have alluded and although they are already in a state of oxidation & decomposition I have ventured to take exception to Mr. Gurneys dictum on this subject which he is nevertheless purposed to defend – may I ask of you whether he or I am right?³

Most truly yours | Charles Barry

1. Goldsworthy Gurney (1793–1875, ODNB). Inventor.

2. David Boswell Reid (1805–1863, ODNB). Worked on the ventilation and lighting of the new Houses of Parliament, 1836–1845.

3. For the difficult relations between Barry and Gurney over the ventilation and lighting of the new Houses of Parliament, see Porter (1998), 168–88.

Letter 3474**Faraday to Henry Bence Jones****7 July 1858****From the original in RI MS F1 E10**

Royal Institution | 7 Jul 1858

My dear Bence Jones,

As I shall be out all day and hope to go off tomorrow I cannot resist writing to tell you that Jane & I were at Hampton Court yesterday & found all very active in repairing &c. I cannot help thinking that Mr. Becketts [sic] influence has been felt there but did not hint at anything of the kind. They find the house &c very much out of repair – the Green house especially & wanted to pull it half down but as it is especially mentioned in the warrant¹ as part of the Queen's favour I am not inclined to lose the pleasure we anticipate from it. Whether I shall get that & the Garden done or not I do not as yet know.

You have I suppose no objection to my urging Mr. Vulliamy² & Mr. Faraday³ on with the lecture room⁴. As it will bring workmen into the house we want it advanced & finished before repairs in the stairs Hall &c⁵ come on for as the house has to be kept open we cannot have repairs in all parts at once.

Tomorrow we hope to go off for a fortnight, but circumstances have changed our destination from *Folkstone* to *Eastbourne*. I am on some points sorry for this – as you may suppose from what you know of our intentions & hopes of meeting your family here & there: – but one cannot arrange everything to harmonize.

Ever My dear friend | Truly Yours | M. Faraday

We yesterday as a Committee agreed warmly & unanimously to the enclosed report on Degrees in science⁶. I expect some opposition in the *Senate* to day – but think we shall carry it on – perhaps warmly – but the senate must have time to think of it⁷.

MF

1. Letter 3457.

2. Lewis Vulliamy (1791–1871, ODNB). Architect to the Royal Institution.

3. James Faraday (1817–1875, GRO). Gas engineer. Nephew of Faraday.

4. This was for improving the lighting and ventilation of the lecture theatre. See RI MM, 5 July 1858, 11: 248.

5. *Ibid*.

6. *Report of the Committee appointed to consider the propriety of establishing a degree or degrees in Science*, London, 1858. See note 3, letter 3444.

7. Faraday proposed the acceptance of the report at the Senate. However, the philosophical radical Henry Warburton (1785–1858, ODNB) sought to refer it back to the committee. This proposal was overwhelmingly defeated and the report was adopted. University of London Senate Minutes, 7 July 1858, pp. 57–8.

Letter 3475**William Edward Fitzmaurice to Faraday****7 July 1858****From the original in GL MS 30108/3/90.5**

Hamilton Lodge | Wednesday, 7 Jul

Dear Sir,

I regret much that I shall not be able to see on Friday¹ one who can so truly appreciate the smallest efforts in science but if I might take such a liberty as to ask you alone I should be most happy to see you either this Evening or tomorrow Evening (Thursday) as soon after half past nine as might be convenient. I am going to [do] a few experiments upon different objects in the garden to see where the best effect is to be had and as I shall be at work both evenings it will make no difference in trouble or arrangements in any way.

I should be glad you saw it for although there would be *little* new to you in the matter still I think that *little* would satisfy you that a most brilliant and *perfectly permanent* light is to be had at small cost & without complication[.]

I remain Dear Sir | yours faithfully | W.E. Fitzmaurice

Endorsed by Faraday: /58

1. That is 9 July 1858.

Letter 3476**Faraday to Peter Henry Berthon****8 July 1858****From the original copy in GL MS 30108/2/84**

Royal Institution | 8 Jul 1858

Dear Sir,

The Catadioptric light apparatus intended for the Bishops rock light-house having been erected at the Trinity house, so as to be accessible in all directions & capable of examination, I proceeded yesterday to inspect it both by day & night. The points which admit of such examination are Colour,—bubbles,—striae,—workmanship, & optical action of the parts. When these are all right, there is no doubt that the effect at a distance will be good[.]

As to *colour* the glass was greener than I expected to find it. It was sensible in the refractive & chief part of the apparatus, and much more in the reflecting prisms above and below. There the colour in many pieces equalled Nos. 6 & 7 or the deeper shades of the experimental scale I have before referred to. Improvement in this respect is desirable and attainable.

In respect of *bubbles* the glass was very clear indeed. There were a few here & there, but the apparatus is in that point of view unexceptionable.

There were no large or strong *striae*, but numerous small ones, often occupying the whole of the glass and lying horizontally in it from end to end.

Their smallness & the manner in which they are disposed, is evidently due to the way in which the glass has been gathered & drawn out. As far as they can act they tend to disperse light in a vertical plane; but it is difficult to say to what extent this may take place, & whether the effect at a distance is importantly affected by them or not.

The optical action & distribution of the light is not so good as I expected; considering the results with some former French apparatus. As the eye travels up and down near the apparatus, between the top & bottom, the flame or other object in the focus appears in the ribs of glass in the proper order:— but at 15 or 20 feet off, many of them present the luminous object in the reverse order. Closer examination shows that this is because the secondary foci of many of the ribs are at distances not greater than from 8 to 20 feet instead of being on the horizon;— the rays from these pieces do not therefore proceed in a parallel direction to the horizon but diverge after passing these foci. The effect on the screen at different distances is manifested by the production of light & dark bands. A dark band originates $5\frac{1}{2}$ inches below the level of the focus (at the line where the 11 inch central rib joins the one beneath it); which is common to many of the pannels & does not disappear at any distance which I can obtain in the room. I cannot tell how far these circumstances would affect the result at a distance and it is right that I should call to remembrance experiments made many years ago (& reported upon) in which two pannels at Purfleet, were examined & compared from Blackwall wharf¹, the one being a well wrought French pannel & the other a pannel from Newcastle having the condition described above. The latter did not present the inferior condition which was expected & I concluded that in that case the imperfect workmanship of each piece or rib was compensated for by the overlapping of the rays of *all the 17 pieces* of which the pannel was composed.

The workmanship of the frame of the apparatus appears to be very good².

I am | My dear Sir | Most faithfully Yours | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Faraday report, 16 October 1840, GL MS 30108/A1, pp. 144–56. (This will be published in an appendix in volume 6).

2. This letter was read to Trinity House By Board, 13 July 1858, GL MS 30010/41, pp. 466–7. It was agreed that Wilkins would be asked to replace the faulty portions as advised by Faraday.

Letter 3477

Faraday to Carlo Matteucci

9 July 1858

From the original in the possession of Kurt Wolfgang Vincentz

Royal Institution | 9 Jul 1858

My dear Matteucci,

You will learn from Madame Matteucci that I received your letter of the (no date) informing us of her journey to London & that whilst in the act off

setting off[f] for Albany Street Madame Matteucci came to us[.] It gave us great pleasure to see her & hear of you & since that I have had the pleasure of a chat with her & have also received a note from her of the 7th instant telling us of her departure. Pray acknowledge this note for me with expressions of our kindest hopes & wishes.

Madame Matteucci refers to some letter I may possibly have received from you as requiring an immediate answer[.] I am not aware of any since that referred to above. As for original matter I have none to give you for I work but slowly have no new results perfected and cannot tell when I may have. As to what I read of the works of others I cannot remember it & so have no power to talk about it. I rejoice to hear that you are at work indeed I think wherever you are you work and so must advance. We are always expecting new results.

I have no idea of being able to reach Switzerland this year – I doubt if I shall be able to go to our own association at Leeds¹. Indeed I always hold the future as if a week might change it or even close it as regards this life. No man can reach my time of life without the natural warnings and to me they are as interesting as any point or part of physical science indeed I hope for more so. So you see which way my philosophy tends at present:– & it does not exclude other thoughts & motives which are far more important than any philosophy[.]

Ever My dear Matteucci | Yours | M. Faraday

Address: Professor Matteucci | &c &c &c | Hotel l'Empereur Joseph | Rue de Tournan | à Paris

1. That is the Annual Meeting of the British Association.

Letter 3478

Peter Henry Berthon to Faraday

15 July 1858

From the original in GL MS 30108/2/84

Trinity House, London, E.C | 15 Jul 1858

Sir,

Having laid before the Board your Letter of the 8th. Instant¹, reporting the result of your examination of the Catadioptric Apparatus supplied by Messrs. Wilkins and Co. for the Bishop Rock Light House,– I am directed to express the disappointment with which the Elder Brethren have received so unsatisfactory an account of it's general character and quality,– and to acquaint you that having felt it necessary to call upon Messrs. Wilkins to replace all the portions which you have pronounced to be defective, with others of unexceptionable quality, at such time and in such manner as not to interfere with the exhibition of the Light on the 1st. October next, of which Public Notice has been given, the Elder Brethren will be obliged by your

advice as to the particular requirements which they should make of Messrs. Wilkins, in respect of the portions of the Apparatus to be replaced.–

I am, | Sir, | Your most humble Servant, | P.H. Berthon
M. Faraday Esq. | &c &c &c

1. Letter 3476.

Letter 3479

Louis Sautter to Faraday

15 July 1858

From the original in GL MS 30108/2/84

Londres | 15 Juillet 1858

M. Faraday Esqr. | London | Royal Institution
Monsieur,

Ayant reçu communication du rapport présenté par vous à Trinity house¹ au sujet de l'appareil construit par MM Wilkins & Co. pour le phare de Bishop Rock, et dont la partie optique sort de mes ateliers, je suis venu à Londres dans le but de vous présenter à ce sujet quelques observations. Les conclusions de ce rapport sont loin de nous être favorable, et le nom dont il est signé lui donnera sans aucun doute un grand poids, non seulement aux yeux de Trinity house mais aux yeux de tout le monde savant. Bien que mon opinion ne puisse être mise en regard de la votre, cependant, je crois pouvoir, en m'appuyant sur mon expérience de constructeur, et sur l'opinion des hommes distingués qui sont en France à la tête de l'administration des phares, répondre d'une manière satisfaisante à la plupart des reproches que vous adressez à notre appareil. J'ose espérer, Monsieur, que prenant en considération la gravité que cette question a pour moi, vous voudrez bien accueillir ma demande; et m'indiquer le moment auquel il vous sera possible de me recevoir.

Veillez agréer, Monsieur, l'assurance de ma considération respectueuse | L. Sautter
London coffee house | Ludgate Hill | London

TRANSLATION

London | 15 Jul 1858

Mr. Faraday Esq. | London | Royal Institution
Sir,

Having received communication of the report presented by you to Trinity House¹ concerning the apparatus constructed by Messrs Wilkins & Co. for the Bishop Rock lighthouse, and for which the optical part was made in my workshop, I have come to London in the hope of presenting to you a few observations on this subject. The findings of this report are far from being

favorable to us, and the name of the signatory will no doubt give it great weight, not only in the eyes of Trinity House but also in the eyes of every savant. Although my opinion cannot be compared with yours, nevertheless, I believe I can, relying on my experience as a constructor, and on the opinion of the distinguished men who are at the head of the French lighthouse administration, answer in a satisfactory manner most of the criticisms that you make about our device. I dare to hope, Sir, that taking into consideration the importance of this matter to me, you will wish to accede to my request; and to indicate a convenient time for me to visit you.

Please accept, Sir, the assurances of my respectful consideration |
L. Sautter

London Coffee House | Ludgate Hill | London

1. Letter 3476.

Letter 3480

Peter Henry Berthon to Faraday

19 July 1858

From the original in GL MS 30108/3/90.6

Trinity House | 19 Jul 1858

My dear Sir,

Adverting to your inspection in company with the Deputy Master¹, of the Lights exhibited at Major Fitzmaurice, on the Evening of Monday last²,— I am directed to request you will state for the information of the Elder Brethren, whether you consider that the means by which the Light is produced are such as to render it applicable to Light House purposes,— or what preliminary data you would require for the purpose of forming an opinion on the Subject.

I remain | My dear Sir | Yours very Truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. John Shepherd.

2. That is 12 July 1858.

Letter 3481

Louis Sautter to Faraday

19 July 1858

From the original in GL MS 30108/2/84

24 & 25 Long Acre, London | 19 Juillet 1858, WC

Monsieur Faraday M.R.S. &c &c &c | Londres

Monsieur,

Je prends la liberté de vous adresser quelques observations au sujet du rapport adressé par vous à Trinity House sur la partie optique du phare de

Bishop Rock¹; Vous avez trouvé qu'il laissait à désirer

1° Sous le rapport de la coloration du verre

2° Sous le rapport des stries

3° Sous le rapport de l'effet optique.

(1.) 1°. La coloration du verre. Vous estimez qu'elle est trop foncée, et que sous ce rapport des améliorations peuvent être réalisées.

Le verre entrant dans la composition de nos appareils est du verre de glace de St. Gobain. Il a toujours eu une couleur légèrement verdâtre, et il nous semble pas que cette coloration aie augmenté dans ces dernières années. Cependant ne voulant pas nous en rapporter sur ce point à notre impression personnelle, nous avons consulté l'ingénieur des phares de France qui nous a répondu la lettre ci jointe.

(1.A.) Sans méconnaître l'avantage qu'il y aurait à ce que le verre fût plus blanc, nous croyons qu'il faudrait pour cela, en changer la composition, et sacrifier des qualités plus essentielles, la *pureté*, la *dureté* et l'inalterabilité. C'est là le motif qui à la suite d'expériences qui durent depuis 36 ans, a décidé l'administration des Phares de France, à préférer le *verre de glace à base de soude*, malgré sa couleur verdâtre aux verres à *base de potasse et de plomb*.

Nous croyons d'ailleurs qu'en outre de leur plus grande inalterabilité, les verres que nous employons sont, du raison de la forte proportion de *silice* qu'ils contiennent, plus transparents, plus lumineux que les autres – C'est ce qui pourrait être vérifié par des expériences photométriques.–

(2) 2° Les stries. Nous croyons les *filles secs* que notre verre contient, moins nuisibles que les *filles gras* qu'on observe presque toujours dans les verres qui contiennent du plomb. Nous n'avons pas remarqué qu'ils ajoutassent sensiblement à la divergence de l'image dans chaque prisme.

(3) 3° *L'effet optique*. Il est bien possible que quelques uns des arceaux catadioptriques aient été légèrement dérangés de leur position dans le voyage. Nous avons l'habitude de les vérifier, et s'il y a lieu, de les corriger au moment de la mise en place de l'appareil. Mr. Wilkins a négligé cette vérification, qui sera toujours faite à l'avenir, et (3.A) qui pour l'appareil de *Bishop Rock*, pourra avoir lieu après son installation sur la tour.

Les *bandes noires* que vous signalez dans les panneaux dioptrique peuvent être dues à la largeur du point entre deux prismes successifs: il est possible aussi que dans le but de faire disparaître certains petits défauts tels qu'écailles, dentelures, sur un des bords de la lentille centrale, on l'aie usée plus d'un côté que de (3.B) l'autre, en sorte que son axe optique ne se trouve plus compris dans le plan focal de l'appareil.

La différence en tout cas ne peut être que faible (3.B) et nous aurons soin d'éviter ce défaut à l'avenir.

L'existence d'un *foyer secondaire* à une certaine distance en avant de la lentille, est un inconvenient qu'il ne nous est pas possible d'éviter complètement. Les légères, mais inevitables variations dans la composition, (3.C) et dans l'indice de refraction de nos verres, se traduisent par les differences dans la longueur focal de chaque prisme, differences que peuvent être d'une centimetre au dela ou en deça de la longueur voulue. De là l'existence d'un foyer secondaire, réel ou virtuel placé en avant ou en arriere de la lentille.

Nous avons l'habitude de nous rendre compte du degré d'exactitude de chaque panneau, en plaçant à une certaine distance en avant de la lentille une lampe ordinaire. Connaissant l'éloignement de cette lampe et la distance focale principale de la lentille, nous calculons celle du foyer conjuguée. Nous promenons un ecran derriere la lentille, et nous nous assurons si chaque armeau donne son foyer bien net; si ces foyers coïncident, et si cette coincidence a lieu à la distance de la lentille voulue par calcul.

Il n'est pas une des lentils que nous avons livrées à Trinity House, qui n'aie été soumise à ces experiences, et dont le foyer n'aie été reconnu exact à moins d'un centimetre près.

(3.D) Nous espérons, Monsieur que les observations qui précédent, tant en laissant subsister vos critiques vous sembleront de nature à en diminuer la portée, en les presentant les defauts reproches à nos verres, soit comme des inconveniens inherens à notre fabrication, et qu'on ne pourrait éviter sans tomber dans des inconveniens plus graves, soit (1A) comme de legers défauts de montage qu'il est facile de corriger, et que nous aurons soin d'éviter (3.E) à l'avenir.—

Veuillez agreer, Monsieur, l'assurance de ma consideration respectueuse | L. Sautter

TRANSLATION

24 & 25 long Acre, London | 19 Jul 1858, WC

Mr. Faraday F.R.S. &c. &c. &c. | London

Sir,

I take the liberty of addressing to you some observations relating to the report addressed by you to Trinity House on the optical part of the Bishop Rock lighthouse¹; you reported that there were problems concerning:

1st the colour of the glass

2nd the straie

3rd the optical effect.

(1.) 1st. The colour of the glass. You consider it to be too dark, and that in this regard some improvements can be made.

The glass used in the construction of our apparatus is sheet glass from St. Gobain. It has always had a slightly greenish colour, and it does not seem to us that this tint has increased in the last few years. However not wishing to

rely on our own personal impression with regard to this matter, we consulted the French lighthouse engineer, whose letter of response is attached.

(1.A.) Without underestimating the advantage that white glass would give, we believe that to achieve this, it would be necessary to change the composition, sacrificing more essential qualities, *purity, hardness* and stability. This is the reason why, following experiments which have lasted for 36 years, the French lighthouse administration has been persuaded that soda based glass is better, in spite of its greenish tint, than *potash and lead based* glass.

We believe, moreover, that in addition to stability, the glass that we use is, because of the high proportion of *silica* that it contains, more transparent and more luminous than any other – this could be verified by photometric experiments.–

(2) 2nd *The strae*. We believe that the *thin lines* that our glass contains, are less injurious than the *thick lines* that one almost always observes in lead glass. We have not noticed that they add significantly to the divergence of the beam in every prism.

(3) 3rd *Optical effect*. It is possible that some of the catadioptric ribs had been slightly dislodged from their position during the journey. We usually check them, and if necessary, adjust them when the apparatus is set up. Mr. Wilkins did not carry out this verification, but it will always be done in the future, and (3.A) with regard to the apparatus for *Bishop Rock*, can take place after its installation in the tower.

The *black bands* that you refer to in the dioptric panels might be due to the distance between two successive prisms; it is also possible that in trying to eliminate some small defects, such as flakes or serrations on one of the sides of the central lentil, which occur on one side more than (3.B) the other, the optic axis may no longer be within the focal plane of the apparatus.

The difference in any case can only be slight (3.B) and we will take care to avoid this defect in the future.

The existence of a *secondary focus* at a certain distance before the lentil, is an inconvenience that it is not possible for us to avoid completely. The slight, but unavoidable variations in the composition (3.C) and in the refractive index of our glass, express themselves in the focal length of each prism, differences which can be of about a centimetre this or that side of the required length. Hence the existence of a secondary focus, real or virtual, before or behind the lentil.

We usually test the degree of accuracy of every panel, by placing an ordinary lamp a certain distance in front of the lentil. Knowing the distance of this lamp and the main focal length of the lentil, we calculate the combined focus. We place a screen behind the lentil, and we ensure that each piece gives a clear focus, that these foci coincide, and that this coincidence takes place at the distance from the lentil predicted by calculation.

The lentil delivered to Trinity House had not been subjected to the tests to ensure that it was precise to at least one centimetre.

(3.D) We hope, Sir, that the preceding observations, whilst accepting your criticisms, will seem significantly to diminish their import, presenting the defects attributed to our glass either as inevitable inconveniences due to our manufacturing process and which could not be avoided without introducing more serious disadvantages, or (1A) as small defects in installation which it is easy to correct, and which we will take care to avoid (3.E) in the future.–

Please accept, Sir, the assurance of my respectful consideration |
L. Sautter

1. That is letter 3476.

Letter 3482

Faraday to William Scrope Ayrton

20 July 1858

**From the original formerly in the possession of the
late Mr. and Mrs. Aida**

Royal Institution | 20 Jul 1858

My dear Sir,

Accept my very earnest and hearty thanks for your kind invitation. I do hope to see Leeds during the Association week¹ but if I am there it will be as the Guest of Mr. Becket[t]. With the strongest sense of your kindness I am

My dear Sir | Yours Very faithfully | M. Faraday
W.S. Ayrton Esqr | &c &c &c

1. That is the Annual Meeting of the British Association.

Letter 3483

Faraday to Peter Henry Berthon

20 July 1858

From the original copy in GL MS 30108/2/84

Eastbourne | 20 Jul 1858

Sir,

On the Receipt of your letter of the 15th¹ at Eastbourne I instantly went up to London and finding M. Sautter the constructor of the Bishops Rock apparatus arranged to meet with him at the Trinity House and explain to him my mode of examination. Unfortunately there was no part of the Bishops rock apparatus that had not been sent off but we placed up & examined an old French pannel of excellent character and also a new one of M. Sautter which were quite sufficient to illustrate all the points we had in view.

M. Sautter has since that written to me in reference to my Report to you and I enclose his letter marked S.A. 19 July 1858² and marked also in the margin for reference. You will see that he admits the correctness of my criticisms (D) but adds observations which should influence their effect on the final judgement of the Trinity House. In my reports I have never presumed to express an opinion as to the acceptance or refusal of an apparatus. My object has been to state how near to or far from perfection any apparatus was, not that perfection is ever attainable but to mark the relative position in which any particular apparatus may stand – to prevent retrogression – and to point out what may yet be wanting. To do this seems to me the more important because if a concentrated light, intense in effect, but small in dimension, as the Electric light or the lime light ever becomes available, it will require a perfection of work & quality which may easily be dispensed with as at present when a large Fresnel flame is used.

I will now refer to the three points of 1 Colour, 2 Striae, 3 Optical action; which are referred to in M. Sautters letter.

The colour is admitted (1) and its disadvantage (1.A.), but it is considered as a necessary consequence of that composition of the glass which gives hardness, inalterability by sea air, and diminished striae. I have no reason to doubt the quoted result of many years experience in France and I accept the subject as one of balanced considerations. Glass of less colour has come to us from France and other glass to which I cannot refer more particularly here is whiter in colour:– but the compensations hardness & unalterability of such when forming part of the apparatus are points very difficult to judge of[.]

As to Striae (2). I have not made them an objection in my report I have only marked their existence & character as a record in respect of this particular apparatus[.]

As to Optical effects (3) I need say little or nothing[.] You will see (3.A.) that some things are to be corrected – and others (3.B.B. 3.E.) in future avoided.

I may conclude by recommending the apparatus to be accepted. I admit the considerations regarding colour hardness & inalterability – though I do not know that a white glass cannot be had possessing the requisite degree of the latter qualities – and I have full confidence that M. Sautter will cause the corrections referred to (3A) to be made³[.]

I am Sir | Your Very faithful Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letter 3478.

2. Letter 3481.

3. This letter was read Trinity House By Board, 27 July 1858, GL MS 30010/41, p. 487. It was agreed to act on Faraday's recommendation.

Letter 3484

Faraday to Peter Henry Berthon

21 July 1858

From the original copy in GL MS 30108/3/90.7-9

Eastbourne | 21 Jul 1858

My dear Sir,

The light which I saw in company with the Deputy Master¹ at the house of the Honble Major Fitzmaurice² had in every respect both as to its high power and the apparatus employed the character of the ordinary oxyhydrogen lime light as used at the Polytechnic & other similar establishments and as I have myself frequently employed it in the Royal Institution. The lime light was proposed many years ago by Lieut. Drummond³ for lighthouse purposes⁴ & I have no doubt was then very fully considered.

The person who superintended the light at Major Fitzmaurice[']s house, professed to know most about it, & was supposed to be the responsible person, said there were *three* causes of its superiority over the known lime light[.] *One* of these was in something added to the oxygen & hydrogen gases this was kept secret – *Another* was in the nature of the medium which as far as I could then judge was lime but this also was kept secret. The oxygen & hydrogen gases were preserved in a compressed state in strong iron cylinders and the *third* cause of superiority was assumed to exist in the friction of the gases at the valves as they issued from the cylinders; this friction being supposed to produce an electric or some other favourable state.

(1) Speaking for myself I should as a practical philosophic adviser first require to know *all* the circumstances and liabilities of the light in order to judge of their permissibility & probable utility. I should therefore require to know the two secret points; & I should desire this the more earnestly because I place no confidence in the third supposed cause of advantage.

(2) Besides these secrets I should then require to know in what form or arrangement the light is to be applied practically – whether as a central light or in the manner of lamps, i.e. each medium being in its own reflector also whether when in reflectors as a central light a fixed light, or a revolving light for each of these would require different consideration as to arrangement, expence, power of the light, & its proper service. In any case the assumed advantage of the mode of application should be stated – If that advantage be in superior illumination; then I should desire to know the cost of the light and the proof that the estimation was correct and should also require experiments by which the light could be measured in relation to an Argand or Fresnel lamp; & the cost of the preparation and consumption of the gases be ascertained[.]

(3) Next would come trials respecting the *time* during which such a light might be left – whether, as under present arrangements, the time might extend to an hour or more without fear – or whether the attention must be constant:– for very much of the safety & certainty of a light depends upon its comparative steadiness when arranged & left to itself.

(4) Then must be considered the preparation of the two gases Oxygen & hydrogen & the *peculiar substance* to be added to them;– the quantity of the gases required for a winters night; the storing of the gases;– and as compression is supposed to be necessary, the engine pump apparatus required for that compression. Also the peculiar knowledge & care both chemical & engineering required in the persons taking charge of these points.

(5) As the gases are very explosive when mixed, and as the Trinity house have in the prevention of plans proposed to them had an explosion in a lighthouse, so the possibility of such an event under any circumstances must be most carefully considered & for that reason the details regarding the preparation & use of the oxygen & hydrogen must be fully stated.

(6) Such is the general order in which I should have to consider the subject & I think it the only manner in which it can be usefully developed. It is not until that has been done that I think trial in a light house would be of service. When tried in a light house it should be in some form available in practise. I do not know that a single fixed narrow concentrated ray is ever required. If tried as a *revolving* light it will of course give a very intense flash; but its divergence, & therefore its time, will be small compared to the Argand lamps in reflectors perhaps only one third.

If it be tried as a fixed light merely for the purpose of a visible effect, then in order to make it instructive for lighthouse purposes, it ought to be compared with the usual oxyhydrogen lime light, or Drummond light; and if possible with the Magnetic Electric light: these also being each in its own reflector.

Perhaps I have gone more into this matter than you required of me but I do not see any other way in which a useful practical result may be obtained & disappointment avoided⁵.

I am | My dear Sir | Very Truly Yours | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. John Shepherd.

2. See letter 3480.

3. Thomas Drummond (1797–1840, ODNB). Officer in Royal Engineers. Worked on Ordnance Survey of Ireland. Later Under Secretary in Ireland, 1835–1840.

4. Drummond (1830).

5. This letter was read to Trinity House By Board, 27 July 1858, GL MS 30010/41, pp. 488–9. It was agreed to send the substance of this letter to Fitzmaurice and to ask that any comments he had should be sent directly to Faraday in strict confidence.

Letter 3485

Faraday to James David Forbes

23 July 1858

From the original in SAU MS JDF 1858/80

Eastbourne | 23 Jul 1858

My dear sir,

I received your letter & paper¹ here & it was very pleasant to hear from you; not merely for the subjects sake but for friendships sake;— & I may take the opportunity in this reply to thank you for your kind estimate of my scientific work in your history of physical science². Perhaps I have no right to thank you for what you wrote in the cause of science; but it was not the less grateful to my feelings, that I had to consider it *impartial*.

I have not worked on the subject of *regellation* since the first³. I am very glad it has been taken up by others & shall leave it for the present to them. I hold my mind therefore suspended amongst the views put forth respecting the prime cause of the effect. As however you wish me to make observations on any point I will venture one or two_[.]

I do not consider the proof you give at paragraph marked 1 as sufficient to shew that metals freeze to ice at 32°⁴. That the lowest of a pile of shillings after *sinking* into the ice – becoming cooled to 32° – and moulding the ice to its own rough form, should adhere to it at the bottom of the cavity, seems a very probable convergence of the roughness of the shilling & the ice; their clean contact, & the pressure of the atmosphere.— My experiments were made with gold leaf placed between two faces of ice the whole being immersed in ice cold water⁵ – the ice cold water always being prepared by pounding the ice & the water together, & leaving much of the finely powdered ice in the fluid_[.]

As to *Paragraph 3*⁶ the possibility of a *lower temperature* within the mass of ice than at its surface was considered by me, & at the time answered in the *negative* as to its necessary connexion with *regellation*, thus:— Ice was put into a water kept at 32° externally, water added, & the ice broken up & triburated in the finest possible manner. No matter how long this was continued, – whether the water was left or poured off from time to time, – whether warm water was added to the *finely divided* ice & the triburation continued until one half or more was dissolved;— in every case, if a quantity of the fine particles were taken on the hand & squeezed together *regellation at once took place*. So also if the finest snow be scattered into water (made a little warm before hand if required) & be shaken violently up together so as to make a thorough mixture of the snow particles & the water;— still, a portion of these taken in the hand & squeezed together *regellate*_[.]

I am only speaking of the old facts – I may add the following.— A Mr. Harrison⁷ has been manufacturing ice in London lately. He cools brine by

the evaporation of ether and then lets the brine flow round metallic vessels containing water which becomes frozen. These vessels are about $1\frac{1}{2}$ inch wide 20 inches long & 20 inches deep; a little narrower at the bottom, so as to give a wedge shaped plate of ice. The freezing of course begins against the metallic sides of the vessel; but as it proceeds, exceedingly fine thin crystals of ice proceed from the sides obliquely towards the middle of the water;— these often being 3 or 4 inches long, half an inch or an inch broad, and yet as thin as paper:— yet when I took one of these & held its end against the end of another. *regellation* occurred.

Ever My dear Sir | Very Truly Yours | M. Faraday
J.D. Forbes Esqr | &c &c &c

1. Forbes (1858).
2. Forbes (1857), 977–82.
3. *Athenaeum*, 15 June 1850, pp. 640–1 which contains an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'.
4. Forbes (1858), 104.
5. See *Athenaeum*, 15 June 1850, pp. 640–1.
6. Forbes (1858), 104.
7. James Harrison (c.1816–1893, AuDB). Scottish-born Australian inventor.

Letter 3486

Edward Sabine¹ to Faraday

24 July 1858

From the incomplete original in IET MS SC 2

Llanfair, Llandoverly, S. Wales | 24 Jul 58

Dear Faraday,

In one of your friday evening lectures at the Institution 2 or 3 years ago you spoke of the possibility of the lines of force (on which you were lecturing) affording by & bye a more precise means of measuring the magnetic variations than those which we have now in use². I wished then, and I have often wished since to make better known to you the capabilities of our present means; and a description which I have just completed of the lunar diurnal variation of the horizontal magnetic force at the Cape of Good Hope gives me I think a good opportunity of doing so. The *observations* are those hourly ones made between 1843 & 1846 which were published in the 1st Vol. of the Cape Magnetic Obsn. some years ago³ – The *discussion* will be in a volume now in the press⁴. The result is that twice in every lunar day, the horizl. magc. force of the earth is increased by one two-hundred-thousandth of its whole amount; and twice in each lunar day the force is diminished by an equal amount the maxima are at intervals of 12 lunar hours from each other, as are the minima; & the minima six hours from the maxima. And not only are these wonderfully minute differences of the force cognisable & satisfactorily determined, but each progression from maximum to minimum & from minimum

to maximum is determined by strictly independent observations at each of the intermediate hours with a very remarkable approach to regularity. Thus in every six lunar hours there is a variation of the force amounting to two, two-hundred thousandth of the whole force, which takes place by a regular progression cognisable by our present instrumental means by independent observations at every hour: These are the mean numerical values derived from 3 years of hourly observation. It really appears to me that greater precision than this need not even be desired in the present state of the science. If & when you show your audience a needle suspended by a silk thread under a glass cover directing itself into the m. meridian by the earth's force you told them that by the instrument employed and the systems practised in magc. observatories, a variation of a hundred-thousandth part of that

1. Writer identified on the basis of handwriting and that letter 3487 is the reply.

2. Faraday (1852a), Friday Evening Discourse of 23 January 1852.

3. Sabine (1851–76), 1.

4. The preface to Sabine (1851–76): 2 referring to the volume stated 'It was long hoped that it might have been completed under the superintendence of Sir Edward Sabine, and have included a discussion of the results of the observations. This cannot now be expected'.

Letter 3487

Faraday to Edward Sabine

27 July 1858

From the original in Berkshire Record Office MS D/EBY F48 111

Eastbourne | 27 Jul 1858

Dear Sabine,

How pleasant it is to receive a letter¹ from you out of the country and to write to you from the country in reply. Generally science is found in closer atmospheres than those we enjoy at present: I hope your place is doing you and still more Mrs. Sabine² much good in the way of health & strength; & that the weather has suited you. We have had much Electricity here & I was fortunate enough whilst watching a storm, to see a Church struck by lightning within a 100 feet of me; I was looking by the very pinnacle at the cloud & in the direction from which the lightning came.

Your data & results are most beautiful & I hope to have a good occasion to state them in all their force. I suppose there can be no doubt that the *moon* & the *Earth* are in magnetic relation as close & sure as is the Earth & a magnetic needle upon its surface. The elimination & determination of so minute a quantity is wonderful, & the method by which it is done must be beautiful. I had a notion that placing a series of soft iron bars in position & watching by needles their variation in force, as the lines of Magnetic force acted upon them varied from hour to hour & season to season, we might obtain results more quick & ready than those given by the effect of the same lines of force directly upon the magnets themselves; as in Gauss's³ methods⁴ & course in

yours:– but as yet the retention of a given state by the iron offers obstacles. Perhaps the use of helices might do good but I have not pursued the subject; & such results as yours seem quite sufficient for every present purpose.

I cannot help hoping that when refinements like those you speak of, are multiplied, Mathematicians will be able to draw their conclusions closer about the suppositions that are afloat than heretofore. When Clarke Maxwell examined my views of lines of Magnetic force mathematically⁵, he placed them at last upon an equality with the others; and whilst saying that views more comprehensive and competent were required to embrace the phenomena of Electricity & Magnetism jointly, he made me to hope that mine might be such. It is only by referring on the results & increasing the force of the experimental & observed facts, that we shall be able to select the most correct view & throw off the limited & superfluous ones.

Ever My dear Sabine | Very Truly Yours | M. Faraday

1. Letter 3486.

2. Elizabeth Juliana Sabine, née Leeves (1807–1879, ODNB under E. Sabine). Scientific translator who married Sabine in 1826.

3. Carl Friedrich Gauss (1777–1855, DSB). Director of the Göttingen Observatory, 1807–1855.

4. See Gauss (1841).

5. Maxwell (1856).

Letter 3488

Faraday to Justus Liebig

27 July 1858

From the original in BS MS Liebigiana II B, Faraday, M.

Royal Institution | 27 Jul 1858

My dear Liebig,

Having nothing of importance to communicate I must take advantage of a smaller matter to communicate with you – and yet it is no small matter that you should send me by Graham a mark of your good will in a piece of Glass silvered by Yourself¹ – It looks very beautiful – I have not as yet heard how it is done for I have through the force of several circumstances been very irregular in my reading for some time past and very forgetful of what I have read.

Whenever I think of you I picture you to myself as *well*, & in good health_[.] I hope in that that my expectations are true & yet I have a faint feeling of a report that you had not been in good health. My wife & I are both in the Country trying to re-energize but it is hard work_[.] I think I feel the effects of 67 years this season more than I anticipated from the 66 of last year. Except this circulatory laziness I am very well_[.]

Ever My dear Liebig | Yours Truly | M. Faraday

1. See Brock (1997), 136–9 and Vaupel (1991) for a discussion of this aspect of Leibig's work.

Letter 3489

Faraday to Julius Plücker

27 July 1858¹

From the original in NRCC ISTI

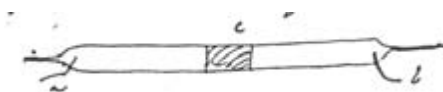
Royal Institution | 27 Jul 1857 [sic]

My dear Sir,

I seemed to have two or three of your letters unanswered & wondered at it but then I remembered that you had been *here* since some of them & we had talked them over. Your last I shewed as you desired to Mr. Gassiot & also to Dr. Tyndall[.] The latter is now in Switzerland amongst the Glaciers[.] I do not know whether you will see him at some of your German meetings. I have been obliged to give up thinking about the luminous current but whilst such as you Gassiot and others work at the subject I know it is progressing. The Arch which you shewed me corresponding to the Magnetic line of force and that other one of which you speak in one of your letters of like nature interest me very much inasmuch as their course seems to be directly at right angles to the course of the Electric current – The alternate light & dark parts of the stratified column are also exceedingly exciting: What is the essential difference of these parts? It is very easy to imagine a difference or even two or three kinds of it but what is the real difference?

Then again the question of transmission of the discharge across a perfect vacuum or whether a vacuum exists or not? *is* to me a continual thought and seems to be connected with the hypothesis of the ether. What a pity one cannot get hold of these points by some *experiments* – more close & searching than any we have yet devised[.]

You seem surprized to have had the double current when one end of the Ruhmkorf was connected with the coating of the tube & the other with the wire. With Gassiot & myself this was an instant conclusion from the experiment with two coatings & is described in his paper² as such. A very pretty variation of it is made by connecting



the two wires a.b. with one end of the Ruhmkorf & the coating c with the other: double currents are obtained in both halves of the tube but in opposite directions as is shewn by their examination with a magnet[.]

Ever | My dear Sir | Very Truly Yours | M. Faraday
Professor Plucker | &c &c &c

1. Dated on the basis that this is the reply to letter 3447 (because of the references to Tyndall and Gassiot) and the reference to Plücker's visit in April 1858 (letters 3413 and 3414).

2. Gassiot (1858).

Letter 3490**Faraday to Christian Friedrich Schoenbein****28 July 1858****From the original in UB MS NS 437**

Royal Institution | 28 Jul 1858

My dear Schoenbein,

Though I date as above yet I am residing in the country & that has caused me to miss your friend M. Vischer¹ which I was very sorry for[.] I called in Golden square – and wrote a letter in hopes he might return there but have heard nothing yet direct from him.

But I saw Miss Schoenbein last Sabbath day² and she gave me the papers & letters from you & your portrait³ all of which I was very glad to have[.] I like the portrait very much & was vastly glad to have it[.] It is very like my old friend but I perceive he is getting a little a very little older – when you see my photograph which Miss Schoenbein has you will see that is my case but then I have the advantage of you by 8 or 10 years – and am getting not merely older but *idler* & that is a worse thing.

I like your summary⁴ brief as it is of your views very much & was just on the point of sending it off to Mess Taylor & Francis for the Phil Mag when I doubted a little about the latter end & as the date was too late for this month thought I would write to you. It is the part about Davy & the criticism on his view, & those of chemists generally. I have no objection to them for I think all hypotheses unwholesome unless accompanied by criticisms – but I was not sure whether you might object intending it for me only. As there is time tell me so in a short note before I send the M.S. to the Editors for their acceptance or judgment[.]

Miss Schoenbein seems quite well – So are we generally & so must you be considering your intentions[.] I have no philosophy for you I am idle

Ever Truly Yours | My dear friend | M. Faraday

1. Wilhelm Vischer (1808–1874, ADB). Professor of Greek at Basle.

2. That is 25 July 1858.

3. This is in RI MS F1 I67.

4. Letter 3462.

Letter 3491**Faraday to Caroline Deacon****29 July 1858****From a typescript in RI MS**

Eastbourne | 29 Jul 1858

My dear Caroline,

My wife is preparing a letter for you and so I haste to use the few minutes allowed me to send my love. You have had much trouble of illness lately

I think, and though I do not remember the circumstances exactly I feel as if you had been useful in it to others – and that is to be useful to oneself – for the thoughts often work up in these matters – and though our own plans are disturbed, yet something comes out of the plans of him who is ruler of all things that is good to his people. Dear Caroline think of me sometimes – not in proportion to my thoughts of you for they are very slow;– nature and lukewarmness combining to make them so. I do not know what can be done under such conviction than first to examine ourselves and then to commit our keeping unto him who keepeth his people and maketh his reproofs acceptable in their results.

Remember me to your husband and to Constance

Ever My dear Caroline | Your Affectionate Uncle | M. Faraday

Letter 3492

James Thomson to Faraday

30 July 1858

From the original in IET MS SC 2

6, Franklin Place, Belfast | 30 Jul 1858

Sir,

At the request of my brother Professor William Thomson of Glasgow College, I take the liberty of sending to you the enclosed abstract of a Paper “On the Effect of Pressure in Lowering the Freezing Point of Water and on the Plasticity of Ice”¹[.]

The theory explained in this paper, as you will observe by a clause near the conclusion of the abstract, I consider affords a satisfactory explanation of the interesting property of ice to which you directed attention²;– that separate masses of ice laid in contact with one another, will, even in hot weather unite or freeze firmly to-gether.

I am Sir | Your obedient servant | James Thomson
Professor of Civil Engineering | Queen’s College | Belfast
Professor Faraday | Royal Institution London

1. This seems to have been privately printed which was common practice at this time for the Belfast Natural History and Philosophical Society where this paper was read on 20 December 1857 (private communication from Peter Bowler). There is a proof copy in IET MS SC 2.

2. *Athenaeum*, 15 June 1850, pp. 640–1 which contains an account of Faraday’s Friday Evening Discourse of 7 June 1850, ‘Certain Conditions of Freezing Water’.

Letter 3493

Faraday to James Thomson

4 August 1858

From the original in QUB MS 13/M/7

Royal Institution | 4 Aug 1858

My dear Sir,

I receive it as very kind of you that you should send me the Belfast report of your paper on Ice &c¹. I knew of, & have been very much interested with, your views respecting ice, & the effect generally of pressure on the fusing point. Profr. W. Thomson told me of them long ago². All the reasoning in the report I accept as truth; though I may hesitate in supposing that it contains the *whole* of what concerns the assumption of the solid or the liquid state by a particle of water under given circumstances_[.] It is curious & interesting to observe how much the general question has drawn attention.—Forbes is thinking about it—so is Tyndall & others also in Paris. The peculiar supposition of a *stickiness* of the ice, at the freezing or solidifying point is interesting; but one wants some better proof than, or additional proof besides, the fact of regellation_[.] I have not worked at the subject of late but I could not make ice stick to gold or metal at 32°; and I do not think that Forbes shilling is any proof of it³_[.]

Being sure that your principle is correct, which requires pressure & its variations;—& admitting stickiness, simply because I am not prepared to deny it;—I am at present strongly of opinion that there is another efficient cause of regellation to which I think I have referred in the old Athenaeum report⁴:—but I have not that here & therefore cannot clearly say. It seems to me that a particle of water touching ice on one side and water at the same temperature on the other, is not so apt to change its state for that of ice as another touching ice on both sides;—and this, not as the consequence of any very limited peculiarity in water & ice—but in subordination to a far more general law, if I may so call it, that in bodies of the same kind the particles tend to retain the state of those which surround them. Thus water may be cooled many degrees below 32°F. but a particle of the water in the midst of the cooled mass remains *as water* though colder than ice:—and yet a warmer body than itself, as for instance *a specula* of ice, touching it on one side & so breaking up the continuous liquid contact around it; instantly makes it solidify—In the same manner I can conceive that a particle of ice in the middle of ice may be raised with the mass to a *higher* temperature before it become[s] water than a particle of the same ice at the surface, can. This change from ice to water & water to ice, being independent of any effect of *pressure*.

We have an illustration of this effect in water also, when it changes from the liquid to the gaseous state, instead of from the liquid to the solid state. It is given as in Donny's⁵ beautiful results⁶; where he shews that water freed from air, may be heated to I think 300°F under the pressure of one atmosphere only,

and yet not boil or be converted into vapour within, though the introduction of the minutest bubble of air will cause it to explode:— a given liquid particle in the mass not being able at this high temperature to change its state into that of vapour whilst in contact on every side with liquid particles like itself though if once the contact of these particles be broken in any point, they will burst with violence into the new state.

Great numbers of other case[s] of this kind may be found amongst bodies able to change their state but they will readily occur to you. I am about to reprint my report from the Athenaeum in a Volume of Experimental Researches on Physics⁷; & think I shall arrange these views into some kind of form so as to give them a place beside your views & those of Forbes & others⁸[.] You have them uncorrected by any experiments except those of former time for I have made none lately though I have seen Tyndalls⁹[.]

Believe me to be | My dear Sir | Most truly Yours | M. Faraday
Professor | J. Thomson C.E. | &c &c &c

1. Letter 3492 and note 1.
2. Thomson to Faraday, 10 January 1850, letter 2251, volume 4.
3. Forbes (1858), 104. See letter 3485.
4. *Athenaeum*, 15 June 1850, pp. 640–1 which contains an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'.
5. François Marie Louis Donny (1822–1896, Seyn (1935–6), 1: 403). Belgian chemist.
6. Donny (1846).
7. Faraday (1859b), 372–4.
8. 'On Regelation' in Faraday (1859b), 377–82.
9. See Tyndall (1858).

Letter 3494

Christian Friedrich Schoenbein to Faraday

4 August 1858

From the original in UB MS NS 438

Speicher on the heights of the Canton of Appenzell | 4 Aug 1858

My dear Faraday,

I won't let wait you long for an answer to your very kind letter¹, with which you favored me some days ago and first of all permit me to tell you that I felt much gratified at learning from it, that you have not altogether condemned my heretical views. You are aware that I have these many years entertained them and tried on more than one occasion to combat Davy's doctrine on the nature of Chlorine &c². I can therefore see no harm in making known those views to the scientific public of England³, though I am quite sure that they will be but slightly relished by the majority of the british Chymists. I am even prepared to see Mr. Schoenbein declared to be half if not an entire fool, but being very little ambitious and caring far more for what I consider to

be true than for earning applause and eulogies from others, I shall take very coolly any strictures made upon my old-fashioned notions.

If you think my last letter to you worthy of being published in the *Philosophical Magazine* I give you full liberty to modify and curtail it, where and in what manner soever you please to do so. Getting more and more out of practice to speak and write your native tongue, I have no doubt, that my epistolary production will teem with all sorts of grammatical blunders and if your kindness is not too much taxed by the demand, I ask you the favor to correct the most palpable faults of my letter in order to render it less grating to english ears.

These last three weeks Mrs. Schoenbein, my two youngest daughters and myself have been residing upon the heights of the canton of Appenzell, that spot of Switzerland, I am most particularly fond of. It is the greenest land I know and I doubt very much, whether Ireland, emphatically called "the Emerald Island" can compete with Appenzell, the whole country about looking like an immense carpet of the softist velvet and being broken up into numberless hills chasms, valleys, dales, which here and there are patched with firwoods and covered with neat and snug little houses, the mere sight of which conveys comfort to the eye. Add to all these beauties a most extensive view on a great part of Switzerland Swabia, Bavaria and the Tyrol, between which the "swabian Sea", the stately lake of Constance is expanding its broad and blue sheet of water, you will readily allow, that such a seat and sight deservedly merit to be called glorious.

All of us, as you may easily imagine, fully enjoy the Charms of the country, the peculiar nature of which seduces us to lead a truly gipsy life, to-day making this, to-morrow another hill our temporary leager.

As often as I discover new beauties, I cannot help saying to myself: how should my friend Faraday enjoy such a sight! That under such circumstances Chymistry and every sort of philosophy are entirely forgotten, I hardly need assuring you.

In the beginning of next week we shall leave our alpine abode, Mrs. Schoenbein and the girls returning to Basle and Mr. Schoenbein going to Jena.

Pray present our best compliments to Mrs. Faraday and believe me
Your's | most truly | C.F. Schoenbein

1. Letter 3490.

2. See Knight (1992), 86–7.

3. In letter 3462 published as Schoenbein (1858c).

Letter 3495

John Tyndall to Faraday

4 August 1858

From the typescript in RI MS JT TS Volume 12, pp. 4084–6

Eggischhorn | 4 Aug 1858

My dear Mr. Faraday,

I wrote a letter to Mrs. Faraday five or six days ago but it was written during bad weather, and after a succession of rainy days, which probably cast their dull influence into the writing – so that on the whole the letter is not worth sending. My course hitherto has been thus. (1) To Paris where I saw nobody except Wertheim¹. Duboscq² I learned was suffering from inflammation of the brain. (2) To Zurich where I saw Clausius³ and bought a watch. (3) To Lucerne, making three *voyages* on the way. (4) to Meyringen over the Brännig. (5) to Grindelwald. Spent two days there upon the glaciers. At Rosenlani we⁴ slept a night, and next morning to my consternation found that a French tourist had taken away my boots. They were quite new and first rate. He left behind him a pair of vile Continental *souliers* in their place. A strong man followed him, overtook him at the Scheideck and deprived him of his illgotten booty. The subject of the structure of glacier ice has long been a source of discomfort to me. I had offered an opinion upon the subject, but still I was not *quite sure*, and the different opinions entertained by many intelligent glacier observers increased my hesitation. On this point I think I am now at peace, and it is the principal point which I had to settle during the present excursion. From Grindelwald we crossed the Strahleck and came down along the Finsteraar glacier to the Grimsel. Here we spent a day examining the traces of ancient glacier action. These are perfectly astounding: To a height of at least 2000 feet above the valley of Hasli this action can be traced with perfect distinctness. We crossed the Grimsel pass to the Rhone glacier and spent some time upon it – it was very instructive. How different things appear when the mental eye is cleared for their proper apprehension! The weather was here dismal, we spent a night at the little Auberge at the foot of the glacier and started down the valley in the rain next day. From Viesch we ascended to the Hotel Jungfrau situated half way up the Eggishorn, round which runs the great glacier of the Aletsch. From the summit of the Eggishorn the view is perhaps the finest I ever saw. But the great object of interest is the glacier: it is a most noble stream, and its origin is the grandest conceivable. Five valleys converge upon a single point, each pouring down a massive *névé*: all unite to form the trunk of the Aletsch. The Jungfrau, the Monk, the Eiger, the Trugberg, the Aletsch Horn, and other mighty masses are the collectors of the material. The mountain forms are beautiful, and laden with their snows smooth and shining

in the sunlight appear lovely beyond description. I was anxious to make some observations on the diathermancy of the lower atmospheric strata, and hence wished to make simultaneous observations upon a high summit and in a low valley. With this object in view the night before last I took lodgings in a wild mountain cavern, with a single hardy mountaineer at my side purposing to start early to make an attempt upon the highest mountain of the Oberland, – the Pinsteraarhorn. At 3 o'clock yesterday morning we were on foot, and at half past 10 we were on the summit of the mountain. I had sent Ramsay to the valley of the Rhone with a black bulb thermometer, but unfortunately the day was not sufficiently serene to give us information on the principal point in question. Water boils at 187° farht, on the top of the mountain. I left a minimum thermometer there, which some future tourist may read, and give us some notion of the cold attained in these high regions. I reached this hotel at half past seven yesterday evening; as fresh and well as could be expected after $16\frac{1}{2}$ hours as hard work as I ever went through. I am resting to day. Eight hours unbroken sleep have done much to restore my forces. The only effect I feel is a kind of pleasant drowsiness, which to day's rest will wear away, so that I shall be fit for a similar excursion tomorrow.

Will you kindly remember me to Mrs. Faraday and Miss Barnard. I have learned from a visitor here that Mr. Geo. Barnard is at Zermatt. I shall feel much obliged if you would ask Anderson to direct any letters that may be for me to Saas, Canton Valais, Switzerland.

Good bye | Ever Yours most truly | John Tyndall

1. Wilhelm Wertheim (1815–1861, P2). Austrian-born Professor at Montpellier.

2. Jules Duboscq (1817–1886, P3). Scientific instrument maker in Paris.

3. Rudolf Clausius (1822–1888, DSB). German physicist.

4. Tyndall was travelling with Andrew Crombie Ramsay (1814–1891, ODNB), the lecturer in geology at the Government School of Mines.

Letter 3496

Faraday to William Scrope Ayrton

9 August 1858

From the original formerly in the possession of the late Mr. and Mrs. Aida

Royal Institution | 9 Aug 1858

My dear Sir,

In my absence from town I fear your most kind letter has not been attended to & I am ashamed of that & hardly know how to apologize for unaware neglect. Nevertheless I thank you for it as warmly as if I had just received it & could make use of it to the full extent both of the house & the liberty which you offer me. I value both & the kindness which appear there very much. It so happens that before I left town, Mr. Becket[t] asked me to his house & I arranged to be with him for the first three days of the meeting¹ with Dr. Bence Jones. Longer than that I shall not be able to s[t]ay.

Believe me to be most grateful for your kindness & most truly Yours |
 M. Faraday
 W.S. Ayrton Esqr | &c &c &c

1. The Annual Meeting of the British Association in Leeds.

Letter 3497

Faraday to John Tyndall

10 August 1858

From the typescript in RI MS JT TS Volume 12, p. 4147

Royal Institution | 10 Aug 1858

My dear Tyndall,

We had your letter¹ this morning, and was very glad to hear of you, especially as we had some reason to think that Ramsay's² circumstances and trouble would derange you. I find you have both been together, but I suppose you will know before you receive this that he has been informed of the death of his mother³, i.e. if Anderson's answers to the enquiries here have caused the letter sent to him to arrive. It must be a great grief and disturbance to him. I have given Anderson your message, and I take two letters from the hall today, that I may send them to you with this. We arrived at home from Eastbourne two days ago, all well, and all desiring to be remembered to you kindly. My wife does not like losing the letter you had written. We have no news like yours to send you in return for your descriptions, but we can enjoy your letter very much, for we have been at so many of the places – not the summits that you talk of, but the bases of the elevations. I remember how weariness and illness laid hold of us at the little inn at the foot of the Rhone glacier⁴. I shall be very glad if you meet Mr. Barnard, but I doubt he will have left. Dr. Bence Jones is well and active. He had two of the Niger electrical fishes, and we had the luck of electrical shocks from them; since then they have gone off to Dubois Reymond⁵ and arrived safe. They must have been very hungry, nevertheless a couple of minnows that went in company with them arrived *alive and well* in Berlin. Mr. and Mrs. Barlow are off, and I suppose by this time near to Bad Homburg. We have been a good deal disturbed, since we came on, with the accounts of Mrs. Edwd. Forbes⁶. You have probably heard that she married in Edinburgh a while ago, but another lady claims the new husband as her husband, and I understand he admits a simulation of marriage with that lady, but denies the legality. The affair must be a sad one for all concerned⁷. Anderson says he sent off a letter to you at Zermatt, and hopes you have received it.

Ever, my dear Tyndall | Yours most truly | M. Faraday

I sent off Miss Moore's⁸ letter.

Address: Dr. J.T. | Poste Restante | Saas | Canton Valais | Switz



Plate 10. John Tyndall, Sarah Faraday, Julia Moore, Michael Faraday and Harriet Moore in 1858. From RI MS JT/8/5, p. 2.

1. Letter 3495.
2. Andrew Crombie Ramsay (1814–1891, ODNB). Lecturer in geology at the Government School of Mines.
3. Elizabeth Ramsay, née Crombie (c.1773–1858, ODNB under A.C. Ramsay). Ramsay's mother.
4. On 19 August 1841. Bence Jones (1870a), 2: 156–8.
5. Emil Heinrich du Bois-Reymond (1818–1896, DSB). Electro-physiologist. Associate Professor at University of Berlin, 1855–1858.
6. Emily Marianne Forbes, née Ashworth (d.1909, age 84, GRO under Avonmore). Married the geologist Edward Forbes (1815–1854, ODNB) in 1848. Married secondly, in 1858, the army officer William Charles Yelverton, 4th Baron Avonmore (1824–1883, CP).
7. On this see CP under Avonmore. The woman in question was the writer Maria Theresa Longworth (1827–1881, B2) whom Yelverton married in 1857 by Anglican rites in Scotland and later that year by Roman Catholic rites in Ireland. After nine years of litigation these marriages were found invalid.
8. Harriet Jane Moore (1801–1884, James (2001)). Painter and member of the Royal Institution, 1852–1881.

Letter 3498

George Herbert to Faraday

11 August 1858

From the original in GL MS 30108/3/90.13

Trinity House, London, EC | 11 Aug 1858

Sir,

I am directed to transmit to you, for your information, the accompanying Copy of a Letter from Major the Honorable Fitzmaurice, dated 2nd instant¹.

I am | Sir | Your most humble Servant | George Herbert
Professor Faraday | &c &c &c

1. Fitzmaurice to Berthon, 2 August 1858, GL MS 30108/3/90.12.

Letter 3499

William Edward Fitzmaurice to Faraday

12 August 1858

From the original in GL MS 30108/3/90.15

Hamilton Lodge | 12 Aug 1858

Dear Sir,

I have forwarded to you an *official* answer to the letter of the Board which I regret that I could not do sooner as I was busy at Cherbourg where I was highly complimented on the splendour of the light by Her Majesty, the Queen, and also the Emperor¹. The Lords of the Admiralty also expressed themselves very much pleased with brilliancy of the illumination, and the [sic] amongst the general public I understand it created more sensation than anything else there²[.] Considering the way I was hurried and thwarted in making my arrangements I was the most surpassed of any one there. I could have given ten times the effects if I had had my own way, however I was

under *orders* so there was no help; any how they were all pleased beyond measure so one could not expect more[.]

Yours faithfully | W.E. Fitzmaurice

1. On 5 and 6 August 1858 the Queen visited the naval fête at Cherbourg. See *Ill. Lond. News*, 21 August 1858, pp. 162, 180.
2. See Copcutt (1859).

Letter 3500

George Biddell Airy to Faraday

12 August 1858

From the original press copy in RGO6/377, f. 248

12 Aug 1858

My dear Sir,

Thank you for the Quartz threads, which arrived about a month ago, when I was going up the Tarantaise.

I returned from Switzerland five days ago. I missed Tyndall at the Grimsel by one day, for which I am sorry. But I visited the Unter Aar Glacier in my own way, and am equally interested and puzzled. I did not see any of Tyndall's lenticular masses of ice there¹, but I saw plenty on the Eismear of the lower Grindelwald Glacier.

I should like much to live for a time in Agassiz' cottage by the Aar glacier². Without some such near residence, little can be done.

I am, my dear Sir | Yours very truly | G.B. Airy
Professor Faraday

1. See Tyndall and Huxley (1857), 335.
2. On this see Agassiz (1885), 1: 299–300.

Letter 3501

James David Forbes to Faraday

14 August 1858

From the original in IET MS SC 2

Pitlochry, Perthshire | 14 Aug 1858

M. Faraday Esq | &c &c &c

My dear Sir,

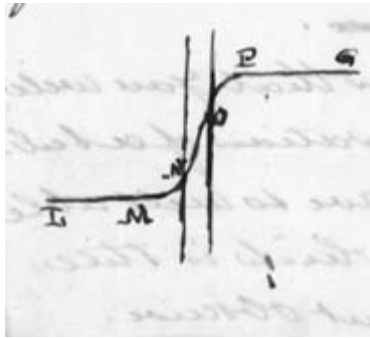
I was very much obliged to you for your letter of the 23d July¹ – & especially for the expressions of regard which it contains & which I can assure you that I heartily reciprocate.

I am also much obliged by your remarks on my little paper²; on which I shall not trouble you with more than a few additional words.

I hope however that you will resume consideration of a subject which you are so well able to illustrate & which is still to a certain extent obscure.

I cannot doubt that regelation takes place between Ice & Metals. The pile of shillings, though perhaps the simplest is not the only experiment I have tried; but is so easy that I hope you will repeat it. I have frozen in like manner a Bronze Letter presser of several pounds weight firmly to ice in a warm room: but this requires a long time.

I do not see any thing contradictory to the views which I have advanced, in the other experiments you mention. The finely trituated Ice – or Mr. Harrison's³ crystalline laminae when in contact with an indefinite mass of water



clearly belong to the portion of the curve in my figure between *N* and *O*, or to the physical boundary between water & Ice having a temp. intermediate between 31.7 and 32.0 & possessing the plastic quality proper to that intermediate state, just as in the case of wax, tallow, or fusible metal, when portions brought into contact by a gentle pressure become moulded into one another's substance by molecular cohesion. Further in the case you mention where a quantity of finely trituated particles of ice are taken in the hand and squeezed together, I seem to understand perfectly why "regelation" takes place:— You have drained or squeezed away all the *perfect water* from the mass, & the molecules of plastic ice ceasing to receive heat from the perfect water [which is operating a minute fusion at the surface of each particle *without communicating the smallest quantity of heat to its interior*]⁴ the condition of the Ice becomes that of more or less hard Ice not being in contact with perfect water, & this is what is called regelation.

I had last the pleasure of seeing you at Greenwich in Oct 1851⁵. Since the end of November in that year I have been more or less of an Invalid – at times severely ill – which will account for my comparative inactivity in matters of Science. I trust that your health is good & will long continue so.

Yours sincerely | J.D. Forbes

1. Letter 3485.
2. Forbes (1858).
3. James Harrison (c.1816–1893, AuDB). Scottish-born Australian inventor.
4. Square brackets in text.
5. Forbes was in London for the end of the Great Exhibition (Shairp *et al.* (1873), 351), while Faraday was staying in Blackheath (Faraday to Hawkins, 11 October 1851, letter 2466, volume 4).

Letter 3502

George Herbert to Faraday

14 August 1858

From the original in GL MS 30108/3/90.16

Trinity Houses, London, EC | 14 Aug 1858

Sir,

I am directed by the Elder Brethren to forward you the enclosed Copy of a Letter which they have addressed to Major Fitzmaurice, whereby you will perceive that that Gentleman has been requested to communicate in relation to his Oxyhydrogen Light, direct with you, and in accordance with the terms thereof I am to request that you will treat his Communications with strict confidence.–

I am | Sir | Your most humble Servant | George Herbert
Professor Faraday F.R.S. | &c &c &c

Letter 3503

George Herbert to Faraday

14 August 1858

From the original in GL MS 30108/3/90.17

Private

Trinity House | 14 Aug 1858

My dear Sir,

I find the minute of the Board requesting you to treat Major Fitzmaurice's communications with strict confidence has not been carried out, – I have therefore thought it most advisable with reference to our conversation yesterday, to send you the accompanying Letter¹ informing you of it,– and any observations you may think it right to offer will take their place in due sequence–

Always most faithfully yours | George Herbert

P.S. I will forward you the Copy of your Letter of the 21st ult.² to the Board, on Monday.
Professor Faraday F.R.S.

1. Letter 3502.

2. Letter 3484.

Letter 3504

Faraday to William Edward Fitzmaurice

16 August 1858

From the original copy in GL MS 30108/3/90.18

Royal Institution | 16 Aug 1858

Dear Sir,

I received your Official & your private¹ letters of the 12th instant in due time. I have waited this long before answering the latter because I wished to obtain a copy of my letter to the Society of the Trinity House, containing the points on which I desired to be informed if called upon for an advising opinion². I have not yet obtained it, but expect to do so to day & will send it to you[.] You will find the paragraphs numbered. I do not find that your official letter of the 12th gives any of the information wanted under paragraphs 2. 3 and 4; without which I can form no opinion. If when you receive my letter you have any further data to send me in writing, I shall be glad to consider them before I communicate to the Trinity Board[.]

You wrote "private" on the outside of your official communication, and "confidential" within. I will take care to respect these intimations as far as falls within my duty: but I can have nothing private or confidential *as regards the Trinity House* which is my chief. Whatever opinions I send to them I must accompany with the papers you send me. If therefore you wish anything held back from them send me another official answer and I will return you the one I have, marked "confidential"* . Our correspondence is indeed likely to become a little irregular, because your papers have not come to me through the Trinity House. You will feel that I cannot communicate any opinion I may form, to you:— I am bound to the Trinity house to whom I must communicate in confidence. I have no objection to your knowing my conclusions but the *Trinity House* is the fit judge of the use it may make of them, or the degree of confidence they may think they deserve, or the parties to whom they may choose to communicate them.

You will see that it is only after a careful consideration of the full answers to the many enquiries made in paragraphs 1. 2. 3. 4 & 5 of my letter and then only in consequence of a favourable or at least an uncertain result, that I should think a trial in a lighthouse as at Blackwall necessary. When such a trial is made it ought to be full including every arrangement that would be needed

at any lighthouse; such as Gas retorts – furnaces – gasometers – cylinders – pumps, pump motors &c in fact every thing required for a continuous light, supplied night after night for some weeks. It is only so that the liabilities of any proposed plan can be tested[.]

I write thus fully to prevent the necessity of future communication. Do not answer before you receive the copy of my letter which I am to procure from the Trinity house. Then send me *all* the information on which I am to form an opinion:– and I will send it on with my opinion to the Trinity House[.]

Very Truly Yours | M. Faraday
Honble | Major Fitzmaurice | &c &c &c

* returned by desire on the 20th aug

1. Letter 3499.
2. Letter 3484.

Letter 3505

Faraday to Peter Henry Berthon

20 August 1858

From the original copy in GL MS 30108/3/90.21–2

Royal Institution | 20 Aug 1858

Sir,

I am now able to reply to your letter of the 14th instant¹ regarding the lime light of Major Fitzmaurice. I received an official letter from him on the 12th after which I felt it necessary to write a letter in reply (a copy of which I send you), that my position might be made quite clear²[.] I sent him also a copy of my letter to you of the 21st of July³ in which I stated what information I should desire & I asked (as you will see) for that information especially as regarded paragraphs 2. 3. & 4 which his letter of the 12th did not contain. I have received from Major Fitzmaurice his final communication dated 18th August (having returned to him by his desire his letter of the 12th) and now send it to you with the accompanying remarks[.]

The letter of Major Fitzmaurice does not give me any information in answer to my enquiries which at all modifies my opinion respecting the uncertain applicability of the lime light in lighthouses. That the lime light is very intense & beautiful is manifest to every one & its application in lighthouses has been often thought of, but the numerous circumstances & precautions regarding the manufacture storing & application of two different gases the kind of machinery required, the peculiar knowledge necessary, the comparative delicacy of the arrangements compared to those now in use to which would now be added the employment of that volatile & combustible substance camphorated ether & the lonely & out of the way position of the most

important lighthouses are serious & opposing reason against any attempt to establish the light in a lighthouse until it has been wrought out perfectly on a full scale. I am not prepared to expect an applicable result satisfactory in all points; but if a discoverer says that he has & can realize such a result I should be very happy to see it done^[.] I do not at present believe in the superiority of Sulphate of lime calcined &c as a medium over quick lime – nor in the assumed value of the addition of the camphorated ether: these are points which need proof, and in such trials would obtain either it or correction.

As to the price of the light namely six pence per hour that would require verification during the trials. Major Fitzmaurice states that his medium power light consumes from 3 to 4 cubic feet of mixed gases per hour. A lime light which I have used consumed very nearly 4 cubic feet per hour and gave light equal to that of $14\frac{1}{2}$ oil argand lamps, that is to say it gave such light over *one half* of the horizon for as the lime is dark on one side and light only on part of the other it would require two if not three such lime lights to equal a central lamp equal to $14\frac{1}{2}$ oil Argand lamps. The cost & value of the relative lights however can only be deduced after they have been compared on a full scale^[.]

Major Fitzmaurice says in the 2nd paragraph of his letter that the lights can be practically applied as central lights for dioptric arrangements and it seems to me that this is the proper form for comparison with the Fresnel lamp both with & without lenses; but in such trial a single lime light will not be sufficient – the whole horizon must be filled as much as it is filled by the Fresnel lamp. I am not advising the Trinity House to make such a trial for I fear that even if the result promised well as a light, the numerous other circumstances before referred to would prevent its adoption. It is not as it appears to me the place of the Trinity House to *disprove* an insufficient plan but the duty of the Inventor to *prove* in all points a good & sufficient one. Nor should the Trinity House take upon itself in any degree the arrangement & conduction of the proof, since it would render itself liable to the charge of misrepresentation if the case failed – and might even unawares interfere with that perfect liberty & command on the part of the inventor which he might think necessary to his success. But the proof must in its nature be complete the mere exhibition of the light in a parabolic reflector is in my eyes of no lighthouse value⁴^[.]

I am Sir | Your Very Obedient Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letter 3502.

2. Letter 3504.

3. Letter 3484.

4. This letter was read to Trinity House By Board, 24 August 1858, GL MS 30010/41, p. 519. It was decided to refer this to the Deputy Master and Wardens.

Letter 3506

John Tyndall to Sarah Faraday¹

23 August 1858

From *The Times*, 3 September 1858, p. 10, col. e

23 Aug 1858

My dear —,

I now sit down to wipe away the reproach of having written a letter to you and not sent it². I reached this mountain wild the day before yesterday. Soon after my arrival it commenced snowing, and yesterday morning the mountains were all covered by a deep layer. It heaped itself up against the windows of this room, obscuring half the light. To-day the sun shines, and I hope it will soon banish the snow, for the snow is a great traitor on the glacier, and often covers smoothly chasms which it would not be at all comfortable to get into. I am here in a lonely house, the only traveller. If you cast your eye on a map of Switzerland you will find the Valley of Saas not far from Visp. High up this valley, and three hours above Saas itself, is the Distil Alp, and on this Alp I now reside. Close beside the house, a many-armed mountain torrent rushes; and a little way down a huge glacier, coming down one of the side valleys, throws itself across the torrent, dams it up, and forms the so-called "Matmark see". Looking out of another window I have before me an immense stone, the unshipped cargo of a glacier, weighing at least 1,000 tons. It is the largest boulder I have ever seen, is composed of serpentine, and measures 216,000 cubic feet. Previous to coming here I spent ten days at the Riffel Hotel, above Zermatt, and explored almost the whole of that glorious glacier region. One morning the candle of my guide gleamed into my room at 3 o'clock and he announced to me that the weather was good. I rose, and at 4 o'clock was on my way to the summit of Monte Rosa. My guide had never been there, but he had some general directions from a brother guide, and we hoped to be able to find our way to the top. We first reached the ridge above the Riffel, then dropped down upon the Görner glacier, crossed it, reached the base of the mountain, then up a boss of rock, over which the glacier of former days had flowed and left its marks behind. Then, up a slope of ice to the base of a precipice of brown crags; round this we wormed till we found a place where we could assail it and get to the top. Then up the slopes and round the huge bosses of the mountain, avoiding the rifted portions, and going zigzag up the steeper inclinations. For some hours this was mere child's play to a mountaineer, — no more than an agreeable walk on a sunny morning round Kensington-gardens. But, at length the mountain contracted her snowy shoulders to what Germans call a

kamus – a comb; suggested, I should say, by the toothed edges which some mountain ridges exhibit, but now applied to any mountain edge, whether of rock or snow. Well, the mountain formed such an edge. On that side of the edge which turns towards the Lyskamm there was a very terrible precipice, leading straight down to the torn and fissured *névé* of the Monte Rosa glaciers. On the other side the slope was less steep, but exceedingly perilous-looking, and intersected here and there by precipices. Our way lay along the edge, and we faced it with steady caution and deliberation. The wind had so acted upon the snow as to fold it over, forming a kind of cornice, which overhung the first precipice to which I have alluded. Our track for some time was upon this cornice. The incessant admonition of my guide was to fix my staff securely into the snow at each side, the necessity of which I had already learned. Once, however, while doing this, my staff went right through the cornice, and I could see through the hole that I had made into the terrible gulf below. The morning was clear when we started, and we saw the first sunbeams as they lit the pinnacles of Monte Rosa, and caused the surrounding snow summits to flush up. The mountain remained clear for some hours, but I now looked upwards and saw a dense mass of cloud stuck against the summit. She dashed it gallantly away, like a mountain queen; but her triumph was short. Dusky masses again assailed her, and she could not shake them off. They stretched down towards us; and now the ice valley beneath us commenced to seethe like a boiling cauldron and to send up vapour masses to meet those descending from the summit. We were soon in the midst of them, and the darkness thickened; sometimes, as if by magic, the clouds partially cleared away, and through the thin pale residue the sunbeams penetrated, lighting up the glacier with a kind of supernatural glare. But these partial illuminations became rarer as we ascended. We finally reached the weathered rocks which form the crest of the mountain, and through these we now clambered up cliffs and down cliffs, walking erect along edges of granite with terrible depths at each side, squeezing ourselves through fissures, and thus by jumping, swinging, squeezing, and climbing we reached the highest peak of Monte Rosa.

Snow had commenced to fall before we reached the top, and it now thickened darkly. I boiled water, and found the temperature 184.92 deg. Fahrenheit. But the snow was wonderful snow. It was all flower; the most lovely that ever eye gazed upon. There, high up in the atmosphere, this symmetry of form manifested itself, and built up the exquisite blossoms of the frost. There was no deviation from the six-leaved type, but any number of variations. I should hardly have exchanged this dark snowfall for the best

view the mountain could afford me. Still, our position was an anxious one. We could only see a few yards in advance of us, and we feared the loss of our track. We retreated, and found the comb more awkward to descend than to ascend. However, the fact of my being here to tell you all about it proved that we did our work successfully. And now I have a secret to tell you regarding Monte Rosa. I had no view during the above ascent, but precisely a week afterwards the weather was glorious beyond description. I had lent my guide to a party of gentlemen, so I strapped half a bottle of tea and a ham sandwich on my back, left my coat and neck-cloth behind me, and in my shirt-sleeves climbed to the top of Monte Rosa alone. When I see you I will tell you all about this ascent, which was a very instructive one. I expect to remain here a week. The house is cold, and at present the wet comes through the ceiling. I have caught a slight cold, which I hope will soon pass away, as I want all my vigour upon the ice. When I quit this place I shall make my way to Chamouni, where I expect to be eight or nine days. With kindest, &c.

Most sincerely yours | John Tyndall

1. For the identity of the recipient see letter 3512.
2. See letters 3495 and 3497.

Letter 3507

George Henry Bachhoffner to Faraday

24 August 1858

From the original in GL MS 30108/3/111.47

Royal Colosseum, Regents Park & Albany Street, London |
24 Aug 1858

Dear Sir,

I beg to send you the following statistics of the cost of 2 lime lights for a period extending from Jany. 1st to March 31st being 77 working days, total time of burning each day upon the average being 4 hours. I feel them to be unsatisfactory for any practical purpose without the photometric value that I am having a lime light attached to my Bunsens photometer¹ and should this investigation afford you any additional information I shall have much pleasure either in sending you the result or, should you like to go through them yourself I shall me most happy to see you any day you may appoint between 5 and 7.

I am, Dear Sir | Yours truly | Geo H. Bachhoffner
M. Faraday Esqr | D.C.L. F.R.S. &c

The total cost of generating oxygen and Hydrogen for 2 lime lights for 3 months, 77 working days each light burning 4 hours daily.

	£ s d
Manganeze & Chlorate Potash	53 3 3
Acid Sulph	9 14 3
Zinc Cuttings	12 2 .
Lime Cylinders	7 14 .
Coals	3 11 .
Labour	10 . .
Repairs & Sundries	6 . .
	2/102 4 6
	51 2 6

Cost of each light burning 77 days × 4 hours = 308 hours²

- 1. Reiset (1843).
- 2. Endorsed by Faraday here: or $\frac{3}{4}$ per hour each light.

Letter 3508
George Henry Bachhoffner to Faraday
26 August 1858
From the original in GL MS 30108/3/111.49

Royal Colosseum, Regents Park & Albany Street, London |
26 Aug 1858

My dear Sir,

I have much pleasure in sending you the result of my experiments on the relative photometric value and consumption of the lime light, compared with an argand coal gas burner consuming 6 cubic feet per hour, under a pressure of 10/10ths. I have not attempted to calculate the ratio of light or cost, thinking you would prefer, from the data given, to do so for yourself. I may add that every care was taken to ensure correct results and sincerely hope they may in some degree assist you in your investigation.

I am, my dear Sir, | Yours faithfully, | Geo H. Bachhoffner
M. Faraday Esq. D.C.L. F.R.S. &c

Time of observation 10 minutes. Temp. 61°Fah.

The oxygen gas holder is 3 ft. 3 in. in diameter and 4 ft. 4 in. high, having a head pressure of 10 in of water. This fell during the 10 minutes consumption $1\frac{1}{4}$ in. The other gas holder of the same dimensions charged with coal gas but, under a pressure of 13 in. of water fell during the 10 minutes exactly 1 in.

Bunsens Photometer¹ was employed to ascertain the illuminating power of the two lights, the spot disappeared upon the disc when the latter was at 86 in from the lime light and 17 in from the gas flame.

I find that 21 lbs of the bin-oxide of manganese yields a sufficient amount of oxygen under the above pressure to raise the gas holder exactly 8 in. – the cost of the manganese being 9/6 per cwt. The coal gas 4/6 per 1000 cubic feet. 10 cubic feet of which raised the gas holder exactly 10 in.

1. Reiset (1843).

Letter 3509

George Henry Bachhoffner to Faraday

28 August 1858

From the original in GL MS 30108/3/111.50

Royal Colosseum, Regents Park & Albany Street, London |
28 Aug 1858

Dear Sir,

I have again caused 10 cubic feet of coal gas to be pumped into the gas holder which produced the same elevation – viz – 10 in within a fraction. I do not remember if I mentioned to you that the gas *holder* is *not counter poised* and must weigh with its gearing at least 3 cwt – minus its loss by immersion in the water of the tank – and has to be lifted by the gas in addition to the 13 in column head of water – these observations apply only to the coal gas holder, as I have no means of testing the oxygen gas holder in the same way.

I am, my dear Sir | Yours faithfully | Geo H. Bachhoffner
M. Faraday Esq D.C.L. F.R.S. &c

Letter 3510

Faraday to William Starie¹

31 August 1858

From the incomplete original copy in IET MS SC 3

Royal Institution | 31 Aug 1858

My dear Sir,

The progress of time & of the House at Hampton Court has certainly brought nigh the period at which I feel bound to thank you for the very kind manner in which you have carried out the Queens wishes as expressed in the messages her Majesty sent to me. I hope to enjoy the house much & I feel that I shall very often & in every part of it be reminded of the care & thought

you have bestowed upon it[.] I know very well that you may say that you have only been performing an agreeable duty but the extent & manner of performing it adds a great charm to the many which surround this unsought for gift from the Queen[.] We hope to go in next week having received the Warrant from Lord De la Ward [sic]²:- & as that document promises us the pleasure of the Green house

on which we count much[.] We hope through you to enjoy them fully next year.

I am | My dear Sir | Very Truly Yours | M. Faraday
Starie Esqr | &c &c &c

1. William Starie (d.1875, age 65, GRO). Assistant Surveyor of Works. *Royal Kalendar*, 1858, p. 163.

2. Letter 3457.

Letter 3511

Charles Anderson and Faraday to John Tyndall

31 August 1858

From the typescript in RI MS JT TS Volume 12, p. 4148

Royal Institution | 31 Aug 1858

Sir,

A letter was left at the Institution by post for you on Saturday morning¹ on Her Majestys Service, I opened it and found a cheque in it and sent it in the afternoon to Mr. Francis, all here are very glad to hear that you are so strong and in good health. We hope you will long continue so. Every thing is very quiet here at present and we are getting on with the repairs².

Your Humble Servant at all times, | C. Anderson

My dear Tyndall, as this is all the paper that can go, I that [sic] one side from Anderson just to shew myself. We all think of you and are very glad you are so strong.

Ever yours, | M. Faraday

1. That is 28 August 1858.

2. See RI MM, 5 July 1858, 11: 248.

Letter 3512

Faraday to John Tyndall

2 and 3 September 1858

From the typescript in RI MS JT TS Volume 12, p. 4149

Royal Institution | 2 Sep 1858

My dear Tyndall,

I found Anderson a day or two ago about to write to you on a little bit of paper just passable by the post, and I made free with one side of it¹. In the

uncertainty of knowing where you might be found, perhaps I might not have written to you again, but for the receipt of your letter by my wife², detailing the ascent of Monte Rosa, and the enormous indiscretion I have committed thereupon. What shall I say? I have sent it to the Times. There, the whole is out. I do not know whether to wish it may appear tomorrow or next day or not. If you should dislike it, I shall ever regret the liberty I have taken. But it was so interesting in every point of view, shewing the life and spirit of a philosopher engaged in his cause: shewing not merely the results of the man's exertions, but his motives and his nature:— the philosophy of his calling and vocation, as well as the philosophy of his subject; that I could not resist, and I was the more encouraged to do so because, from the whole character and appearance of the letter, it shewed it was an unpremeditated relation and that you had nothing to do with its appearance, i.e. it will shew that if it should appear. Now I hope you forgive me. Nobody will find fault with me but you. It came too late for the Phil. Mag., but if the Times does not put it in, I shall send it to the Phil. Mag. However as this is only the 3rd of the month, there is time enough for that.

I won't give you any scolding. I dare say my wife will, when you see her: "êtes-vous marié", indeed! I cannot but feel glad you have done it now it is done, but I would not have taken the least portion of responsibility in advising you to such a thing.

I have no philosophy and no news for you. I feel just out of the world – forgetful, and dull headed in respect of science and of many other things – but well and content, as I have great reason to be. My wife and Jane are pretty well: the latter absent, or she would send her remembrances with ours. I shall send this to Chamouni on the chance of catching you.

Good bye, my dear friend | Ever truly yours | M. Faraday

Friday morning, 3rd. The letter is there.

Address: Dr. Tyndall | Chamounix | Savoie | Switz

1. Letter 3511.

2. Letter 3506.

Letter 3513

Faraday to the Editor of The Times

2 September 1858

From *The Times*, 3 September 1858, p. 10, col. e

Royal Institution | 2 Sep

Sir,

The following letter¹ from a most earnest and philosophic investigator of the glaciers is so interesting that I thought you would like to use it for the

advantage of a certain class of your readers, and therefore send it for you to deal with as you may think fit.

I am, Sir, your most obedient servant | W. Faraday [sic]

1. Letter 3506.

Letter 3514

John Tyndall to Faraday

11 September 1858

From the typescript in RI MS JT TS Volume 12, pp. 4087–90

Chamouni | 11 Sep 1858

My dear Mr. Faraday,

I see “it is all out”¹, for I have just read it². There is nothing *very* stupid in it, and it gives me pleasure to think that you considered it sufficiently interesting to be made public. For the little bit at the commencement³ I thank you much. I was puzzled two or three days ago on opening a letter from General Portlock⁴, to find the first words of it referring to my letter to you published in the Times. Beside his letter came your own⁵ explaining all. The only difference it makes is that instead of the single lecturing of Mrs. Faraday, I shall have half a dozen ladies solemnly admonishing me. But I can patiently bear any amount of lecturing from ladies, and so on this score I am not very much disturbed.

I came here soon after I wrote to Mrs. Faraday, but the weather for a long time proved obstinately bad. Heavy rain in the valleys and heavy snow on the mountains. I came to look at the Mer de Glace once more, and to see whether hints obtained upon other glaciers this year were illustrated upon it. The fresh snow, however, on the upper portions of the glacier disguises the structure of the ice, and renders observations difficult. It was also my intention to assist the eminent guide Auguste Balmat in placing some thermometers at the summit of Mont Blanc. We have not a single observation to show either the minimum winter temperature, or the depths to which the cold of winter penetrates the ice at the summit. Zaupire⁶ has some *conjectures* upon this latter point, but we have no direct observations. The weather however opposed itself to this expedition – aided and abetted, I am sorry to say, by the *guide chef* at Chamouni, who attempted to impose on me, in all their rigour, the regulations which have been made for *tourists*. He would not permit me to have a boy to carry a little instrument up the Mer de Glace – I must take a *guide*. He also opposed himself to my concerted ascent with Balmat, and declared that I must conform to the rules and take *four* guides. I vainly endeavoured to shew him the difference between my position and that of a tourist; or to make him understand that the works of scientific men had done more for Chamouni

than hundreds of ordinary travellers. To their credit, however, be it spoken, his superiors think a little differently upon the subject from the *Guide Chef*; The "Intendent" of the Province has told him that in the case of a man of science he must interpret the laws widely and liberally, and must not attach to them a "Judaical" signification. Having come to this conclusion that the bad weather offered an insuperable barrier to the ascent of Mont Blanc I went the day before yesterday with Balmat to the glacier du Taléfre, and at a height of about 10,000 above the sea we sank a thermometer in the ice. We found it excessively hard and difficult to pierce. A second thermometer was placed beside a rock which forms the summit of the Jardin, so as to give the minimum temperature of the *air*. Next year Balmat will ascend and read the result. I had never seen the wonderful circus of the Taléfre so wonderful, rendered so by the glorious weather, which has suddenly changed, and the heavy fresh snow which covered the surface of the glacier and rolled incessantly in avalanches from the surrounding mountains. During portions of our little expedition we had to plod through snow nearly three feet deep. (I had a most intelligent companion in Mr. Wills⁷, who as you know has written an interesting little book upon the Alps⁸). The weather at present is magnificent, but our thermometers are disposed of. Balmat possesses one of his own, which, though not graduated low enough to give us the temperature of the air, might tell us something regarding the depth to which the winter cold penetrates the ice. Balmat himself conceives the idea of ascending Mont Blanc for the sole purpose of making his observation. I learned this last year, and made it known to the Council of the Royal Society, recommending the enterprise as one worthy of assistance and encouragement. The Council promptly voted me a small sum out of the government grant⁹; but as for personal remuneration Balmat steadily refused it. He affirms that he was actuated by no hope of pecuniary reward when he conceived the idea of placing the thermometer at the summit, and that he will not now accept such recompense. It gives me pleasure to make known to you the spirit which actuates this brave, gentle, and independent Chamouni guide, who never once shuned fatigue or danger if a scientific object was to be gained by encountering it. He has ascended in winter through the snow to the Mer de Glace, and observed the motion of the boulders upon the glacier. These observations, which are recorded in the excellent papers of Professor Forbes¹⁰, are the most important, if not the only ones, that we possess, as to the influence of the seasons upon glacier motion. I think we must help him to carry out his idea. The observation will not be a complete one, but it will teach us something, and others may be associated with it to repay the ascent. Thus matters stand at present, and a day or two will decide whether the mountain is to feel this year the shock of a crowbar upon his head.

Remember me most kindly to Mrs. Faraday and Miss Barnard
And believe me always | Most sincerely Yours | John Tyndall

Would you have the goodness to have the enclosed posted for me?

Like many other apparently 'impractical' things I think the climbing tendency of Englishmen might be turned to profitable account. There are many men of intelligence and culture among these mountain climbers who would be rejoiced to lend a hand in making scientific observations.

Water boils here at 194.6 Faht.

Chamouni, 11th, Sep, 1858.

1. See letter 3512.
2. Letter 3506 in *The Times*.
3. Letter 3513.
4. Joseph Ellison Portlock (1794–1864, ODNB). Officer in Royal Engineers and member of the Council of Military Education.
5. Letter 3512.
6. The typescript at this point has been annotated 'Saussure?'.¹
7. Alfred Wills (1828–1912, WWW1). Lawyer and alpinist.
8. Wills (1856).
9. This was requested in Tyndall to Sabine, 10 February 1857, RS MS MC 5.275 and granted RS CM, 2 April 1857, 2: 386.
10. For example Forbes (1851).

Letter 3515

Faraday to Peter Henry Berthon

13 September 1858

From the original copy in GL MS 30108/2/87

Hampton Court | 13 Sep 1858

Sir,

I have examined the apparatus intended for the Needles lighthouse. It is so like in character to that for the Bishops rock recently reported upon¹ that I have nothing to add except that the remarks I then had occasion to make respecting the adjustment of the parts of the central bands have here no application – all is in excellent order & the apparatus unexceptionable[.] It of course has the green colour of the former; but with that it is to be presumed that it has the extra hardness & the relative freedom from Striae[.]

I take the liberty of stating that it would be a very great advantage in the examination of these apparatus if they could be placed on a revolving platform at one end of the room[.] This would give the greatest distance possible & allow of the examination of every part in turn. The platform would be moveable need not be of larger diameter than the apparatus & not more than from 4 to 6 inches in height if indeed so much²[.]

I have the honour to be | Sir | Yours Very faithfully | M. Faraday
P.H. Berthon Esqr | &c &c

1. Letter 3483.
2. This letter was read to Trinity House By Board, 14 September 1858, GL MS 30010/41, p. 543. It was agreed that the apparatus would be installed in the Needles lighthouse.

Letter 3516**Faraday to Miss Pinney¹****15 September 1858****From the original in Burndy Library, Massachusetts Institute of Technology**

Royal Institution W | 15 Sep 1858

My dear Miss Pinney,

I am very sorry that my three next Proxies are promised in succession²[.] I will place the card you sent me fourth but I hope the Candidate will be successful before I shall be able to help him[.]

Ever Your faithful Servant | M. Faraday

1. Unidentified.

2. This letter refers to the provision a place for an orphan in the London Orphan Asylum in Clapton, founded in 1813, of which Faraday became a subscriber in 1831. See Faraday to Roberts, 24 January 1832, volume 2, letter 533. Each subscriber had one or more votes (depending on the size of their subscription) for suitable candidates for a place in the orphanage. Votes could be transferred by proxy between subscribers. For accounts of the London Orphan Asylum see Alvey (1990) and Bache (1839), 58–65. Miss Pinney is not listed as a subscriber in the records of the Asylum in SuRO, but there was no prohibition on non-subscribers seeking to place an orphan in the Asylum.

Letter 3517**Faraday to John Phillips¹****23 September 1858²****From the original in UMO MS John Phillips Papers**Kirkstall Grange, Leeds³ | Thursday

My dear Phillips,

My head is too full of aches & my memory too false for me to endeavour to hold & compare all the relations of Mr. Mercers⁴ paper⁵. You must give it to some other of the formalists.

I am afraid I shall not see you[.] I sought all to day for you in vain & I must return on Saturday mornng⁶. Kindest remembrance to Miss Phillips⁷[.]

Ever Yours | M. Faraday

1. John Phillips (1800–1874, ODNB). One of the founders of the British Association and its Assistant General Secretary until 1859.

2. Dated on the basis of Faraday's attendance at the Annual Meeting of the British Association in Leeds.

3. The residence of William Beckett with whom Faraday stayed during the meeting. See letters 3482 and 3496.

4. John Mercer (1791–1866, ODNB). Chemist.

5. Mercer (1858).

6. That is 25 September 1858.

7. Anne Phillips (1803–1862, see ODNB under J. Phillips). Sister of John Phillips for whom she kept house from 1829.

Letter 3518

Arthur-Auguste De La Rive to Faraday

25 September 1858

From the original in IET MS SC 2

Geneve le | 25 7bre 1858

Mon cher & excellent ami,

Il me tarde de répondre à votre bonne lettre de ce printemps¹, & de vous en remercier. Quoique je n'aie eu aucune communication directe avec vous depuis cette époque, je n'ai pas moins été au courant de vos nouvelles par plusieurs amis communs, & en particulier par M. Hoffman² que j'ai eu le plaisir de voir il y a quelques jours. Je sais que vous allez bien, grâce à Dieu, & que vous êtes toujours aussi jeune d'esprit & de corps. J'espère que votre tête ne vous fait pas souffrir; il me semble que depuis quelques années elle va aussi bien physiquement qu'intellectuellement. Dieu vous maintienne le plus long-temps possible dans cet état satisfaisant qui est une bien grande douceur pour vos amis.

Les enfants de notre bonne amie Madame Marcet m'ont demandé de faire une petite biographie de leur mere³. Je m'occupe de ce travail qui est toujours un travail délicat & difficile. Il y a un point sur lequel vous pouvez me donner une information, & je viens vous prier d'avoir la bonté de me la donner. Est-il vrai que c'est la lecture des *Conversations sur la Chimie*⁴ qui vous a inspiré le premier gout pour la Chimie & la Physique & a déterminé la direction de vos travaux? Si cela est vrai, m'autorisez-vous d'en faire mention dans ma notice sur Madame Marcet? Voilà les deux questions auxquelles je vous demande de vouloir bien me répondre; vous me rendrez un vrai service.

Un autre service que je vous demande encore, ce serait, si cela vous est possible, de donner à mon jeune ami Monsieur Alexandre Prevost⁵, l'indication des Constructeurs d'instruments d'optique qui pourraient lui fournir des informations sur la quantité de verre optique de Daguet⁶ qu'on consomme en Angleterre. Monsieur Daguet va probablement transporter son établissement à Genève, & les personnes qui s'intéressent à cet établissement qui sera important pour notre pays, tiendraient à avoir ce renseignement. J'avais engagé Monsieur Prevost à recourir à complaisance pour cet objet; peut-être l'a-t-il déjà fait. – En tout cas je vous remercie d'avance des facilités que vous pourrez lui fournir pour cet objet. Il ne s'agissait que de lui donner peut-être une introduction auprès des personnes compétentes à cet égard, & personne ne peut mieux le faire que vous.

Pardonnez moi mon indiscretion, Monsieur & cher ami; vous y êtes accoutumé, voilà tout ce que je puis vous dire pour mon excuse; ma femme me charge de ses meilleurs compliments pour Madame Faraday & pour vous & je vous prie de recevoir l'expression de mes sentiments les plus affectueux

Auge. de la Rive

Veillez, quand vous verrez M. Tyndall, lui exprimer tous mes regrets de ce que je ne l'ai pas vu à son passage à Genève; je lui écrirai incessamment.—

TRANSLATION

Geneva | 25 Sep 1858

My dear and excellent friend,

I have been slow to answer your kind letter of last spring¹, and to thank you for it. Although I have had no direct communication with you since then, I have nevertheless been kept up to date with your news through several mutual friends, & in particular through Mr. Hofman² whom I had the pleasure of seeing a few days ago. I know that you are well, thanks be to God, and that you are still as young in mind and body. I hope that your head does not make you suffer; it seems to me that for several years it has been in as good a state physically as mentally. May God maintain you for as long as possible in this agreeable state which is a very great consolation to your friends.

The children of our good friend Mrs. Marcet have asked me to write a short biography of their mother³. I am engaged on this work which is always a delicate and difficult task. There is a point on which you can furnish some information, & I would like to ask you to have goodness to give it to me. Is it true that it was the reading of *Conversations on the Chemistry*⁴ which inspired you with your first taste for Chemistry and Physics and determined the direction of your work? If it is true, would you allow me to mention it in my note on Mrs. Marcet? Those are the two questions to which I ask you kindly to respond; you will render me a great service.

Another service that I would ask of you, would be, if it is possible, to give to my young friend Mr. Alexander Prevost⁵, an indication of the constructors of optical instruments who could provide him with some information on the quantity of optical glass made by Daguet⁶ that is used in England. Mr. Daguet is probably going to transfer his establishment to Geneva, and the people who are interested in this establishment, which will be important for our country, are keen to have this information. I had asked Mr. Prevost to kindly appeal to you for this; perhaps he has already done so. — In any case I thank you in advance for any information that you will be able to provide concerning this. It may only be a question of perhaps introducing him to persons competent in this matter, and no one can do that better than you.

Please forgive my indiscretion, Sir and dear friend; you are accustomed to it, that is all I can use as my excuse; my wife asks me to convey her best compliments to Mrs. Faraday & to you & I ask you to receive the expression of my most affectionate sentiments

Auge De La Rive

When you see Mr. Tyndall, kindly express to him my regrets that I did not see him during his journey to Geneva; I will write to him shortly.

1. Letter 3445.
2. August Wilhelm Hofmann (1818–1892, ODNB). Professor of Chemistry at the Royal College of Chemistry, 1845–1865.
3. De La Rive (1859).
4. Marcet (1809).
5. Alexandre Pierre Prevost (1821–1873, P2, 3). Swiss physiologist.
6. Théodore Daguet (1795–1870, DHS). Swiss glass maker.

Letter 3519

Faraday to Arthur-Auguste De La Rive

2 October 1858

From the original in BPUG MS 2361, f. 89–90

Hampton Court | 2 Oct 1858

My dear friend,

Your subject¹ interests me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of 13, in the year 1804, remained there 8 years, and during the chief part of the time bound books. Now it was in these books, in the hours after work, that I found the beginnings of my philosophy. There were two that especially helped me; the *Encyclopaedia Britannica*, from which I gained my first notions of Electricity²; and Mrs. Marcets conversations on chemistry³, which gave me my foundation in that science. I believe I had read about phlogiston⁴ &c in the *Encyclopaedia*, but her book came as the full light in my mind. Do not suppose that I was a very deep thinker or was marked as a precocious person;— I was a very lively, imaginative person, and could believe in the *Arabian nights*⁵ as easily as in the *Encyclopaedia*. But facts were important to me & saved me. I could trust a fact,— but always cross examined an assertion. So when I questioned Mrs. Marcets book by such little experiments as I could find means to perform, & found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge & clung *fast* to it. Hence my deep veneration for Mrs. Marcet; first as one who had conferred great personal good & pleasure on me;— and then as one able to convey the truths and principles of those boundless fields of knowledge which concern natural things to the young, untaught, and enquiring mind.

You may imagine my delight when I came to know Mrs. Marcet personally;— how often I cast my thoughts backward delighting to connect the past and the present;— how often when sending a paper to her as a thank offering I thought of my first instructress;— and such like thoughts will remain with me⁶[-]

I have some such thoughts even as regards *your own father*⁷:- for when, later in life, I was first at the Royal Institution and then abroad with Sir H. Davy, your father was one of the very earliest, I think I may say *the first*, who personally, at Geneva, and afterwards by correspondance, encouraged and by that sustained me.

Though I have not seen M. A. Prevost⁸ I have had a letter from him & written a reply. I am afraid I shall be very useless for I have no knowledge of the opticians & am without the information he wants. An alien as regards Society, & of very bad memory, I cannot either pick up, or lay up, information of that kind; but thought the Dollonds⁹ might inform him.

My wife desires her kindest remembrances to you & Madame De la Rive. She keeps pretty well but cannot walk many yards. We are now at Hampton Court, in the house which the Queen has given me. We shall use it in the summer months, & go into town in the cold weather & the Season. I believe it will be a comfortable pleasure for the few years that remain of life;- but hope for a better house shortly;- and we may do that without presumption, seeing through whom it is that we obtain right to such a hope[.]

Ever My dear friend | Yours Affectionately | M. Faraday
M. A. de la Rive | &c &c &c

1. In letter 3518.

2. [Tytler] (1797).

3. Anon (1797) and the cross-references therein.

4. Marcet (1809).

5. The Arabian Nights had been translated into English and published in five volumes in 1802.

6. These recollections were published in De La Rive (1859), 453-4.

7. Charles-Gaspard De La Rive (1770-1834, DSB). Swiss chemist.

8. Alexandre Pierre Prevost (1821-1873, P2, 3). Swiss physiologist.

9. George Dollond olim Huggins (d.1866, age 68, GRO under Dollond). Optical instrument maker of 59 St Paul's Churchyard, Clifton (1995), 86-7. William Dollond (d.1893, age 67, GRO) also an optician.

Letter 3520

Faraday to John Barlow

4 October 1858

From the original in RI MS F1 E11

Royal Institution | 4 Oct 1858

My dear Barlow,

Herewith are the letters I had from Dr. Bence Jones - it is a pity that they came into my hands for I have forgotten them two or three times & if I remember rightly I heard of a call that J. Lacaita¹ made here whilst I was away² - I hope no harm will have happened through my carelessness[.]

As to the Christmas lectures I will look at the notes I wrote out for last year³ & then had to put aside - I will give them if I can - and if I break down I break down⁴ - but the time for lecturing is fast passing with me.

I grieved that I could not speak to Mrs. Barlow at Leeds⁵ but really time & circumstances seemed to come & go like clouds. Our kindest remembrances if you please to her.

Also to Miss Coutts & say that I saw the gentleman she wrote of though only for a moment.

Ever dear Barlow | Yours truly | M. Faraday

1. James Philip Lacaita (1813–1895, ODNB). Italian-born politician and scholar.
2. Lacaita had delivered courses of lectures on various Italian topics in 1857 and 1858 and would do so again in 1859. RI MS Le4/200, 211 and 221 respectively.
3. Faraday's notes for his 1857/8 Christmas lectures on static electricity are in RI MS F4 J18.
4. Faraday delivered the 1858/9 Christmas lectures on metals. His notes are in RI MS F4 G61.
5. At the Annual Meeting of the British Association.

Letter 3521

Faraday to the Commissioners of Woods and Forests

18 October 1858

From the original copy in IET MS SC 2/3/7

Hampton Court | Green | 18 Oct 1858

My Lords & Gentlemen,

I wish to place some fire bricks within those of the stoves of the house which Her Most Gracious Majesty has been pleased recently to grant to me, but am informed that I may not do so without leave from the Board of Works, though at my own expence. May I be allowed to do so? It is simply the introduction of so much brick instead of so much coal.

There is a garden attached to the House; it is small, but a part of it is laid out as a Kitchen Garden. I wish to change this part into grass plat. I understand this can not come under the term *repairs* as used in the second letter from Her Majesty to me¹; and therefore propose to do it myself. May I do so?— and in that case, may I be allowed to have turf from the Parks, which I understand is sometimes granted, as it would be an economy to me. The quantity required would be but small.

If (in reference to an application I was induced to make respecting a Garden chair) there has since been an alteration of the rule that we should be admitted to the Palace Gardens may I be allowed the advantage of the alteration? I refer to the permission again, only because I have seen chairs in the Gardens several times since I had the honor of writing to the Board.

I am ashamed to trouble the Board with such questions as these, especially the first of these; but am instructed that it is quite necessary & proper[.] I am, above all things, most anxious not to be intrusive, unawares.

I have the honor to be | My Lords & Gentlemen | Your most humble
Servant | M. Faraday

To the | Commissioners of Woods, & Forests | &c &c &c | Office of Works

1. Letter 3421.

Letter 3522**Alfred Austin to Faraday****29 October 1858****From the original in IET MS SC 2/3/7****4312**

Office of Works, &c, SW | 29 Oct 1858

Sir,

The First Commissioner of Her Majesty's Works, &c.¹, has had before him your letter of the 18th. Inst.² and I am directed by him to inform you that he has no objection to your placing some Fire Bricks in three of the Stoves of the House occupied by you on Hampton Court Green and converting the Kitchen Garden of that House into Grass Plat at your own expense but that he cannot allow any turf to be taken from the Park for the purpose.

I am also directed to inform you in reply, to your enquiry that no new regulation has been made in regard to the admittance of Invalid Chairs into the Palace Gardens since the date of the First Commissioner's letter to you of the 11th. Ult³. In that letter it was omitted to be stated that the privilege was allowed to Residents in the Palace alone. Under the circumstances, however, of your occupying your house by grace and favor of Her Majesty the First Commissioner thinks it reasonable to make your case an exception to the general rule; and he has accordingly given orders for Mrs. Faraday's Chair to be admitted into the Gardens.

I am | Sir | Your Obedient Servant | Alfred Austin | Secretary

Endorsed by Faraday: Board of Works | Application & answer | Oct. 1858
| Chair – turf – &c

1. Lord John James Robert Manners (1818–1906, ODNB). Conservative MP for North Leicestershire and First Commissioner of Works, 1858–1859.

2. Letter 3521.

3. Not found.

Letter 3523**Christian Friedrich Schoenbein to Faraday****c.2 November 1858¹****From the original in UB MS NS 439**

My dear Faraday,

As Doctor Bernoulli², a former pupil of mine is going to London and from there to Guatemala, I make use of this opportunity to send you through my young friend amongst other memoirs that paper, in which I have treated the reciprocal Katalysis of a number of oxy-compounds³. You may give the "fasciculum" to a scientific friend, who happens to be master of the german tongue. The little parcel joined, you will be good enough to forward it to its place of destination.

It is not long since I returned from a journey undertaken to the south-west of Germany, which has turned out highly pleasant and interesting to me. First I attended the meeting of german philosophers held at Carlsruhe in the middle of September last⁴, which was the most numerous and brilliant one, I have as yet had the good luck of attending. With a very few exceptions all the leading scientific men of Germany were present: Liebig, Woehler, Bunsen, Magnus, Dove⁵ and a host of others. Under such "auspiciis" the meeting could not but be excellent. All sorts of honors and attentions were showered down upon us from the grand duke⁶ and his young amiable duchess⁷ (the sister of the husband⁸ of your princess⁹), the government and magistrates down to the very lowest inhabitants of the capital. I think indeed, that science has very seldom been so much honored in its representatives, as it was the case at Carlsruhe some weeks ago.

Both their Royal Highnesses, all the Ministers, a number of political notabilities and the chief Magistrate of the Metropolis attended all the general meetings holding out from the beginning to the end. No less than three times we enjoyed the hospitality of the reigning duke, supping, dining and taking tea with the court. Of other festivities there was no want: the finest plays were acted before the learned audience, splendid balls given in honor of the philosophers, the town of Baden-Baden in the beautiful ruins of the magnificently situated old castle treated the association in a sumptuous style and the good people of Durlach invited us to enjoy their delicious grapes in their vine-yards, celebrating, what we call a "Wintzerfest" (vintage-feast) in which beautiful young Ladies of the town, clad in white, offered in a graceful and highly engaging manner the choicest fruits of the Land to the philosophers present, the number of whom was very great indeed at least five or six hundred. In music-loving Germany nothing can be done without songs and other musical performances, and certainly we had plenty of them along with patriotic toasts and other manifestations of joy at Carlsruhe, Baden and Durlach. The people on the other side of the water have hardly a notion of the teutonic enjoyments and the comfortable ease, in which those things are done. Am I right or not, if I say, that pleasure is a sort of business to the majority of the English and the enjoyment of it too much ruled by the codex of "bienséance" the statutes of which are too much in favor of formalities and ceremonies. But every nation may have its own ways and whims and after all "*de gustibus non est disputandum*"¹⁰.

After having been fully satiated by intellectual and bodily pleasures at Carlsruhe, I took a trip with Liebig, Rose¹¹ and some other philosophers to see some interesting establishments in the country and then tempted by the glorious weather of Autumn and the seducing neighbourhood of the finest scenery of the Rhine I loungered about in the classical regions of the history of the Rhine, visiting many an old friend and drinking more than one glass of old Hock. One Excursion was most particulare beautiful: With a couple

of friends I descended from Mayence to Bingen and arrived there all of us, devout reverers of father R[h]ine, went up to the Chapel Saint Rochus emptying there in honor of his Majesty a bottle or two of his most generous and incomparable nectar. Those heights afford one of the most picturesque views along the Rhine. I won't tell you any more about my idle ramblings, suffice it to know, that they proved delicious and that Mr. Schoenbein was "joliment" scolded by Mrs. Schoenbein on account of his very long outstays. By this time I have entered the career of every day life and shall, before long, live again in the consortium of my chemical hero, whose interior nature I want to know much better, than I do now. You have no doubt enjoyed a tranquil and pastoral country-life at Hampton court and I confidently hope, that Mrs. Faraday's health has been much benefited by it. Miss Schoenbein is, as far as I know doing well at Stamford Hill and continues to like her stay in England.

Expecting to hear soon of and from you and asking you the favor to present my humble respects to Your Lady

I am, my dear Faraday | for ever your's | C.F. Schoenbein
Pray be kind to the bearer of this letter, written in a hurry.

1. Dated on the basis that letter 3531 is the reply.
2. Carl Gustav Bernoulli (1834–1878, Meyer-Holdampf (1997)). Swiss physician and explorer.
3. Schoenbein (1858d).
4. That is the Gesellschaft Deutscher Naturforscher.
5. Heinrich Wilhelm Dove (1803–1879, DSB). Professor of Physics at Berlin.
6. Friedrich I (1826–1907, NDB). Grand Duke of Baden, 1856–1907.
7. Luise-Marie-Elisabeth (1838–1923, NDB under Friedrich I). Grand Duchess of Baden, 1856–1907.
8. Friedrich III (1831–1888, NDB). Crown Prince of Prussia and, later, Emperor of Germany, from March to June 1888.
9. Victoria Adelaide Mary Louisa (1840–1901, ODNB). Eldest child of Queen Victoria and Prince Albert. Princess Royal and later Empress of Germany who married Crown Prince Friedrich on 25 January 1858.
10. 'There is no disputing about tastes'.
11. Heinrich Rose (1795–1864, DSB). Professor of Chemistry at Berlin University from 1835.

Letter 3524

Charles Frederick Winslow to Faraday

5 November 1858

From the original in IET MS SC 2

West Newton, Mass | 5 Nov 1858

My dear Sir,

Your valuable note dated at London April 26th¹ was received & I felt highly flattered by it. Notwithstanding your conclusions are not understood nor appreciated by astronomers & mathematicians – & because my doctrine of cosmic repulsion has been treated more contemptuously than by mere

coldness by several opinionated mathematicians here, who could not see how cosmic repulsion could exist without destroying the force of gravitation, I value your encouraging words very highly.

The subjects of researches such as yours & mine, are very abstruse for common thinkers, & especially to astronomers educated strictly in the Newtonian faith do they seem like unnecessary innovations. These latter as teachers, think but little, & communicate to others what they were severally taught: To think anything else would be heresy. They never think how rejoiced Newton would have been to be surrounded by such light & accumulated physical knowledge as we possess in these days. Nor what progress he would himself make on his own discoveries. Little by little however the world will wake up, & even astronomers like Arago in his last days will question the universe for *the other great secret* which has been so long hidden beneath the glare of the sublime Newtonian discovery.

The recent comet² will greatly enlarge our knowledge of the forces at work within cosmic masses. Seen through the great equatorial telescope at Cambridge, as related to me by the Elder & younger Bond³, the translucent sphere was in a violent state of agitation, throwing up constant disruptions from its central parts toward the sun & these luminous outbreaks were then swept off behind the nucleus, that is in lines away from the sun as if some repulsive force proceeded from the sun which seized upon them & swept them off in rapid currents into space to produce luminous appendages. The elder Bond even assured me that it seemed as if some other force besides that of gravitation did exist in space & that it seemed to proceed from the sun. But he was greatly perplexed for an explanation of the phenomena, & said astronomers were very tender about admitting any other force than gravitation. I am truly glad, my dear Mr. Faraday, that comet, so beautiful, so wonderful, so full of tidings from infinite space & the hand of God, so full & overflowing with the secret forces which abide in the atoms of all cosmical spheres, should appear in *our* days & gladden *our* eyes & strengthen *our* hearts with the prospect of the increasing physical knowledge which must the sooner, open upon the world.

About the time I received you[r] letter I received one also from M. Alexis Perr[e]y⁴ of Dijon whose investigations on lunar agency in producing earthquakes became known to me some time after I published my views in 1853, on *Solar* causation of the same phenomena⁵.— The letters from both of you were read with great interest by a number of my friends who urged me by all means to have them published as an honor to their authors, as a means of advancing science, & as due to myself inasmuch as my views had been publicly ridiculed by two prominent & leading mathematicians in this country. I hesitated a long time, but an eminent scientific friend & prominent gentleman in Boston urged me so strongly upon the point that I consented to place copies of both the letters at his disposal — that is the philosophical portions of them. Now, my dear Sir, what do you think — This friend took them to

the Boston Courier, a newspaper conducted with some ability, one of whose editors is a Professor at Cambridge, & the organ of the Cambridge party or school or influence whatever it may be called.— They were kept 3 weeks & then my friend had some difficulty to get them back & offered to pay for their insertion. But they would not publish them at any price, even as advertisements. I was surprized at this last, but not surprized that they were unwilling to publish at mere request; for in my controversy with Professor Pierce⁶ last year on “The Sun & Continents”, The Courier espoused his cause as he was the pride of Cambridge. It was however the only journal in Boston, or the United States who did take his part, & I saw the Courier was still in his interest or under his advisement or control.— The editor of the Boston Advertiser however very gladly availed himself of the opportunity to publish them⁷ as he esteemed them too valuable & of too much public & scientific importance to be longer suppressed.— I can only hope, the publication of your note will not meet your disapproval.— The next day after their publication, the *Courier* appeared with a short notice of “a new step in Cometology”, announcing that “Professor Pierce of Cambridge had at length” discovered or “accomplished” the theory of the Curvature of the comets tail “& had not “abstracted” the idea of it “in form nor substance from any Winslow, Warner⁸, Peter’s⁹, or Bond”. I smiled & wondered what the discovery was. Yesterday morning the Courier announced the Discovery itself – & it is no less a novelty than that the tail is the result of *repulsion whose force is about three & one third times as great as that of gravitation!*—

In your note you said you “should be very glad to find some effect of gravity that might be considered complimentary to the variation of that force by the change of distance” – As I view this department of nature it seems to me all magnetic force & its congeners or convertibilities, are secondary powers arising from disturbance of equilibrium in the two primal forces of attraction & repulsion at work among atoms.— It is a question with me now, the solution of which I am earnestly seeking, whether force does not exist & may not, independent of matter – whether it did not originally:– & did not also have existence prior to matter. In my contemplations I can divide forces into strata (so to speak & illustrate thought by physical conditions) – & allowing matter to have affinities for gravitation which after certain accumulation, engenders a capacity for the manifestation of repulsion. I obtain the first class of phenomena – The play between these fundamental forces on atoms brings into existence – or manifestation to our senses – the second class including heat, light, magnetism, electricity.– which also I strongly suspect lie in latent or unappreciated conditions to our senses, outside of matter – the excitation of atoms by the dynamic play of condensation i.e. gravitation & reaction only creating a capacity to receive forces whose fountains are boundless or rather

would be boundless in space if matter was set at liberty from attachment to all force. As nature exists matter, & force, both primal & secondary with all their combinations & complications, are united, their relations only changing to produce all the phenomena of the physical world palpable or conceivable by our profoundest researches.— My idea might be illustrated by conceiving all forces united into a homogeny, like a ray of common light which dissolved by a prism presents a multitude of parts all possessing different functions or powers. Our mental prism may be clear enough one of these days, to dissolve the great secret of nature now hidden in the action & reaction of matter & force. At any rate it is by the *study* of atoms alone that progress can ever be *surely* made. However these atoms may accumulate never fear to follow the fundamental force as an increasing magnitude however mighty the mass may become. Gravitation is a unit in an atom — it is only a magnified unit in a world according to the number of atoms it embraces. So with repulsion: and in the mighty play of these accumulated forces we get heat light & magnetism, in proportion to the *amount* of matter & the *activity* of the primal forces which produce motion among the atoms.— In planets & comets these are more active the nearer the mass is to the sun & they must vary according to its distance from the sun.—

The earth is full of phenomena the opposite of gravitation. Did it ever occur to you that the growth of any tree is an act the contrary of gravitation & that the repulsion between atoms, which carries it upward, may be a mere conversion of the mundane force of gravitation into another living power — a compound of *magnetism & life*?— All these things are marvellous & I beg you to consider them.

I am very truly Yours | C.F. Winslow
Professor M. Faraday | London

1. Letter 3419.
2. This was Donati's comet, the tail of which in September covered 36° of arc. See *Ann. Reg.*, 1858, **100**: 166–8.
3. William Cranch Bond (1789–1859, ANB), Director of the Harvard Observatory, and George Phillips Bond (1825–1865, ANB), American astronomer.
4. Alexis Perrey (1808–1882, P3). Seismologist and professor in the Dijon science faculty. See Perrey to Winslow, 14 May 1858, *Boston Daily Advertiser*, 26 October 1858, [p. 2, col. b].
5. Winslow (1853), 121–74.
6. Benjamin Peirce (1809–1880, ANB). Professor of Astronomy and Mathematics at Harvard, 1842–1880.
7. Part of letter 3419 was published in *Boston Daily Advertiser*, 26 October 1858, [p. 2, col. b].
8. John Warner (d.1873, Reingold and Rothenberg (1972–2002), **8**: 503). American amateur mathematician.
9. Christian Heinrich Friedrich Peters (1813–1890, ANB). German-born Director of the Observatory at Hamilton College, 1858–1890.

Letter 3525**Faraday to Thomas Loveday¹****8 November 1858****From the original in APS Misc MS Collection**[Royal Institution embossed letterhead], Albemarle St.,
London W | 8 Nov 1858

My dear friend,

I am glad to hear that there is a revival of the hopes that the pond near your houses on Old Buckenham Green will at last be filled up. I need scarcely say again that in my opinion it is an improper thing where it is & a very unwholesome neighbour in the hot weather. I have spoken very strongly of the state of the Thames in London² and I believe that good will now come of it. If I could make my voice heard by those who rule these matters at Old Buckenham I would speak as strongly against the pond for I consider it a dangerous neighbour to the families who live near it[.]

Ever Very Truly Yours | M. Faraday

Mr. Thos Loveday

1. Thomas Loveday (d.1908, age 95, GRO). Noted as foundryman in Old Buckenham in 1861 census, TNA RG9/1235, f. 63, p. 20. Member of the Sandemanian Church there since 1841, DUA Acc M/409/5/3, p. 130.

2. Faraday to the Editor of *The Times*, 7 July 1855, letter 3003, volume 4.

Letter 3526**James Thomson to Faraday****9 November 1858****From the original in IET MS SC 2**

2 Donegall Square West, Belfast | 9 Nov 1858

My dear Sir,

I thank you very much for having so kindly written to me in August last on the subject of the freezing and melting point of water & ice¹. I intended to write to you at the time, thanking you for your letter; but, through illness in my family, and repeated absences from home on business, I have hitherto found myself detained from reverting to the subject.

I recollect that in the old Athenaeum report of your Paper² – to which however I have not had access of late – mention is made of your supposition that there is a tendency for a film of water between two surfaces of ice to become ice, other particles touching ice only on one side becoming water at the same time, and supplying the cold necessary to freeze the water between

the two surfaces of ice. Having been long aware of that supposition, and now having farther had your letter of August 4 to consider; I still incline to think that the freezing of the film between the two masses of ice is due simply to the melting by *pressure* of portions of the ice pressed against one another at places of contact.

Soon after the time when I received your letter, I met with a paper by Professor Forbes of Edinburgh (from the proceedings of the Royal Society of Edinburgh dated the 19th of April 1858) "On Properties of Ice near its melting Point"³. This I presume to be the paper in which his shilling experiment is mentioned to which you alluded in your letter to me. In that paper Forbes states an experiment as proving that two masses of ice placed extremely close to-gether, and having a film of water intervening, but free from pressure against one another, will unite even in a moderately warm atmosphere. I am not, however, prepared to admit the validity of his proof in this matter. He *had* slight springs pressing the masses of ice together: but beyond this I conceive that the capillary attraction of the intervening water would draw the masses against one another with a force quite notable:— and also I conceive that the film of water between the two masses of ice would be sustained almost entirely by capillary attraction at its upper surface, and would therefore exist (like mercury in a barometer) under a pressure *less* than that of the atmosphere. This diminished fluid pressure would *raise* the freezing point of the film of water, and would produce a tendency to its freezing, even by contact with such parts of the adjacent ice as exist under atmospheric pressure, and much more by contact with the parts of the two masses of ice in contact with one another and pressing against one another by the forces of the spring used to maintain contact and of the capillary attraction of the fluid pulling the masses against one another.

Thus I still incline to think that mere proximity is not enough to cause the two pieces of ice to unite.

In your letter to me you make reference, as bearing on this subject, to what appears to be a very general law, namely that, in bodies of the same kind, the particles tend to retain the state of those which surround them:— as, for instance, that water may be cooled much below 32°F. without freezing; but that a splinter of ice touching it will instantly make it solidify: and that water may be raised under atmospheric pressure to a temperature far above 212°F. without boiling but that a bubble of air introduced will cause it to explode. It seems to me, however, that these phenomena are essentially distinct from what can occur with a film of water touching ice on one side, or on two opposite sides: because, in the phenomena you adduce the tendency is for a particle to *retain* a state in which it already exists, that state being the same as the state of the surrounding ones: but I do not think they show a tendency

for a particle differing in its state from the surrounding particles to *assume* their state. It is certain that, in numerous cases, every particle shows a great resistance to change from a state in which it and all adjacent particles exist. In the case of a film of water between two masses of ice, however, the difficulty of *making a beginning* either of melting or freezing does not exist, as both water & ice are present to-gether.

I do not see in the principle I have proposed any insufficiency to explain the regelation of fractured ice, & the plasticity of ice; and, according to the views I have just now submitted, I do not see that we ought to suppose the occurrence of another efficient cause of regelation such as you have suggested.

With great deference I beg to offer the above remarks to you.

I am, With thanks | Yours most truly | James Thomson

P.S. It may be well to mention that I conceive the powerful tendency to adhesion manifested between flannel & ice in a warm atmosphere, is to be attributed partly to the fibres of the flannel being drawn against the ice by capillary attraction of the liquid films, & thus being made to apply a pressure to the ice which must slightly lower its melting point; & again partly to the *diminished fluid pressure* of the liquid films produced by capillary attraction, which must *raise* the freezing point of those films. | J.T.

1. Letter 3493.

2. *Athenaeum*, 15 June 1850, pp. 640–1 which contains an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'.

3. Forbes (1858).

Letter 3527

Joseph Dalton Hooker to Faraday

10 November 1858¹

From the original in IET MS SC 2

Kew Gdns | 10 Nov 1858

Dear Mr. Faraday,

Many thanks for your kind contribution to the Ralfs² annuity. I am getting on very well & have myself raised already upwards of £100 – This morning a bountiful individual sent me £30! toward it³, & many have offered to double their subscriptions if required. If there is a lot of money in this county there is surely a lot of charity too thank God. The only refusal I have met with is from a man who is both well to do, & had just received an unexpected legacy of nearly £3000! What a world it is to present such [word illegible].

With united regard | [word illegible] Sincerely & obliged |
Jos D. Hooker

My wife⁴ desires sincere regards to Mrs. F. Miss Barnard & yourself

1. Dated on the basis of the reference to the Annuity which Hooker was organising. See Huxley, L. (1918), 1: 414.
2. John Ralfs (1807–1890, ODNB). Botanist who lived in Penzance.
3. This was the Sussex botanist William Borrer (1781–1862, ODNB). See Hooker to Darwin, 12 November 1858, Burkhardt *et al.* (1985–2005), 7: 188.
4. Frances Harriet Hooker, née Henslow (1825–1874, ODNB under J.D. Hooker). Married J.D. Hooker in 1851.

Letter 3528

Faraday to Christian Friedrich Schoenbein

13 November 1858

From the original in UB MS NS 440

Royal Institution | 13 Nov 1858

My dear Schoenbein,

Daily & hourly am I thinking about you and yours, and yet with as unsatisfactory a result as it is possible for me to have. I think about Ozone – about Antozone – about the experiments you shewed Dr. Bence Jones – about your peroxide of barium – your antozonized oil of turpentine – and it all ends in a giddiness – and confusion of the points that ought to be remembered. I want to tell our audience what your last results are upon this most beautiful investigation¹; and yet am terrified at the thoughts of trying to do so, from the difficulty of remembering from the reading of one letter to that of another what the facts in the former were. I have never before felt so seriously the evil of loss of memory and of clearness in the head:– and though I expect to fail some day at the lecture table, as I get older, I should not like to fail in ozone or in any thing about you.– I have been making some of the experiments Dr. B. Jones told me of, & succeed in some but do not succeed in all. Neither do I know the *shape* in which you make them as (I understand) good class experiments and telling proofs of an argument.– Yet without experiments I am nothing. If I were at your elbow for an hour or two, I would get all that instruction (as to precaution) out of you which might bring my courage up. I remember in old times (at the beginning of Ozone) you charged me with principles & experiments.– I wonder whether you could help me again?– Most likely not:– & it is a shame that I should require it:– but without such help and precautions on my part, I am physically unable to hold my place at the table;– and without I justify my appearance on a Friday Evening I had better withdraw from the duty.

What I should want, would be from *ten* to *fifteen* or at most twenty table experiments,– with such instructions as to vessels, quantities, states of solution materials & precautions as would make the experiments visible to all: & certain & ready.– Also the points of the general subject in what you have found to be the *best order* for the argument & its proof.

I have sought for the old bottle of *Antozone oil of turpentine* but believe I have used it all up. I fear it is of no use trying to make it by the end of January next year:– yet about that time I must give the evening if I give it at all. If you encourage me to give the argument (& I can only try if you help me)– have you any of the substance you could spare? and could you find conveyance for it by rail or otherwise? I fear there is no other substance that will represent it:– i.e. that approaches so near to isolated Antozone as that body does.

Now do not scold me. I am obliged to speak as I do. Perhaps you had better tell me that I must *give up the subject* for that I can hardly succeed in telling it properly by the way I propose. Do not hesitate to say so;– for I am well prepared by my inner experience in other matters, to suppose that may be the case. But then tell me so at once, that I may think *over* my position here for January[.]

Now for a more cheery subject. I saw Miss Schoenbein a few days ago (after a long interval) and was glad to see her looking well & happy. I am sure you will not think the worse of us for the effect England has had upon her, when you see her again. She will make you, her Mother, – Sisters & all happy. But I know she tells you all about herself & as to her state of contentment or happiness that will breathe in her letters. I have more to say but cannot bring it to mind. Believe me to be as Ever My dear Schoenbein

Your true & obliged friend | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | Switzerland

1. Faraday (1859a), Friday Evening Discourse of 25 February 1859.

Letter 3529

Faraday to James Thomson

15 November 1858

From the original in QUB MS 13/M/7

[Royal Institution embossed letterhead], Albemarle St. W |
15 Nov 1858

My dear Sir,

I am much obliged by your note¹ – I happen to be occupied in collecting my papers (not electrical) into a volume²; & on reprinting the notice on ice from the *Athenaeum*³ have added a further development of my views⁴:– it will come out in the course of the winter I dare say; but that is as the printer likes[.]

You represent that my view gives no account of the *beginning* of rege-lation. That is not so great a difficulty to my mind as to yours; because, since the year 1833, if not before that time, I have been obliged to admit

that particles cannot be so exclusively engaged, *even by a combining chemical action*, as to be indifferent to, or without relation, to those along side of them—

I will mention a difficulty as regards this *beginning* of regelation which occurs to me under your view; *If the particles be supposed to be without this external relation.*— You admit with me that bodies tend to retain that state which they for the time possess; and, that against a change of temperature of *many degrees of heat*:— then how can the small change of temperature not amounting to the $\frac{1}{100}$ th of a degree, due to difference of pressure in many of the regelation experiments, cause that change from solid to fluid or fluid to solid, which many degrees of temperature change, applied in the common way, will not effect?

It seems that in ice the melting temperature is irregular; i.e. that certain portions of the ice tend to melt before other portions. I have given my view of this irregularity in a note, which Tyndal[1] has added to his last paper in the Phil Trans⁵ — If ice of *equal purity* should either from crystalline arrangement or some other cause, prove to be a mixture of particles having differences in their fusibility, then it would be very easy to build up a fourth theory of regelation.— But time — reconsideration — new thoughts & new experiments will no doubt clear up all these matters.

Ever My dear Sir | Very Truly Yours | M. Faraday
Professor Jas. Thomson | &c &c &c

1. Letter 3526.

2. Faraday (1859b).

3. *Athenaeum*, 15 June 1850, pp. 640–1 which contains an account of Faraday's Friday Evening Discourse of 7 June 1850, 'Certain Conditions of Freezing Water'. This was republished in Faraday (1859b), 372–4.

4. 'On Regelation' in Faraday (1859b), 377–82.

5. Tyndall (1858), 228–9.

Letter 3530

James Thomson to Faraday

24 November 1858

From the original in IET MS SC 2

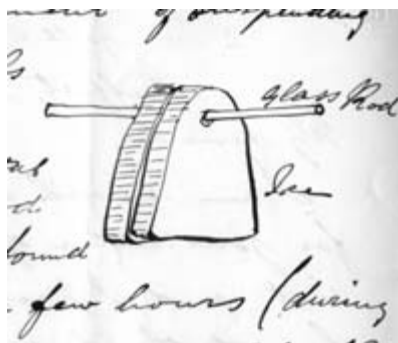
2 Donegall Square, West, Belfast | 24 Nov 1858

My dear Sir,

I am much obliged for your letter of the 15th inst.¹ and I presume I have to thank you and Professor Tyndall jointly for a copy of his paper² containing the note from you appended³, which he has kindly sent to me and of which the contents interest me much.

There was hard frost here last night:— and having happened to meet with slabs of ice about $1\frac{1}{2}$ ins thick, I have repeated Forbe[s]'s experiment of

suspending two slabs of ice on a horizontal glass rod thus



I found that in a few hours (during which water was constantly dropping from them) the two slabs were stuck fast together: and I did not find it necessary to apply springs to produce any pressure at all. I merely pressed them gently with my hands for a few seconds a first to make them keep close to-gether by an incipient cohesion or a cohesion of some minute spots. The cohesion then went on increasing till in a few hours it became very strong. Although there is no spring *necessary*, yet the capillary attraction due to the films of water between the two plates alluded to in my last letter to you⁴ must keep up a steady force drawing the plates together, and causing pressure at some parts. I could see many square inches of films of water between the two plates or slabs of ice. This water being situated as at a, a, a, a, a,



in the figure must exist, by virtue of capillary attraction, under less than atmospheric pressure *at all parts of itself*. This diminished pressure must tend to cause the water to freeze even at a temperature slightly above the ordinary freezing point. The diminished pressure in the liquid film, extending over several square inches, will cause the external atmospheric pressure to force the two slabs against one another with quite a notable force.

It seems to me that you have somewhat misconceived my meaning in respect to a "difficulty of making a beginning" as you say in your last letter:-

"I will mention a difficulty as regards the *beginning* of regelation which occurs to me under your view *if the particles be supposed to be without this external relation*. You admit with me that bodies tend to retain that state which they for the time possess; and, that against a change of temperature of *many degrees of heat*;— then how can the small change of temperature not amounting to the $\frac{1}{100}$ th of a degree, due to difference of pressure in many of the regelation experiments, cause that change from solid to fluid, or fluid to solid, which many degrees of temperature change applied in the common way will not effect?"

With reference to this I would say, that, I think the resistance to change of state, from liquid to solid or solid to liquid; or liquid to gaseous, or gaseous to liquid, under consideration occurs only when the substance is not present in the two states already (as for instance when water is cooled below 32°F. without freezing, there is no ice present) but when both water and ice are in contact, as is the case in the regelation experiments it is a part of my theory to suppose that there is no tendency for water, to remain water, or ice to remain ice, against any change of temperature however slight tending to change the state of the water or the ice.

I shall look with much interest to the ideas given in your note annexed to Professor Tyndall's paper, and to the suggestion you give in your last letter to me of a "fourth theory of regelation" as being perhaps possible to be built up.

I am My dear Sir | Very truly yours | James Thomson
Professor Faraday

1. Letter 3529.
2. Tyndall (1858).
3. *Ibid.*, 228–9.
4. Letter 3526.

Letter 3531

Faraday to Christian Friedrich Schoenbein

25 November 1858

From the original in UB MS NS 441

Royal Institution | 25 Nov 1858

Warmest thanks my dear friend for your last kind letter¹:- it has given me courage – and yet when I look into the journals about ozone & see how many things there are which have been said by different men & how thoroughly I have forgotten most of them – it makes me very doubtful of myself for I cannot hold many points in hand at once as I used to do – but I shall trust in your strength & kindness. I have repeated as I said some of your results. The peroxide of barium which I have seems to do pretty well, but it is vesicular & gray & so unlike what Brodie made with great care² & called the right peroxide that I doubt it but I shall know better when I receive your instructions. I have forgotten the preparation of $\text{HO}\oplus$ by the fluor salicic acid – where is it described in French or where is it? – what strength do you prepare $\text{HO}\oplus$ strong or dilute? – The peroxide of Manganese do you employ the natural or if the artificial what is your process of preparation for solution in AA & do you use it wet or dry?

I have had the paper on reciprocal catalysis (23 June 1858) translated³, so have with the letters &c obtained possession of part of your thoughts – but it is the experimental proofs & the method of making them perfectly about which I am anxious & none but the discovering philosopher himself knows how best to make their value evident – for that reason I desire to work with your tools & in your way and if the chemical you refer to are to be *bought* in Bale in what you know to be the right state send them to me but if not do not waste your time personally. I shall prepare them from your instructions[.]

I had your letters⁴ &c by Dr. Bernoulli⁵ on the 17th instant. I did not see him for he sent them by post & was to leave London the next day. He had been ill & detained in Berlin but I could not tell when you had written for your letter had *no* date & strange to say neither had his except the Postmark[.] Yours by him & mine to you⁶ must have passed on the road[.]

Kindest remembrances to the household from one always under obligation to you & ever yours | M. Faraday

1. Not found, but evidently a reply to letter 3528.

2. Brodie (1850, 1855).

3. Schoenbein (1858d).

4. Letter 3523.

5. Carl Gustav Bernoulli (1834–1878, Meyer-Holdampf (1997)). Swiss physician and explorer.

6. Letter 3528.

Letter 3532**Faraday to Christian Friedrich Schoenbein****2 December 1858****From the original in UB MS NS 442**

2 Dec 1858

My dear Schoenbein,

I have received the packet safe from M. Rumpf¹ & write instantly to acknowledge it with all thanks – but I have not any thing for Miss Schoenbein_[.] I think M. Rumpf means to deliver what you have sent himself. Your daughter was very well & happy last Friday evening² when I had the honor to be in her company at Stamford Hill_[.]

Ever Yours | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | Switzerland

1. Unidentified.

2. That is 26 November 1858.

Letter 3533**Faraday to an unidentified correspondent****10 December 1858****From the original in RI MS F1 E12**

[Royal Institution embossed letterhead] | 10 Dec 1858

All right – the regular way is the only way – for the Committee will have their voice in the matter, & it is a good thing that they are all ready to aid & judge – but it makes the result very uncertain until they have given their voice. However it is not my business to say this or to anticipate any of their determination – as indeed I have written you on former occasions_[.]

Ever Yours | in haste | M. Faraday

Letter 3534**Faraday to Juliet Pollock****16 December 1858****From the original in RI MS F1 E13**

[Royal Institution embossed letterhead], Albemarle St. W |

16 Dec 1858

My dear Mrs. Pollock,

I send you a top in return for your instruction in nine mens holes. Put the peg into the top conical end downwards about $\frac{1}{2}$ an inch through & take off the little weight of tin from beneath. The top will spin pretty well though

not quite true:— never mind that. Now give the handle a push so as to incline it a little towards A and spin it_[.] You will have red to the left & green towards the right – or push the handle towards B & spin it – then you will have red to the right & green to the left.

Or again let the pin be upright so as to give little or no colour then stick the tin weight on beneath A spin it – you will have green to the left – red to the right – remove the tin weight to beneath B & you immediately have red to the left & green to the right.

Now for the reason_[.] When the top of the stem leans towards A then the stem describes a circle as the top revolves & as the stem will lean towards A in all positions of the top so it is only when A & B are in a line with the observer that the stem will eclipse truly the diametral line. When the top is in that position in which all the green is from the observer the portion of green eclipsed will be a column leaning to the left, & when the red is all from the observer the part eclipsed will be a portion of red leaning towards the right – the two do not overlap but the green shadings are all on one side & the red on the other. So you never see the green on the left on the coloured green part beyond the top of the stem *but you see the red there:*— and you never see the red on the right but you see the green there & that is what you do see. If you push the stem over towards B the change of colours of course takes place. The same effect happens when any part of the green or red is shaded from the eye by the rising stem but it is most observable when the line A B is to the right & left becomes then the stem inclination is greatest in that position.

When the *weight is put on* the whole centre of gravity is removed to one side and the red & green shadows are more separated & stand parallel to each other but the cause is the same_[.] As both coloured surface & axis move together whenever the colour has returned to a particular place the stem has returned to the same place & therefore hides the same place from the eye.

Ever Most Truly Yours | M. Faraday

Letter 3535

Faraday to Juliet Pollock

18 December 1858

From the original in RI MS F1 E14

[Royal Institution embossed letterhead], Albemarle St. W |
18 Dec 1858

My dear Mrs. Pollock,

I have received your gift – mean to play with it often – & always think of you. I have not had time yet to try its powers but it will mingle in very well with the Juvenile Lectures. I am only sorry I cannot find something about it that would enable me to quote it in illustration of Metalline qualities¹.

Ever Your | Very Grateful Servant | M. Faraday

1. A reference to Faraday's 1858/9 Christmas lectures on 'Metals' which he began later in the month. His notes are in RI MS F4 G61.

Letter 3536

Faraday to George Gabriel Stokes

21 December 1858

From the original in ULC Add MS 7656, F23

[Royal Institution embossed letterhead], Albemarle St. W |
21 Dec 1858

My dear Stokes,

The accompanying is a MS copy of Dr. Giesecke¹ MS meteorological observations made in Greenland. They have never been printed but are used as I understand by Dr. Giesecke in his papers printed on the continent. Sir William [sic] Trevelyan (of the Athenaeum Club) asked me where the observations could be fitly placed & I said I thought in the Royal Society's library.— I therefore offer them in his name for the Society's acceptance²[.]

Ever Truly Yours | M. Faraday

1. Charles Lewis Giesecke (1761–1833, ODNB). German-born Professor of Mineralogy at the Royal Dublin Society, 1814–1833.

2. These observations, made between 1806 and 1813, are in RS MS MA 154 with the following inscription in Faraday's hand on the title page: 'Transcribed from the originals in Copenhagen, for W.C. Trevelyan'.

Letter 3537

Faraday to Walter Calverley Trevelyan

21 December 1858

From the original in NUL Trevelyan Papers, WCT 127

[Royal Institution embossed letterhead], Albemarle St. W |
21 Dec 1858

Dear Sir Walter,

I am much obliged by your kindness in letting me see the drawings of the Aurora borealis but I never see any account of the phenomena without feeling how utterly unable one is to draw any conclusion from, or reason about, the appearances without seeing them for oneself[.]

I have sent the Quarto volume of Meteorological observations to the Royal Society in your name¹[.]

I am | Very faithfully Yours | M. Faraday

1. See letter 3536 and White (1898), 120.

Letter 3538**Faraday to Christian Holst¹****27 December 1858****From the original in University of Oslo Library**

Royal Institution, London | 27 Dec 1858

Sir,

I hasten to acknowledge the honor done me by the Royal University of Norway in the gift of a medal of that most eminent Norwegian Philosopher Dr. Christopher Hansteen. To be included by any means in the honor done to science and to such a representative of it as he is is indeed very grateful to me who have ever found more pleasure & delight in the study of natural phenomena than in any other direction taken by the human intellect – If it were not intruding too much I would ask you to congratulate our Veteran on my part yet surely it is not unfit that I as an humble element of the University should offer my respectful sympathy to one of its greatest ornaments & supports[.]

I have the honor to be | Sir | With every respect | Your Very Grateful
 Servant | M. Faraday
 M. Chr. Holst | &c &c &c &c

Address: M. Chr. Holst | &c &c &c &c | Royal University | Christiania |
 Norway

1. Christian Holst (1809–1890, AsKL). Secretary of University of Christiania, 1841–1878.

Letter 3539**Faraday to Millicent Bence Jones****31 December 1858****From the original in RI MS F1 E15**

[Royal Institution embossed letterhead] | 31 Dec 1858

My dear Lady Millicent,

I am ashamed that the week has almost passed away before my acknowledgement of the kindness which made you join us in your invitation to Jeannie. I knew that she had to decline the pleasure, and that in a hurry, whilst your son¹ waited (i.e. as regards herself); & then I forgot my part. I hope the next time we shall come & shew how glad we are to have a few friends; a matter which we look upon as very different to joining in society. Excuse your inattentive but very

faithful Servant | M. Faraday

The happiest wishes of the season from all of us to all of you | MF

1. Either Henry Robert Bence Jones (1844–1902, Bence Jones (1929), 21) or Ralph Noel Bence Jones (1845–1866, Bence Jones (1929), 21).

Letter 3540

Faraday to an unidentified relation¹

1859

From Bence Jones (1870a), 2: 431–3

The Revivals², &c., cannot trouble the Christian who is taught of God (by His Word and the Holy Spirit) to trust in the promise of salvation through the work of Jesus Christ. He finds his guide in the Word of God, and commits the keeping of his soul into the hands of God. He looks for no assurance beyond what the Word can give him, and if his mind is troubled by the cares and fears which may assail him, he can go nowhere but in prayer to the throne of grace and to Scripture. No outward manifestation, as of a revival, &c., can give either *instruction* or *assurance* to him, nor can any outward opposition or trouble *diminish his confidence* in “Christ crucified, to the Jews a stumbling-block, and to the Greeks foolishness; but to them who are *called*, Christ the power of God, and the wisdom of God.”³ If his attention is called to the *revivals*, it cannot be that he should feel instruction there or assurance there, other than what he finds in the Scriptures, without reference to them; and it seems to me that any power they may have over his mind other than the Scripture has, must be delusion and a snare.

That man in his natural state is greatly influenced by his fellow-creatures and the forms of emotion which are amongst them, is doubtless true, even when it concerns what he considers his eternal welfare. How else would the wonderfully varied and superstitious forms of belief have obtained in the world? What carries the Mormons into the desert, surrounded by trouble and the enmity of those around them? What sustains a spiritual dominion like the Papacy, aided by the nations around it, to proclaim the name of Christ whilst it contradicts His Word – refuses it (the record of the Spirit) to the people – and crushes out with all intolerance the simple obedience of the truth? Man’s natural mind is a very unstable thing, and most credulous, and the imagination often rules it when reason is thought to be there. Mesmerism has great power over it; so has poetry; so has music; so has the united voice of the multitude; so have many other things: but these things are, so to say indifferent as respects the *character* of the object they may be used to sustain, and are just as powerful in favour of a bad cause as a good one. Among the contradictory and gross systems of religion, or the numerous and opposed systems of political government, any one of them may be sustained by the use of agencies as these.

The Christian religion is a revelation. The natural man cannot know it. He, not knowing it, is liable in respect of religion to all the influences before mentioned, finds in them snares and delusions, and either becomes an infidel or is subject to every wind of doctrine. The Christian religion is a revelation, and that revelation is in the Word of God. According to the promise of God, that Word is sent into all he world. Every call and every promise is made freely to every man to whom that Word cometh. No revival and no temporal teaching comes between it and him. He who is taught of the Holy Spirit needs no crowd and no revival to teach him; if he stand alone he is fully taught, for the Comforter (the Spirit) taketh the things of Christ and showeth them to His people. And if in the mercy of God it *should* please Him that one seeing the commotion about him should be led to examine his ways, it will only be in the Word of testimony, the Word of God, that he will find the revelation of the new and living way by which he may rejoice in hope of entering the Kingdom of Christ.

1. As stated in Bence Jones (1870a), 2: 423.

2. *Ann. Reg.*, 1857, 99: 102–5.

3. 1 Corinthians 1: 23–4.

Letter 3541

Faraday to William Smith¹

3 January 1859

From a copy in RI MS F1 E15a

Royal Institution of Great Britain | (stamped) 3 Jan 1859

Dear Sir,

Many thanks to both you and Mr. Bentley². Mr. Murray made me an unlimited offer like that of Mr. Bentley's³ many years ago, but for the reasons I am about to give you I had to refuse his kindness. He proposed to take them by short hand & so save me trouble – but I knew that would be a thorough failure. Even if I cared to give time to the revision of the M.S. still the Lectures without the experiments & the vivacity of speaking would fall far behind those in the lecture room as to effect:– and then I do not desire to give time to them for money is no temptation to me. In fact I have always loved science more than money & because my occupation is almost entirely personal I cannot afford to get rich.

Again thanking you & Mr. Bentley | I remain | Very Truly Yours |
M. Faraday
Wm. Smith Esqr | &c &c &c

I am much obliged for your promise of a ticket for the Portrait Gallery⁴ | MF.

Endorsement: Copy of a letter from Mr. Faraday to *Wm. Smith* in answer to an application made by the latter on behalf of *Mr. Bentley* to be allowed to publish the lectures delivered to a juvenile audience.

1. Unidentified.
2. Richard Bentley (1794–1871, ODNB). Publisher.
3. See Bentley to Smith, 30 December 1858, UCLA UL Bentley papers, in which Bentley asked Smith to sound out Faraday to see if he would be willing to publish his Christmas lectures.
4. The National Portrait Gallery opened at 29 Great George Street on 15 January 1859. *Athenaeum*, 8 January 1859, p. 51.

Letter 3542**Faraday to John Joseph Griffin¹****4 January 1859²****From the original in RSC MS**

[Royal Institution embossed letterhead] | 4 Jan 1858 [sic]

My dear Sir,

Accept my sincere thanks for your kindness in sending me a copy of Your Radical Theory³. Chemistry has become such a comprehensive & extensive pursuit & my memory has become so imperfect in its action that I dare not profess to retain in mind the data necessary for the decision of the great historical question contained in your work⁴ but I trust that the progress of time & connection will soon remove that which is doubtful & set forth in just prominence the right claimants for disputed honors[.]

I am | Your Very faithful Servant | M. Faraday
J.J. Griffin Esqr | &c &c &c

1. John Joseph Griffin (1802–1877, ODNB). Chemical writer and supplier of apparatus.
2. Dated on the basis that Griffin (1858) was published at the end of 1858. See the review in *Athenaeum*, 25 December 1858, p. 839.
3. Griffin (1858).
4. This question was who had developed the radical theory of chemistry.

Letter 3543**Faraday to Walter Calverley Trevelyan****5 January 1859****From the original in NUL Trevelyan Papers, WCT 127**

[Royal Institution embossed letterhead] | 5 Jan 1859

My dear Sir Walter,

I cannot suggest observations for another person nor can I reason from another persons observations – I am always obliged to *observe* & also *think* for myself. The best thing I can do is to refer Mr. Tayler¹ to the Revd. Mr. Farquharson² observations upon & measurements of the aurora borealis which he will find in the Phil Transactions for 1839. p. 267³.

Very Truly Yours | M. Faraday
Sir W.C. Trevelyan | &c &c &c

1. Unidentified.

2. James Farquharson (1781–1843, ODNB). Scientific writer.
3. Farquharson (1839).

Letter 3544

Faraday to George Matthey¹

5 January 1859²

From the original in SI D MS 554 A

[Royal Institution embossed letterhead], Albemarle St. W |
5 Jan 1858 [sic]

My dear Sir,

Tomorrow is *Silver* – and if <y>our intention & *convenience* about <a> cake of silver extends to tomorrow it would be important to me to know <i>t as I would arrange accordingly [word illegible] cakes of silver have on them <t>hose vermiform tubes due to the escape of oxygen on cooling, if they happen to be present why so much the better[.] The similar ingots of *Platinum* & *Aluminium* to illustrate Specific Gravity would be very acceptable if they are still in existence. The bearer is very faithful & would bring me my message or any thing else you might like to entrust to <him.>

Ever Your Obliged | M. Faraday
Geo Mat[t]hey Esqr | &c &c &c

1. George Matthey (1825–1913, ODNB). Metallurgist and partner in Johnson and Matthey.
2. Dated on the basis of the reference to Faraday's Christmas lectures on 'Metals'. His notes are in RI MS F4 G61.

Letter 3545

Alessandro Palagi to Faraday

7 January 1859

From the original in IET MS SC 2

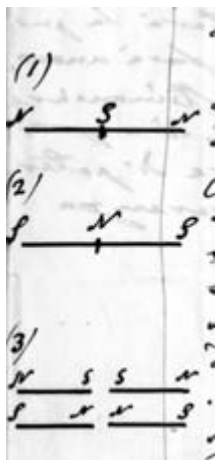
Bologna | 7 Gennaio 1859

Signore,

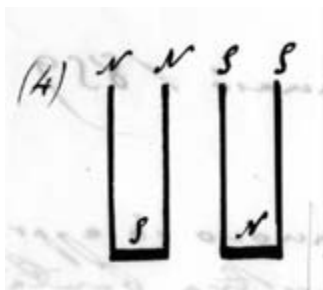
Permettatemi, o Signore, che di nuovo io approfitti della vostra sapienza e della vostra bontà. Nel lungo soggiorno ch'io ho fatto alla campagna nella scorsa estate ho potuto occuparmi di fisica sperimentale e mi è riuscito di vedere di assai bei fatti, per me nuovi, e che sembranmi di non lieve interesse per la Scienza. Fra questi fatti, da me ottenuti, uno ve n'ha, che mi tiene in forza della sua novità, ed è intorno a questa ch'io vi prego, o Signore, del vostro consiglio; abbiate la compiacenza, ve ne prego, d'illuminarmi.

Mi è riuscito di magnetizzare permanentemente degli aghi, dei cilindri di fiero per modo che abbiano alle due estremità due poli simili e al centro

un polo dissimile; posso a mio piacimento variare il genere dei poli, sia alle estremità, che al centro.



E in mio arbitrio p. e. di magnetizzare un ago ed un cilindro di fiero, che abbia il polo Nord alle due estremità ed il polo Sud al centro (1); oppure, se mi piace, posso magnetizzare o quello stesso ago od un altro inversamente, che abbia, cioè, il polo Sud alle due estremità ed il polo Nord al centro (2). Se gli aghi così manetizzati si dividono a mezzo non avviene la inversione dei poli agli estremi divisi, come nelle calamite comuni; ma si divide il polo dissimile, e ottengonsi due magneti polarizzati alla maniera ordinaria e al tutto simili (3). Invece di aghi o di cilindri rettilinei, posso magnetizzarne, che siano in forma di ferro da cavallo, e in tal caso le estremità del cilindro, così ripiegato, potranno avere ambedue o il polo Sud ed il polo Nord e alla base o ripiegatura il polo relativamente dissimile (4).



Questa maniera di magnetizzare non mi sembra un caso particolare dei punti conseguenti; direi, invece, che quelli fossero un caso particolare di questo.

Non so che alcun Fisico faccia menzione di questo stato magnetico artificiale, inclusi il Becquerel¹ e il Du Moncel² nei loro pregevoli lavori del 1858³.

E per tale mia ignoranza, circa alla novità di questo fatto, che mi rivolgo a Voi, o Signore, per di piacervi d'illuminarmi assicurandovi dell'immenza mia gratitudine.

Circa poi all'importanza del fatto, se nuovo, non cadrà alcun dubbio, e meriterà la pubblica luce; se per contrario non sarà nuovo, avrà servito ad illuminarmi, a testimoniare della somma vostra gentilezza, ed a procurarmi al presente l'alto amore di potermi ripetere con ossequio e riverenza

Di Voi, Ch-mo Signore | Umil-mo Dev-mo Servo | Alessadro Palagi
Monsieur | Michel Faraday | etc. etc. etc. | London

TRANSLATION

Bologna | 7 Jan 1859

Sir,

Allow me, Sir, once again, to profit from your knowledge and kindness. During my long sojourn in the country last summer I was able to devote myself to experimental physics and I succeeded in seeing some rather beautiful facts, new to me and which seem to me to be of significant interest to science. Amongst the facts obtained by me, there is one which stands out because of its novelty and it is on this matter that I would like to ask you, Sir, your advice; have the kindness, I beg you, to enlighten me.

I managed to magnetise permanently some needles, some iron cylinders, in such a way that they had the North pole at their ends and the South pole in the middle (1); likewise, at will, I could magnetise this same needle or another inversely, that is, so that the South pole was at the ends and the North pole in the middle (2). If the needles magnetised in this way are divided in the middle, the inversion of poles at the divided ends does not occur, as in common loadstones, but the dissimilar pole divides and two magnets polarised in the normal way and similar to them in every respect are obtained (3). Instead of needles, or rectangular cylinders, I can magnetise horse-shoe shapes, and in this case the ends of the cylinder, bent in this way, can both have the South pole and the North pole and at the base or bend the opposite pole (4).

This method of magnetising does not seem an exception to the corollaries; I would rather say that the latter were an exception to this.

I am not aware of any physicist mentioning this state of artificial magnetism, including Becquerel¹ and Du Moncel² in their prestigious works of 1858³.

My ignorance is so great concerning the novelty of this fact, that I turn to you, Sir, to ask you to enlighten me, assuring you of my immense gratitude.

Concerning the importance of this fact, if it is new, there can be no doubt, and it will merit being made public; if on the contrary, it is not new, it will have served to enlighten me, to testify to your kindness and to procure for me the great pleasure of being able to repeat with homage and reverence, that I am

Your, Dearest Sir | Most Humble and Most Devoted Servant |
Alessandro Palagi
Mr. | Michael Faraday | &c. &c. &c. | London

1. Antoine-César Becquerel (1788–1878, DSB). French chemist who supported the contact theory of the Voltaic cell.

2. Théodore Achille Louis Du Moncel (1821–1884, DSB). French researcher on electricity and magnetism.

3. Becquerel and Becquerel (1858); Du Moncel (1858).

Letter 3546

Julius Plücker to Faraday

9 January 1859

From the original in IET MS SC 2

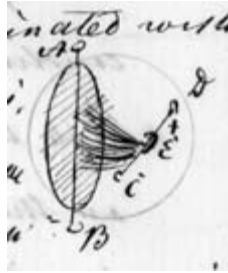
Bonn | 9 Jan 1859

My dear Sir,

I thank you, Sir, for your last kind letter of the 27th of July¹, and would not answer it without communicating to you some new results regarding the electric discharge through gaz vacua. I had observed already the double current – mentioned in your letter – which is produced in Gassiot's tube, if only one tin-cover is touched by one wire of Ruhmkorff's coil. I had made use of it in order to confirm the theoretical views, contained in my paper of the 15th of July². I closer examined double currents in a recent paper³ in order to get analogies with the negative light, constituting the magnetic curves and surfaces. I think it probable that this light, starting from its electrode returns to it in the same way. – If the current find resistance of any description in a longer evacuated tube it partly returns on its own way (will it not be the same in *very* long isolated copper wires?).

But by far the principal object of my last longer paper⁴ (sent before Christmas, to Poggendorff) are the curious appearances of *positive* light which are as characteristic as those of negative light, already described. Let me try to give in a few words a general idea of them. From each of two electrodes entering an evacuated sphere of glass starts a peculiar kind of light. Even if the distance of the electrodes be a few millimetres only, there is produced no current; the double light, filling all the sphere is separated by the magnet. Suppose, for instance, an airvacuated sphere, through which are conducted two

platina wires, equally distant from the centre and directed perpendicularly to each other. Put the sphere upon the two ironpieces of the Electromagnet, let the negative wire AB be vertical, the positive one axial; let the south polarity (the polarity of the North of the Earth) be behind.

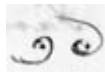


Then all the surface of the axial and vertical cercle passing through AB will be illuminated with beautifull violet light, while the reddish yellow positive light is concentrated in E the midst of CD and from E it moves in a fine spiral toward AB. Between the com̄encement of the spiral and the positive wire CD, there is a small dark interval.



The spiral does *not reach* AB.

After having changed the polarity of the magnet, the appearance of the illuminated circle will not change, but instead of the single spiral, you will get two spirals, starting from the two ends C and D, both turned in the same way, but in opposite way as before



After a com̄utation of Ruhmkorff's coil the phenomena totally change.

In my paper I described a great number of similar phenomena, especially with regard to the propagation of *positive* light. In admitting an electric particle – *sit venia verbi*⁵ – starting from the different points of the positive wire towards the negative one and applying to its movement the well known law of the action of the Magnet on a moved electric particle – I became enabled to predict all phenomena observed, regarding the concentration of the positive light

in certain points of its wire, as well as the curves, which it follows towards the negative wire. With other words, the same laws, which determine the action on a formed current are applicable to its formation.

In our case the positive light goes all the way towards the negative wire; here the combination of both electricities takes place, accompanied with chemical action and production of heat. The magnetic light, which has nothing to do with the current itself, may probably take its origine from these same sources.

If the air be less rarefied *all* changes. Accidentally my above mentioned sphere got a fissure through which air entered *most* slowly. The electric light opposed by an by a greater resistance to the magnetic action. The magnetic light of the circle AB disappeared, the positive light of the spirals became better defined and, at a certain period, the spiral above indicated by a few lines had a most striking resemblance with Donati's⁶ comet⁷.

The indications of the nature of the electric discharge given to me by the magnet seems now, for the greater part, to be exhausted. Its analysis by the prism remains rather unexplored. I would be happy if I could myself show to you the spectra of pure gazes, presented by the positive light on its way to the negative wire. The number of those spectra, observed by myself, is always increasing. The spectrum of mercurial vapour (at ordinary temperature) is the most brilliant and the most instructive too (The light in my tube of Cassiot is produced by this vapour).

The discontinuity of the index of refraction in the gaz-spectra is very extraordinary. If you observe through a telescope a narrow aperture illuminated by electric hydrogen-light, you will see a red band, we may suppose under an angle of 3'-15'. (For my researches I wanted exact measures of angles)[.] In interposing a prism of flint, you will observe *under the same angle* on a dark ground three fine bands, the first one is red, the second bluish green, the third violet. The second band is distant from the first about 3°, the third about 5°. Hence you deduce three different absolute indices for the electric hydrogen-light.

The pure gaz-spectra are only obtained by the current of *positive* light through most rarefied vacua. The light of the negative wire gives the spectrum of the gaz, mixed with bands depending upon the metal of the wire. &c &c – Poggendorff was not so kind to send me copies of my paper, I am not able therefore to send some to England. I am anxious to know Cassiot's new results, perhaps your own, if the seducing power of the object be great enough. Excuse me if I am too long.

Pray, present my respects and compliments to Mad. Faraday and believe myself with all my heart

Yours | Plücker

1. Letter 3489.

2. Plücker (1858d).

3. Plücker (1858e).
4. Plücker (1859a).
5. 'forgive the term'.
6. Giovan Battista Donati (1826–1873, DSB). Astronomer at Florence Observatory.
7. In September 1858 the tail of this comet covered 36° of arc. See *Ann. Reg.*, 1858, **100**: 166–8.

Letter 3547

Faraday to Henry Enfield Roscoe

10 January 1859¹

From the original in RSC MS

[Royal Institution embossed letterhead], 21 Albemarle St. W |
10 Jan 1858 [sic]

My dear Roscoe,

I received your note to day. As to young Vincent² I will answer your enquiries in succession as far as I am able. His moral character is excellent:— he is truthful, temperate, prudent, honorable and *straight forward*:— in respect of intellectual character I believe it to be good; he has written one or two short papers in the philosophical magazine³[.]

As to his qualifications I believe him able & willing to work—at such hard manual work as you express it as is likely to occur in chemical occupation— though may not know the particular practise of a cotton printing laboratory I doubt not he could quickly make himself master of it: for he has now been engaged for more than a year I believe in the analysis &c of Alkali works. The general principles of chemistry he knows very well[.]

His age is 21 years[.]

He is at present engaged in an Alkali work his address being “Messrs Johnstone Alkali works Weston Point near Runcorn, Cheshire” — not indeed very far from Manchester. It is not entirely satisfactory and I think the young man might properly feel that it is not but it is under the condition of a months notice on either side[.]

I have permitted Mr. Vincent to send your letter to his son — but of course he cannot act or do any thing until he hears further about the matter[.] I should think you could write to him direct if this note satisfies any part of your enquiry — or could even see him in Manchester if that were considered important[.]

His father begs to thank you very much for your remembrance of him & his son & thinks well of the possibility — I also beg to thank you heartily for your kindness[.]

Ever My dear Sir | Most truly Yours | M. Faraday
H.E. Roscoe Esqr | &c &c &c

1. Dated on the basis of the reference to Vincent's age.
2. Charles Wilson Vincent.
3. Vincent (1857a, b).

Letter 3548**Faraday to Carlo Matteucci****14 January 1859****From the original in Biblioteca Estense, Modena**

Royal Institution, London | 14 Jan 1859

My dear Matteucci,

I have just had your last letter well written letter (they are always welcome though I have to spell them out) (as I dare say is also the case with you and *mine*) and I hasten to answer the query contained in it. When Melloni¹ sent me a long letter about his views of induction (Static)² and I think before he had published any thing I wrote him also at length answering the cases he laid before me according to my view & so shewing him that I could not accept his³. I do not think the letter was ever published certainly not by me and now my memory is not clear I think I never received any further communication from him but one in reply from his friend the Secretary of the Society of Naples⁴, not in answer to my letter, but acknowledging it & telling me of the death of poor Melloni⁵. I do not know that Melloni had communicated or published my letter, which was altogether meant for his own consideration[.]

I had a letter from Palagi the other day⁶. I thought he had been at Pisa but I find it is Bologna – It contained some simple questions which I thought he would have asked at you if you had been within reach of each other[.]

Having nothing more to say I will not waste more with or more of your time & *patience* with the bad writing than to say with kindest remembrances to Mad Matteucci that I am as ever Truly Yours | M. Faraday

Many thank[s] for the Cimento which arrives regularly | MF

1. Macedonio Melloni (1798–1854, DSB). Director of the Physics Conservatory in Naples.

2. Melloni to Faraday, 1 July 1854 and 12 July 1854, letters 2862 and 2865, volume 4.

3. Faraday to Melloni, 31 July 1854, letter 2870, volume 4.

4. Vincenzo Flauti (1782–1863, LUI). Mathematician and Secretary of the Royal Academy of Sciences, Naples.

5. Flauti to Faraday, 12 August 1854, letter 2875, volume 4.

6. Letter 3545.

Letter 3549**Benjamin Collins Brodie to Faraday****14 January 1859****From the original in IET MS SC 2**

14 Saville Row W | 14 Jan 1859

My Dear Sir,

Pray do not think that in what I said last night¹ I alluded in any way to the observations made in a lecture of yours formerly on the *force of gravity*²; which indeed were not at the time at all present to my mind, & which, if

my recollection be accurate, were based on an entirely different view of the subject.

All that I intended to say was that in physics not less than in metaphysics, there are boundaries beyond which, not only from the want of opportunities of experience, but also from the want of adequate powers of comprehension, the Human Intellect can not penetrate; & that we must be content to accept certain facts as well established, of which we can offer no explanation by referring them to any more general principle. For example: that there are good grounds for believing that there is something pervading the universe corresponding to Newtons notion of an Ether, I do not doubt; but if we venture further than this, & attribute the mutual attraction of masses of matter to the operation of such an ether, we do but substitute one simple fact for another simple fact, & endeavour to explain one thing by another thing which is equally inexplicable.

I dare say that you have forgotten, but I have not forgotten a conversation which I had with you long ago, in which I believe that you expressed yourself as agreeing with me in the opinion that of the ultimate structure of material bodies we neither know, nor can have any actual knowledge, & that neither the ordinary hypothesis of solid impenetrable molecules, nor Boscovich's³ hypothesis of mathematical points, which are centres of attraction & repulsion, is anything more than a contrivance for bringing these things down to the level of all limited comprehension⁴.

I owe you some apologies for occupying so much of your time, & I can only make up for it by asking you not to think it necessary to take the trouble of writing an answer to what I have written.

Yours most truly | B.C. Brodie
M. Faraday Esqr.

1. At the meeting of the Royal Society where, with Brodie in the chair, Gassiot (1859) was read. *Proc. Roy. Soc.*, 1859, 9: 600–5.

2. Faraday (1857a), Friday Evening Discourse of 27 February 1857.

3. Roger Joseph Boscovich (1711–1787, DSB). Jesuit natural philosopher.

4. Boscovich (1763). On this see James (1993).

Letter 3550

Amherst Hawker Renton¹ to Faraday

14 January 1859

From the original in GL MS 30108/3/92

1 Hanover Chambers, Buckingham St, Adelphi WC | 14 Jan 1859

Dear Sir,

Having recently solicited an exhibition of an Improved Oxyhydrogen Lime light before the Elder Brethren of the Trinity House it was a matter of considerable disappointment that you were not present, and I am requested

by the proprietors of the light to solicit the favor of your visit to Westminster Hall on Saty (tomorrow) afternoon at 5 oclck when the same light will be exhibited privately as a preliminary step to the illuminating of the Hall during the early sitting of Parliament_[.]

I am | Dear Sir | Yours obediently | Amht H. Renton
Dr. Faraday | &c &c

1. Amherst Hawker Renton (d.1889, age 85, GRO). Civil engineer.

Letter 3551

Faraday to Mary Somerville

17 January 1859

From the original in Bod MS Somerville dep b.206 MS2 CELE 8

Royal Institution | 17 Jan 1859

My dear Mrs. Somerville,

So you have remembered me again, and I have the delight of receiving from you a new copy of that work which has so often instructed me; & I may well say, cheered me in my simple, homely course through life in this house. It was most kind to think of me; but ah how sweet it is to believe that I have your *approval* in matters where kindness could do nothing,— where judgment alone must rule. I almost doubt myself when I think I have your approbation, to some degree at least, in what I may have thought or said about gravitation, the forces of nature, their conservation &c.¹; and yet I do not feel that you are reproving such thoughts. As it is *I cannot* go back from them; on the contrary, I feel encouraged to go on by way of experiment; but am not so able as I was formerly;— for when I try to hold the necessary group of thoughts in mind at one time, with the judgment suspended on almost all of them, then my head becomes giddy & I am obliged to lay all aside for a while. I am trying for *time* in magnetic action, and do not despair of reaching it, even though it may be only that of light. Nous Verrons.

I have been putting into one volume various papers of mine, as Experimental Researches in Chemistry and Physics². The index & title page has gone to the printer & I expect soon to receive copies from him. I shall ask Mr. Murray to help me in sending one to you, which, I hope you will honour by acceptance. There is nothing new in it except a few additional pages about “regelation”³ and also “Gravity”⁴ — It is useful to get ones scattered papers together with an index, and Society seems to like the collection sufficiently to pay the expences.

I would ask after friends with you, but since the loss of memory have made many sad mistakes and have become afraid. Pray remember me most kindly to all with whom I may take that privilege: and believe me to be most truly

Your admirer and | faithful servant | M. Faraday

1. Somerville, M. (1858), 354–7.
2. Faraday (1859b).
3. *Ibid.*, 377–82.
4. *Ibid.*, 460–3.

Letter 3552

Faraday to Burnside¹

19 January 1859

From the original in FACLM H MS c1

[Royal Institution embossed letterhead]

Mr. Faradays compliments to Mr. Burnside – Machines of Gutta Percha were made some years ago by the Gutta Percha Company in the City Road – but they all failed in practice. Mr. Burnside could probably hear of them there[.]
19 Jan'y 1859

1. Unidentified.

Letter 3553

Faraday to Christian Friedrich Schoenbein

19 January 1859

From the original in UB MS NS 443

Royal Institution | 19 Jan 1859

My dear friend,

I have received your last of the 13th instant¹. You will be weary of my thanks nevertheless I send them[.]. The peroxide of barium I have has been very good in all former experiments – I hope tomorrow it will prove as good in those I shall report from your last – I do not at all doubt it. The evening does not come off until the 25th of next month² but I have sent the tickets to Hampstead to Mr. Rumpf³ – and Miss Schoenbein – also to Miss Hornblower & others whom you know more or less. – I have had some of the German papers translated and hope I have got hold of the subject thoroughly if I can only keep it but memory is most treacherous & I am obliged to look at every reading to see whether ozone is \ominus or \oplus . I stick it before my eyes but that is a clumsy way[.]

You seem to me to be leading a very gay life. Well, I am happy you have health strength & spirits to do so:– that they may long continue with you is the earnest wish of

Ever Yours | M. Faraday

Dr. Schoenbein | &c &c &c

1. Not found.
2. Faraday (1859a), Friday Evening Discourse of 25 February 1859.
3. Unidentified.

Letter 3554**Emil Dessewffy¹ and Ferenc Toldy² to Faraday****20 January 1859****From the original in RS MS 241, f. 154**

MAGYAR AKADEMIA | Academia Scientiarvm Hvngarica
 Illustri Viro Michaeli Faradayo S[alve]

Academia Scientiarum Hungarica, auctoritate Imperatoris Regis Hungariae Apostolici Aenuo Saracita, in concilio die XVI mensis Decembris Anni praeteriti habito, Te Vir insignis ob immortalia in doctrinas physicas merita Sua, unanimis votis in collegium membrorum Suorum correspondentium cooptavit; Archidux vero Serenissimus Albertus³, Regni Gubernator actum hunc die XVIII instem mensis ratum habuit.

Sit Tibi, Vir Illustris, electio haec testies eximii cultus, quo in Nomen Tuum iusta laude celebratum Academia haec fertur.

Deus vero O[ptimus].M[aximus]. vitae Tuae in augmen, tum liteiarum propitius faveat.

Scripsimus Pesthini in Hungaria, XX die Jannarii Anni M.DCCC.LIX

[word illegible] Comes Dessewffy | Praeses.

Franciscus Toldy | Ac Hung. ab epistolis
 Faraday Mihály

TRANSLATION

Hungarian Academy | Hungarian Academy of Sciences
 To the illustrious Michael Faraday, greetings.

The Hungarian Academy of Sciences, by the authority of the Apostolic King of Hungary Aenuo Saracita, in a council held on the sixteenth day of the month of December of last year, by a unanimous vote elected you into the fellowship of its corresponding members, [which it did], distinguished Sir, on account of your immortal services to physics; and the Most Serene Archduke Albrecht³, the Governor of the Kingdom, has ratified this act on the eighteenth day of the present month.

Let this election, illustrious Sir, be a witness to you of the exceptional esteem in which this Academy holds your justly praised and celebrated name.

May God, the Best and Greatest, look upon your life with favour for the increase of learning.

We have written [this] in Pest in Hungary, on the twentieth day of January of the year 1859.

[word illegible] Count Dessewffy | President

Fernec Toldy | Hungarian Academy. By letters
 Faraday Mihály

1. Emil Dessewffy (1814–1866, MN). Politician and President of the Hungarian Academy of Sciences, 1855–1866.

2. Ferenc Toldy (1805–1875, MN). Physician and Secretary of the Hungarian Academy of Sciences, 1835–1861.

3. Albrecht Friedrich Rudolf von Habsburg-Lothringen (1817–1895, OBL). Governor General of Hungary, 1851–1860.

Letter 3555

Charles Locock to Faraday¹

27 January 1859

From the original in RI MS Conybeare Album, f. 30

Hertford Street. May Fair | 27 Jan 1859

My dear Mr. Faraday,

Will you allow me to be the means of introducing to you Mr. Bowles², who wishes to consult you on some point of chemistry.–

Yours very faithfully | C. Locock

1. Charles Locock, (1799 – 1875, ODNB). Retired obstetrician.

2. Unidentified.

Letter 3556

Nassau William Senior to Faraday

28 January 1859¹

From the original in IET MS SC 2

9 Hyde Park Gate, Kensington | 28 Jan

My dear Mr. Faraday,

I send the four first volumes of my Egypt².

If you find these worth reading you shall have the remainder–

Ever | N. Senior

1. Dated on the basis that letter 3559 is the reply.

2. According to Levy (1970), 161 Senior was in the habit of lending his manuscript journals to a limited group of friends. He had visited Egypt in late 1855 and early 1856 and his diaries relating to this journey are now in National Library of Wales, MS Senior, A22–5.

Letter 3557

Mary Somerville to Faraday

1 February 1859

From the original in RI MS F1 I56

Florence | 1 Feb 1859

My dear Dr. Faraday,

I cannot tell you how much I have been delighted and gratified by your letter¹, and by your kind acceptance of my book². I should not have dared to send it to you from any merit it may have in itself, but I have no other way

of offering the tribute of my most sincere and heartfelt admiration of your transcendent discoveries of the laws and deep mysteries of nature.

I fear from what you say that I may have expressed myself ambiguously with regard to your views of gravitation³. I certainly did not mean to do so, for on the contrary, they convey to my mind the most perfect conviction, and I only hope you may live to complete what Newton began, by the discovery of that one comprehensive power of which gravity and all the correlative and convertible forces are but parts. Mean while I wish you success in your research for time in magnetism which there can be no doubt you will accomplish having already so beautifully connected magnetism with light whose velocity is known. I fear I tax your health too severely; subjects so abstruse as you are accustomed to consider must fatigue even your mind which makes occasional repose necessary, so I wish you would come here & amuse yourself for a little, we should be indeed delighted to see you, and there are many things that would interest you.

Many thanks for the volume of your papers and researches⁴ which you intend to send to me it will be a very precious gift – Mr. Somerville⁵ and my daughters⁶ desire to be kindly remembered to you and be assured that I am ever

with sincere friendship yours | Mary Somerville

1. Letter 3551.

2. Somerville, M. (1858).

3. *Ibid.*, 354–7.

4. Faraday (1859b).

5. William Somerville (1771–1860, ODNB). Physician to the Royal Chelsea Hospital, 1819–1838.

6. Martha Charters Somerville (1815–1879, *The Times*, 10 November 1879, p. 10, col. d) who later edited Somerville, M.C. (1873) and Mary Charlotte Somerville (1817–1875, Patterson (1983), 260 and will in PRFDHC).

Letter 3558

Peter Henry Berthon to Faraday

2 February 1859

From the original in GL MS 30108/2/78

Trinity House, London, E.C | 2 Feb 1859

Sir,

I am directed to transmit to you the accompanying Four Samples of Red Lead, and to request you will be so good as to analyze the same, and favor me with the result for the Board's information.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq. | &c &c &c

Letter 3559**Faraday to Nassau William Senior****4 February 1859****From a photocopy in RI MS**

[Royal Institution embossed letterhead] | 4 Feb 1859

My dear Mr. Senior,

I have read with much interest your notes of Egypt¹ and now return them I trust quite safe[.] I owe you many thanks for your kindness & for the pleasure they have given me[.]

Ever Truly Yours | M. Faraday

1. See note 2, letter 3556.

Letter 3560**Faraday to Peter Henry Berthon****7 February 1859****From the original copy in GL MS 30108/2/78**

Royal Institution | 7 Feb 1859

Sir,

I have carefully analyzed & examined the four samples of Red lead labled as under | 1 | 2 | 3 | 4¹

I do not find in any of them any impurity or adulteration; or substance added to make weight. In that respect they are all alike & unobjectionable. There are I believe variations in quality dependant upon the mode of manufacture & the care taken in the operation but on these points I do not profess to be a judge[.] As far as I can see they are well manufactured articles²[.]

I am Sir | Your faithful Servant | M. Faraday

P.H. Berthon Esqr | &c &c &c

1. See letter 3558.

2. This letter was read to Trinity House Wardens Committee, 8 February 1859, GL MS 30025/25, p. 254.

Letter 3561**Nassau William Senior to Faraday****7 February 1859¹****From the original in RI MS F1 I118**

7 Feb

My dear Faraday,

As the first part of Egypt interested you, I send to you the conclusion².

If you wish for any more of the same sort of literature tell me

I'm | N. Senior

1. Dated on the basis that this is the reply to letter 3559.
2. See note 2, letter 3556.

Letter 3562**Peter Henry Berthon to Faraday****7 February 1859****From the original in GL MS 30108/3/93**

Trinity House | 7 Feb 1859

Dear Sir,

A Complaint having been made by Mr. Neale¹, Principal Keeper of the Hurst Lights, relative to the injurious effects, upon his health, of the Drinking Water at that Station, I am directed to forward to you the accompanying Sample Bottle of the same, which has been obtain'd by desire of the Elder Brethren, and to request you will favor them by analysing its Contents, & reporting the result for their Information.

I am also directed to enclose herewith a Copy of the Light Keepers Letter on the subject², and of the Surgeon's Statement to which it refers³; together with a Copy of the Agents Report⁴ in relation to the manner in which the Water alluded to is collected.–

I am | Dear Sir | Yours faithfully | P.H. Berthon
M. Faraday Esq. D.C.L. | &c &c &c

1. George Neale (d.1877, age 63, GRO). Previously keeper of St Catherine's Lighthouse, Isle of Wight, now at Hurst Point Lighthouse opposite the island.
2. Neale to Berthon, 29 January 1859, GL MS 30108/3/93.
3. Statement by C. Fluden, 28 January 1859, GL MS 30108/3/93.
4. Willis to Berthon, 4 February 1859, GL MS 30108/3/93.

Letter 3563**Emilie Schoenbein to Faraday****8 February 1859****From the original in UB MS NS 445**

Stamford Hill | 8 Feb 59

Dear Mr. Faraday,

Emboldened by your great kindness, I venture to take advantage of your offer of one more ticket in my father's name¹. This subject of Ozone being one that seems particularly interesting to medical gentlemen, I make this request in behalf of one of them.

I must add that my father is quite envious for this great privilege that is in store for me of hearing your lecture.

Believe me, dear Mr. Faraday | very Truly yours | Emilie Schoenbein

Endorsed by Faraday: Died at $\frac{1}{2}$ p 7 oclk A.M. on the 13th Feby 1859 – MF

1. For Faraday (1859a), Friday Evening Discourse of 25 February 1859.

Letter 3564

Faraday to Emilie Schoenbein

11 February 1859

From the original in UB MS NS VII D

My dear Miss Schoenbein

I have just come to town & hasten to send the ticket¹. Write in the name if you please[.]

Ever Truly Yours | M. Faraday

Friday | 11 Feb 1859

1. For Faraday (1859a), Friday Evening Discourse of 25 February 1859.

Letter 3565

Faraday to Peter Henry Berthon

11 February 1859

From the original copy in GL MS 30108/3/93

Royal Institution | 11 Feb 1859

Dear Sir,

Since my return to town I have carefully analyzed the water from Hurst point which you sent me¹. It is very bad for culinary use and must be injurious to health. The history of it is this. Being rain water originally (as its state proves) it would if in an inland situation be collected by the roofs & be conducted by leaden gutters without any probable harm. But at Hurst point the wind drives sea spray on to the roofs & gutters & the salt in that spray acts upon the lead of the pipes &c & dissolves it[.] Such lead *remains* in solution as I find is the case in this instance. The salts which occur in the water are the salts derived from the spray[.]

I think cases like this have happened before in lesser degrees[.] They will occur in a greater or smaller degree whenever the spray of the sea may be caught by the roof – the water from which roofs being conducted by leaden gutters or pipes or collected in leaden tanks – The degree of poisoning will of course vary in different weathers as well as in different situations[.]

I am Dear Sir | most faithfully Yours | M. Faraday

P.H. Berthon Esqr | &c &c &c

4 pints of the water gave 0.4 of grain of dry sulphate of lead

1. See letter 3562.

Letter 3566**Peter Henry Berthon to Faraday****12 February 1859****From the original in GL MS 30108/3/93**

Trinity House | 12 Feb 1859

My dear Sir,

I am in receipt of your report upon the Water sent up from Hurst¹, and am requested by Capt Close to say that for many years the Light Keepers as well as the Gunners of the Battery there have had none other water for drinking than that alluded to, and that until now no complaint has been made against it.— he wishes however to know whether you consider that the deleterious effect of the Lead, which it is evident it contains, can be exhausted or neutralized by the use of any of the numerous descriptions of filters recently invented and if so what you would recommend as the most efficacious.

May I beg the favor of as early a reply as you can conveniently give me.

Yours | Very faithfully | P.H. Berthon

Professor Faraday | &c &c &c

1. Letter 3565.

Letter 3567**Faraday to Henry Bence Jones****13 February 1859****From the original in RI MS F1 E16**

[Royal Institution embossed letterhead] |

Sunday Morning, 13 Feb 1859

My dear Bence Jones,

If you read the two notes herewith you will see the case of Miss Schoenbein. The last note has just reached me & I am going now from your door[.] It is a long way off.

Miss Hornblower | Stamford Hill

but I could not do otherwise than shew the notes to you. If death be near how shall I write to her father¹. But the day is a day² of trust & we may leave all to him who rules all for the best[.]

Ever Yours | M. Faraday

1. See letter 3569.

2. See, for example, 2 Kings 19: 3.

Letter 3568**Faraday to Henry Bence Jones****15 February 1859¹****From the original in RI MS F1 E17**

[Royal Institution embossed letterhead] |

Tuesday Morning, 15 Feb

My dear friend,

My cold with sore throat is just now so heavy that I durst not venture out to you, so I send Jeannie, because I think I ought not to defer the matter of Miss Hornblowers note, which is in the 3 & 4th pages. I send what she says. I know its utter inadequacy; but if you will let me double it, I shall be very glad.

My cold is going on all right.

Yours Ever | M. Faraday

Dr. B. Jones | &c &c &c

1. Dated on the basis that this letter refers to the death of Emilie Schoenbein on 13 February 1859 and Faraday's illness referred to in letter 3569.

Letter 3569**Faraday to Christian Friedrich Schoenbein****16 February 1859****From the original in UB MS NS 444**

Royal Institution | 16 Feb 1859

My dear friend,

I must write, not knowing but that you may walk in during the act. I have delayed thus long thinking that possibly when my letter got to Basle you might be here:— but whatever may be the case I must write. If you do not get my letter Mrs. Schoenbein will & though Miss Hornblower wrote off to her on Monday¹ immediately that she knew the cause of your dear daughters death and I can say nothing in the way of information more than she can still my letter will not be wrong. Last Thursday² I think they had sent to us to learn Dr. Bence Jones' hours intending to see him on the Saturday perhaps. On Sunday morning³ as I was dressing about $\frac{1}{2}$ p. 7 o clk a messenger brought me a note which telling me of Miss Schoenbeins very serious state, sent me first to Dr. Bence Jones & then to Stamford Hill⁴: but I was too late to see the poor girl alive[.] She died at $\frac{1}{2}$ p. 7 oclk. Dr. Bence Jones came in very soon after and then we telegraphed off to you the first time[.] In the evening of

the same day Sunday, I sent off the second telegraph message – On Monday Morning an examination took place Dr. B. Jones being present & he tells me it was perforation of the stomach – a matter which could neither be foretold nor distinguished during life (for there was no sickness) nor aided if known and so her end came and as I understand with great peace of mind as to the future, though with much present pain of body.

We do not know what to expect whether you are coming or not. Perhaps even now there is news of you at Stamford Hill but we are some miles apart & unfortunately I have been ill & am confined to this house. I expect to hear from Miss Hornblower in the course of a few hours. You will either by letter or in person instruct her what to do but if nothing is heard from you the burial must take place on Friday next⁵. Miss Hornblower told me she had had a telegraphic message from you but they are of necessity very brief. I left word with Miss Hornblower that if you come and it suited you we should be very glad to make your home *here* for the time. There are some friends of Miss Schoenbein at Hampstead & I think also in Warwickshire where she spent her holidays. They have been informed & I believe one of them a Clergyman from Warwickshire purposes being at the funeral but I am very imperfectly informed of these matters which are all held doubtful until it is known what you will do.

I write to you though I think you may not be a Basle and once I thought of writing to her Mother. This letter indeed is as much to Mrs. Schoenbein as to you. Your good daughter had made unto herself friends who thought very much of her and I grieve to think she will not return to you to be a comfort to you both in future years. But Gods will be done. You may think of her with great though melancholy pleasure. She was full of thought latterly about you & the Ozone evening⁶. I send you a note of hers to me only 5 days before her death⁷.

My dear Wife & Niece as knowing Miss Schoenbein join with me in all sympathy with you both & your children. My nieces sisters⁸ have been indebted to her care for them at Stamford Hill. Associations in every way have risen about her – poor girl, & she will be mourned by many & for some time.

My dear friend – I can write to you about nothing else and I can do no good in writing – I simply grieve again & again for your loss & ours.

Most Affectionately Yours | M. Faraday

Dr. Schoenbein | &c &c &c

Address: Dr. Schoenbein | &c &c &c | Basle | Switzerland

1. That is 14 February 1859.

2. That is 10 February 1859.

3. That is 13 February 1859.
4. See letter 3567.
5. That is 18 February 1859.
6. Faraday (1859a), Friday Evening Discourse of 25 February 1859.
7. Letter 3563.
8. Katharine Barnard (1844–1912, GRO) and Rachel Barnard (1845–1929, GRO, death under Blaikley). The youngest surviving children of John and Margaret Barnard. See letter 3337.

Letter 3570

Christian Friedrich Schoenbein to Faraday

20 February 1859

From the original in UB MS NS 446

Bâle | 20 Feb 1859

My dearest friend,

I enclose a few lines to acknowledge the kind letter of Mrs. Faraday's and your niece's, which really produced a soothing effect upon our harrowed minds and bleeding hearts; and it is particularly Mrs. Schoenbein that feels most thankfully for that proof of friendly and sympathizing feelings. Mr. Crowdy¹ of Winchester and Miss Mayo² of Hampstead friends of mine and Emilia's have most kindly and spontaneously offered to me to put a tombstone upon the grave of my deeply lamented daughter and ask my permission to do so as a favor. We were deeply touched by the delicate expression of their friendly feelings and gratefully accept of their kindness offered; but nevertheless I should consider it as the most grateful deed of yours if you would join your dear name to their's. Mrs. Schoenbein's bodily health is, thank god, nearly reestablished, but the sadness of her heart as yet very great; there are however intervals of tranquil resignation to the inscrutable decree of heaven. I have begun my lectures again, but in what state of mind I leave you to imagine. The whole world has become stale and insipid to me, has even assumed a sad appearance.

Pray offer my most grateful thanks to Mrs. Faraday and your niece and pity | your deeply mourning | friend

Address: Doctor Michael Faraday | Royal Institution | Albemarle Street | London

1. Anthony Crowdy (d.1889, age 87, GRO, AO). Church of England priest.

2. Possibly one of the nieces of Schoenbein's friend the school teacher Charles Mayo (1792–1846, ODNB) listed in Mayo, C.H. (1908), 285.

Letter 3571**Faraday to John Murray****21 February 1859****From the original in the possession of Roy Deeley**

[Royal Institution embossed letterhead] | 21 Feb 1859

My dear Sir,

I enclose you the ticket¹ – & am very glad you are in time[.]

Ever Your Obliged | M. Faraday

J. Murray Esqr | &c &c &c

1. Presumably for Faraday (1859a), Friday Evening Discourse of 25 February 1859.

Letter 3572**Faraday to Christian Friedrich Schoenbein****24 February 1859****From the original in UB MS NS 447**

Royal Institution | 24 Feb 1859

My very dear friend,

I received your touching letter¹ and I am glad you would receive mine² about the same time. Your cry of anguish may well pierce our hearts here for if the effect of the blow was stunning to us how much more would it be so to you. And that you should at the same moment be burdened with the heavy weight of Mrs. Schoenbein's illness! I do indeed grieve for you – but I hope you are by this time somewhat relieved in respect of that heavy home anxiety[.] Do tell her how we feel for her and the two poor sisters – I am glad you did not come here for your first duty was at home to succour and support those dependent on you – You could well trust Miss Hornblower for she had learnt to love your daughter. I have no doubt she has written of all things personal to you & Mrs. Schoenbein & will fulfill all your possible wishes. I expect too that by this time you will have had letters from Dr. Bence Jones – Grove & others for I have shown your letters to me & Miss Hornblower unto them.

You mentioned the matter of a tombstone in your letter³ to me and affectionately desired to have my name by yours on it. I suppose this is usual with you but with us it is very rare or even unknown and would excite much remark – that would be of no consequence if the remarks were indifferent in their nature but they would here be sure to carry a religious feeling or meaning with them and as I am known to be a dissenter strongly differing from the Episcopal church here would give rise to much remark among those who know me. I understand too that a dear friend of your daughter (I think the name is Crowdy⁴) has written to you direct about the inscription on the

stone. I believe he performed the funeral service but as I knew it would be a Church of England service in which I could not conscientiously join I was not there. However his letter with Miss Hornblowers communications will bring about the fulfilment of the proper arrangements.

Poor girl – (happy girl I well may say considering her strong hope in death) we were hoping to have her with us tomorrow evening⁵ – but how vain are all our plans. Instead of a glad and buoyant heart I shall go to my work, as *work* indeed. I was desiring to put it off but when I began to look about for the purpose I found so many engagements had been made contingent upon the evening, and that even the Prince Consort was coming, that I could not properly change the date – I only hope that I shall not break down – I know I shall not be able to forget for the hour; and an overpowering thought may break in[.]

I hope that you are beginning to turn a little to occupation – I know how distasteful it will be – but you must be drawn away at times from the heavy thought; even though the exertion may be painful it will be healing. Do think of this for the sake of yourself and your family and your friends and may God give you that grave & gentle consolation by degrees which you ask for in your letter to me[.]

Ever My dear Schoenbein | Your Affectionate friend | M. Faraday

1. Judging by the content of this letter, Schoenbein appears to have written at least two letters to Faraday at this time of which only letter 3570 has been found.

2. Letter 3569.

3. Letter 3570.

4. Anthony Crowdy (d.1889, age 87, GRO, AO). Church of England priest.

5. For Faraday (1859a), Friday Evening Discourse of 25 February 1859.

Letter 3573

Faraday to Richard Charles Coxe¹

March 1859

From the original in RCP

<A>lbemarle St. W | [MS torn] Mar 1859²

My dear Archdeacon,

I am daily subjecting the dearest thing I have in the world namely my wife to the currents of electricity in the *faint* hope that it may do good & improve muscular power but in the full assurance that it will do no harm. I do not know what good the Newcastle Man may do but I do not think he will cause any mischief unless he adopts very violent means[.] If after enquiry the person gave me reasonable proof that he had produced good in other cases I should <let him try it>³. I wish he may be successful & that you may tell me rejoicingly that such is the case[.] Electricity ought to do us good of this kind but I fear we do not know as yet the laws which would enable us to apply it properly[.]

Ever Your | faithful Servant | M. Faraday

The Very Reverend | Archdeacon Coxé | &c &c &c

1. Richard Charles Coxé (1800–1865, ODNB). Archdeacon of Lindisfarne, 1853–1865.
2. The Royal Institution embossed letterhead has been cut off.
3. This has been inserted in another hand below the cut portion of the manuscript.

Letter 3574

Peter Henry Berthon to Faraday

2 March 1859

From the original in GL MS 30108/2/89

Trinity House, London, E.C | 2 Mar 1859

Sir,

I am directed to transmit to you the accompanying Three Samples of White Lead, and to request you will be so good as to analyze the same, and favour me with the result for the Board's information.—

I am | Sir | Your most humble servant | P.H. Berthon
M. Faraday Esq | &c &c &c

Letter 3575

Faraday to Peter Henry Berthon

5 March 1859

From the original copy in GL MS 30108/2/89

Royal Institution | 5 Mar 1859

Sir,

The three samples of white lead which you sent me¹ having been analyzed answered every enquiry as to the presence of any adulteration in *the Negative* and being good I have nothing to add to or specify in my report upon them except the quantity of oil ground up with each. This was as follows.

Oil in Pilcher lead	7.1 per cent
S & W. Tudor ² lead	7.0
Pontifex & Wood lead	7.8 ³

I am Sir | Your Very Obedient Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. With letter 3574.
2. Samuel and William Tudor. White lead suppliers of 166 and 167 Upper Thames Street. POD.
3. This letter was read to Trinity House Wardens Committee, 8 March 1859, GL MS 30025/25, p. 289. The order was placed with Tudor.

Letter 3576**Faraday to Samuel Alfred Varley¹****5 March 1859****From the original in the possession of Joan Varley**[Royal Institution embossed letterhead], R Institution |
5 Mar 1859

Dear Sir,

I cannot remember the words I used on the occasion referred to² & how much one part was qualified by another but I can tell you what my present thought is[.] That amount of inductive force which is occupied *for the time* in charging the near part of the wire is taken off so to say from the wire & makes it go slower than if there was no lateral induction. The larger the wire with the same thickness of the surrounding gutta percha the greater the conduction & therefore the reduction of the forward tension[.] But as a large wire facilitates conduction in proportion to its mass whether in air water or gutta percha so a large wire must as respect[s] conduction be better than a small one only respect must be had to the conduction effect. My expectation is that as the induction is as the surface of the wire but the conduction as the square of the surface ie as the mass so the large wire as a whole will have less proportional induction compared to conduction than the small one.

As to giving opinions on or making observations on your paper & the controverted points referred to in it I must decline that being always anxious to avoid controversial matter. You are quite able to hold your own there³[.]

Ever Truly Yours | M. Faraday

S.A. Varley Esq | &c &c &c

1. Samuel Alfred Varley (1832–1921, ODNB). Telegraph engineer.

2. This probably refers to Faraday's participation on 27 January 1857 in an extended discussion of submarine telegraph cables at the Institution of Civil Engineers, *Min. Proc. Inst. Civ. Eng.*, 1857, 16: 220–2.

3. See Varley (1858) read, in abstract, to the Institution of Civil Engineers on 30 March 1858. This does not refer to Faraday.

Letter 3577**Faraday to Ferenc Toldy¹****7 March 1859****From the original in Hungarian Academy of Sciences MS RAL 128/1859**

Royal Institution, London | 7 Mar 1859

Sir,

I feel very deeply the high honor which the Academia Scientiarum Hungarica has, through you, conferred upon me, by its reception of me into the number of its corresponding members². I wish I could promise to deserve

this high favour by the production of some new results to Science. It is of the nature of such a stimulant, to make me wish to do so; but the weight of work and years warns me to make no rash promises. Pray do me the favour to thank the Academy in my name and say I will do all I can to deserve its favour[.]

Believe me to be with the highest respect | & esteem | Sir | Your most grateful humble Servant | M. Faraday
A M. | M. Franciscus Joldy [sic] | &c &c &c &c

1. Ferenc Toldy (1805–1875, MN). Physician and Secretary of the Hungarian Academy of Sciences, 1835–1861.

2. See letter 3554.

Letter 3578

Faraday to William Crookes¹

9 March 1859

From a photocopy in RI MS

[Royal Institution embossed letterhead], Albemarle St. W |
9 Mar 1859

My dear Sir,

I am very much obliged by your letter & offered kindness – I do not know whether I shall decide upon producing at our table an experiment which being as yet uncertain & unsettled in its nature is already under good investigation elsewhere:– at present I think I would rather wait for the full development. But if I do resolve to use the evidence Mr. Wheatstone has placed in my power I shall feel much obliged to you indeed for the assistance you so kindly offer me[.]

Ever My dear Sir | Most truly Yours | M. Faraday
Wm. Crookes Esq

1. William Crookes (1832–1919, ODNB). Former student at the Royal College of Chemistry and journalist.

Letter 3579

Faraday to James David Forbes

22 March 1859

From the original in SAU MS JDF 1859/17

[Royal Institution embossed letterhead], Albemarle St. W |
22 Mar 1859

My dear Professor Forbes,

I have received your volume¹ and thank you very heartily for it. I am afraid I shall never see the Glaciers again so as to connect it

with them by reading & observation on the spot: but [it] is wonderful what you and others have made us to know about them since the time of Saussure. What a fine physico-mechanical problem they present and what an added pleasure there is for the intelligent mind which may look at them now with understanding. Great as their beauty & singularity was the comprehension in part of their nature & cause very greatly enhances it[.]

Ever Truly Yours | M. Faraday

1. Forbes (1859).

Letter 3580

Faraday to Jeffrey¹

2 April 1859

From the original in National Library of Australia MS 760/8/145

2 Apr 1859

Mr. Faraday supposes De la Beche's² work called the Geological Observer³ will suit Mr. Jeffrey – He cannot give an opinion on the Nat Histy works mentioned[.]

1. Unidentified.
2. Henry Thomas De La Beche (1796–1855, ODNB). Director of the Geological Survey from 1835.
3. De La Beche (1851).

Letter 3581

Faraday to Thomas Stevenson

11 April 1859

From the original in NLS MS 785, f. 44

[Royal Institution embossed letterhead], Albemarle St. W |

11 Apr 1859

Dear Sir,

I hasten to express my sincere thanks to you for a copy of your account of Lighthouse illumination¹[.] I know before hand its value & by that measure the pleasure I shall have in reading it[.]

Ever Truly Yours | M. Faraday

Thos. Stevenson Esqr | &c &c &c

1. Stevenson (1859a).

Letter 3582**William John Macquorn Rankine¹ to Faraday****13 April 1859****From the original in IET MS SC 2**

59 St. Vincent Street, Glasgow | 13 Apr 1859

My Dear Sir,

I feel much obliged by your letter of the 11th, which by a few words has very materially aided my comprehension of your opinion on the “Force” of Gravity².

I beg leave to send to you a copy of a note³ to my paper on the Conservation of Energy⁴, which the remarks in your letter have suggested, and which I propose to send to the Philosophical Magazine.

I am | My Dear Sir | Yours Very faithfully | W.J. Macquorn Rankine
Michael Faraday Esq. D.C.L. | &c &c &c

1. William John Macquorn Rankine (1820–1872, ODNB). Regius Professor of Civil Engineering at Glasgow, 1855–1872.

2. See Faraday (1859b), 460–3.

3. Rankine (1859b).

4. Rankine (1859a).

Letter 3583**Faraday to Juliet Pollock****14 April 1859****From the original in RI MS F1 E18**

[Royal Institution embossed letterhead], Albemarle St. W |

14 Apr 1859

My dear Mrs. Pollock,

I am sorry I cannot promise you the proxy¹[.] I am engaged three deep unless some of the three candidates are elected in the mean time. If they go off though that is not likely – you shall have it[.]

We are all pretty well & thank you heartily. Jane is at Hampton Court by Doctors orders & will remain there I believe. The reports about the author² of Adam Bede³ are I find very general & very uncertain. How clever the book is. That Mrs. Poyser I hope she will live again & again and the Schoolmaster. What may we not expect from such an author?

Ever | My dear Mrs. Pollock | Most truly Yours | M. Faraday

1. A reference to the London Orphan Asylum, see note 2, letter 3516. Juliet Pollock is not listed as a subscriber in the records of the asylum in SuRO, but there was no prohibition on non-subscribers seeking to place an orphan in the Asylum.

2. For a discussion about the attempts at this time to identify George Eliot see Haight (1968), 284–7.

3. Eliot (1859).

Letter 3584**Faraday to Carlo Matteucci****15 April 1859****From the original in BPRE MS Regg E 210/11 doc. 2**

London | 15 Apr 1859

My dear Matteucci,

Since I received your last I have had reason to regret that I enter so little as I do into the world for thereby I am left unfit to help you in your present earnest exertions_[.] Though I am not political I can feel deeply for one who like you thinks of his countrys welfare & would do his best according to his own views and powers to promote it_[.] It is indeed a sad pity that Italy should be as it is & the intellectual & mental powers of its people drawn away from the fine pursuits which belong to peace & good government that they may be devoted to a conflict with evil & the conflict of brute force with force¹.

I have no connection of any kind however distant with the "Times" paper_[.] I have sought for some since I received your letter but cannot find any_[.] I have taken from my draw the only copy I had of your translated paper – have crossed out the translators note in p. 3 & made the correction at p. 4 and I have sent it off to the times office but unless the Editor accepts it for its own sake I fear that the hands it goes through will not influence him much. Nevertheless that was all I could do. Being with your name it ought to have its full weight & force & I trust it may be so.

I hope Madame Matteucci is well & wish you every good wish. But I dare say that all your desires are just now directed towards the state of Italy. Who can tell how that may come out – it seems impossible that it should stand still_[.]

Ever my dear Matteucci | Your Most truly | M. Faraday

1. A reference to the war between France and Austria over the control of Italy. See *Ann. Reg.*, 1859, **101**: 188–261.

Letter 3585**Peter Henry Berthon to Faraday****15 April 1859****From the original in GL MS 30108/3/94**

Trinity House | 15 Apr 1859

My dear Sir,

The New Lighting Apparatus for the North Foreland is now ready for your inspection here, – and the Deputy Master¹ has invited me to say that he hopes it will suit you to examine it on Monday next² as he and many of the Elder Brethren will be here on that day. I have written Wilkins to arrange with you accordingly.

The Deputy Master also desires me to say that he and a Committee intend going to Dover by the 1.30 Train on Wednesday next the 20th Inst. to inspect the Magneto Electric Light & [to] this he hopes you will accompany them.

Believe me | My dear Sir | Yours very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. Robert Gordon.
2. That is 18 April 1859.

Letter 3586

Thomas Henry Farrer¹ to Faraday

18 April 1859²

From the original press copy in TNA MT4/65, p. 1775

18 Apr 1858 [sic]

Sir,

I am directed by the Lords of the Committee of Privy Council for Trade, to acquaint you that Her Majesty's Paymaster General has been authorized to pay to you the sum of One hundred Pounds (£100) for your services in connection with the examination of Lighting apparatus for Colonial Lighthouses in accordance with the arrangement specified in my letter of the 29th February 1856³.

Enclosed is a form of receipt which upon being stamped and signed by you can be presented for payment at the Office of Her Majesty's Paymaster General through a Banker.

I am | Sir | Your obedient Servant | [no signature]
Professor Faraday | F.R.S. | &c &c &c | Royal Institution | Albemarle Street | W

1. Writer identified on the basis of the reference to letter 3096.
2. Dated on the basis of the location of the letter in the manuscript.
3. Letter 3096.

Letter 3587

Faraday to Peter Henry Berthon

19 April 1859

From the original copy in GL MS 30108/3/94

Royal Institution | 19 Apr 1859

Dear Sir,

I examined the Apparatus for the North Foreland lighthouse yesterday¹ and found it very excellent. The glass is very good and its colour though of the French blue-green tint, is very small, especially in the thinner & principal

parts of the apparatus. The pieces are well worked & well associated together i.e. optically:— for as regards the fittings of the central ribs into the sides of the metal frame these sides, as was observed by Captn Close & others, thicker than they need be, having regard to due strength; & they cause obstruction of light.

The two large back reflectors are imperfect in form & throw the light very irregularly back towards the lamp flame:— This is perhaps inevitable considering the *size* of the reflectors & the *mode* of their manufacture still such light will be sent seaward, & will aid, irregularly, in strengthening the visible light at sea_[.]

The very imperfect action of the lower reflecting ribs, because of the obstruction offered by the size of the burner, when the flame is not more than $2\frac{1}{2}$ or $3\frac{1}{2}$ inches high; presses itself into notice in these examinations. If the flame could with a good combusting be made higher & not so much compressed in width in the upper part it would be a great advantage to lighthouses placed on a high level; for all the extra light which would then reach these lower ribs would be thrown on to the sea between the horizon & the shore; very important parts to be strengthened in light in hazy weather²_[.]

I am | Dear Sir | Your Very Obedient faithful Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c | Trinity House

1. See letter 3585.

2. This letter was read to Trinity House By Board, 26 April 1859, GL MS 30010/42, p. 201. It was referred to the joint Wardens and Lights Committee. See Trinity House Wardens Committee, 18 May 1859, GL MS 30025/27, p. 17 where it was agreed to seek to rectify the problem.

Letter 3588

Faraday to Christian Friedrich Schoenbein

25 April 1859

From the original in UB MS NS 448

Royal Institution, London | 25 Apr 1859

My dear Schoenbein,

I have just seen your letter to Miss Hornblower, and so write knowing you will be at home again. I am glad you went out, for though all things, would be distasteful to you still they work out the transition back again from sudden & deep grief to a more collected, healthy, & necessary state of mind.— For the same reason I am very glad that Mrs. Schoenbein has left home for a little while, & trust that it may calm her spirits and do her good. It is impossible for me to write to you, or do any thing connected with you without thoughts of your dear daughter entering in. I have a volume of my collected experimental papers on physical & chemical matters¹ to send you by the first opportunity. It was ready when Miss Schoenbein's box was sent to you from Stamford Hill, but I felt as if I could not intrude the book into so sacred a deposit as that box

was, and so retained it for some fitter opportunity_[.] I gave a Friday Evening on Ozone & Antozone², for which only a few weeks before I had given tickets at her request to some friends of hers³; but I could not, & cannot, talk to you about it. I did my best;— though with thoughts often pressing in:— still let me thank you for what you had, before the sad event, done to help me.

Your letter to Miss Hornblower spoke of a cypress tree; & I went yesterday to see the state of such trees as are on the ground & how they are likely to grow.— Those that are up do not look well; but if Miss Hornblower will let me, I shall do what I can to plant such a tree on the spot. At present I cannot see her on the matter, for you will be sorry to hear that we are in trouble and anxious on her account. She fell some 4 or 5 years ago & hurt her knee; it has never ceased to be painful, & from falls since has become worse, & at last an operation was decided upon. This was performed the day before yesterday under the influence of chloroform and by a very clever surgeon. He removed part of the bone which had become injured & unhealthy, & we hope for good results; but time & patience will be required. The accounts last night were favourable, but the time since the operation is as yet too short to allow of any thing beyond a hasty and imperfect judgment.

I sent your letter to Grove. He has been suffering from a sharp attack of rheumatic gout, which confined him to the house, but he is now getting better.— All your friends think of you and feel for you.

For a little on the other side, I may say that we are pretty well. My wife joins me in kindest remembrances and thoughts; & so to does my niece Jannie:— for though she is not much known to you yet she was to Miss Schoenbein.— Extend these sympathizing thoughts to the children who remain to comfort you_[.]

Ever My | dear Schoenbein Yours | M. Faraday

1. Faraday (1859b).

2. Faraday (1859a), Friday Evening Discourse of 25 February 1859.

3. See letters 3563 and 3564.

Letter 3589

Faraday to Benjamin Collins Brodie

29 April 1859

From the original in Duke University Medical Library

Green Hampton Court SW | 29 Apr 1859

My dear Sir Benjamin,

What between my ignorance of the German language and my bad memory I really do not feel in a condition to suggest to you names for the Medals¹. I should run the chance of wronging those whose papers have not come to my knowledge. You must apply to those who are more in the Society of Science and who (unlike me) are able to keep possession of their information

when they have obtained it[.] Although I am obliged to say this I wish most heartily I could help you for nobody would be more advantaged by such ability than myself[.]

Ever Yours very Truly | M. Faraday
Sir Benj Brodie Bart | &c &c &c

1. Of the Royal Society.

Letter 3590

Faraday report to Trinity House

29 April 1859

From *Parliamentary Papers*, 1861 [2793], XXV, volume 1, pp. 2–4

Report on the Magneto-Electric Light established by Professor Holmes at the High Lighthouse, South Foreland.

Royal Institution | 29 Apr 1859

The light applied in the South Foreland Upper Lighthouse is an electric light; not produced, however, by a voltaic battery, but by magneto-electric induction. In the year 1831* it was discovered that when a piece of soft iron, surrounded by a metallic wire, was passed by the poles of a magnet, an electric current was produced in the wire, which could be exalted so as to give a spark. The apparatus of Professor Holmes, which is figured and described in the accompanying paper A¹, consists of an accumulation of powerful magnets and iron cores with surrounding coils, accurately arranged, so that when the associated cores are revolving they send all their currents into one common channel, from whence they are conveyed to the lantern by conducting wires, and there produce the electric light. There is no consumption of material or energy, other than that of the burning fuel required at the steam engines to produce motion.

A trial of the light began in the lighthouse, on the 8th December, 1858; but as the apparatus was imperfect in some points and the results unsatisfactory, the lighting by the apparatus was suspended for a while, that the defects might, if possible, be remedied. The lighting was renewed on the 28th March instant, and has been continued regularly since.

I have had the honour of accompanying the deputy master² and brethren, both on the former and present occasion, so that I know personally what the light *was* and *is*. No report was made at the former time, because of the expected improvement of the arrangement; but it now becomes my duty to report on such matters regarding the lamp as properly fall within my province.

Being on board the yacht off Dungeness on the night of the 20th inst., about 21 $\frac{1}{2}$ miles from the South Foreland, the weather being rainy and the sky

covered with clouds, we could see (when the sun went down) the high light illuminated and appearing as a faint star. I could not perceive the low light; but proceeding eastward the low light gradually became visible, and the high light increased in brightness. When about S.W. by S. the yacht approached the lights more directly; then went eastward again, and after awhile turned and proceeded towards Dover; so that the observations were made through about one-fourth of the horizon, and at very different distances from the South Foreland. From the time the upper light was first seen until the last, it remained visible and *steady* (with the exceptions to be mentioned immediately), and much superior to the lower light when that also came in view. The exceptions were as follows:— At times the high light fell off, and once appeared almost out, when the lower light underwent no such change. This effect had been anticipated and was due to the following cause:— the upright bars of the lanthorn windows and the dioptric apparatus are much broader than the electric light, the latter being, indeed, not more than one-sixth of an inch in horizontal width; hence they throw deep and rather sharp shadows. By taking the bearings of these beforehand, it was found at sea, that the falling off of the light *coincided* with these shadows, and hence the above exceptions. The evil occurs in part with the central oil lamp, and is in some lighthouses partly remedied by causing the window bars and astragals to incline from the perpendicular, and therefore out of the plane of refraction. Other remedies (in addition to this) are applicable in the case of the electric light, and in the present instance the effect is lessened by the use of a small reflector at the lamp, close to and behind the light. The light was at all times white, or even blue occasionally, in comparison with the low light, which appeared yellow or reddish.

The next day I examined the lighthouse and apparatus both by day and night. The magneto-electric machines, steam engines, and steam-condenser were generally as at the last visit. In respect of the *commutator* it had worn very little; the application of a file to the surface of one wheel had removed about the thirteenth of an inch of metal since the apparatus was first erected, there remaining about $1\frac{1}{2}$ inches still ready for consumption in like manner, if needed. During the day time I compared the intensity of the light with that of the sun; i.e. it was placed before and by the side of the sun, and both looked at through dark glasses; its light was as bright as that of the sun, but the sun was not at its brightest, and was only seen at intervals between clouds.

In the lanthorn, there was now but one electric lamp in place, two others, however, being on the wire rails ready for change of lamp at any moment. The magneto-electric machines were set in action, and the lamps were manipulated with, both by day and night, to my entire satisfaction. When the steam engines were ready for action the machines could be set in motion, and the current evolved within the space of half a minute; the lamp could then be lighted in an instant, and if it were required to put out and displace that

lamp and replace it by another, the operation could easily be performed by one person within 10 or 15 seconds. The light may be considered as at its full intensity at once, though it was reported to me as growing up in power until about four o'clock in the morning, an effect probably due to the continued recurring inductive action in the cores and coils of the revolving apparatus.

The place of the light in respect of the dioptric apparatus is exceedingly well retained, even more so than in the case of a lamp flame, which, though its base be fixed, varies in its height. The light itself is not that flickering, wavering, revolving light produced by the voltaic battery, but, in a sheltered atmosphere, as in the lanthorn, is fixed in its position; a fact of great importance in the application of small catoptric or dioptric apparatus. An important regulator of the character of the light is given by two magneto-electric coils introduced into the circuit in the lanthorn. The light does not call for continued attention, but is often left untouched for one, two, or even three hours together. The eyes of the keepers are not affected, though the blue glasses provided for them are very pale in colour, for the light is better watched by observing the *place* and *intensity* of the rays which fall here and there on the walls of the lanthorn, than by looking at the light itself.

On going out to the hills round the lighthouse, the beauty of the light was wonderful. At a mile off, the apparent streams of light issuing from the lanthorn were twice as long as those from the lower lighthouse, and apparently three or four times as bright. The horizontal plane in which they chiefly took their way made all above or below it black. The tops of the hills, the churches and the houses illuminated by it, were striking in their effect upon the eye.

All the reports which have come in from the surrounding lighthouses, floating lights, and pilot vessels, confirm the superiority of the upper light above the lower, though many of the reports are imperfect. Those from Dungeness are the best; and include 160 observations made between March 28th and April 16th. The upper and the lower South Foreland and the Grisnez lights were either visible or invisible from the station, according to the weather. The upper South Foreland was visible first and oftenest i.e. *always* if the others were; then the Grisnez came oftenest in sight; and then the Lower South Foreland; but these were nearly equal. The Grisnez was sometimes in sight without the lower South Foreland, but never without the upper; and the lower South Foreland was sometimes in sight without the Grisnez. Upon the 160 observations there were 73 on which none of the three lights were visible; 86 upon which the upper light was visible; 40 upon which the lower light was visible, and 44 upon which the Grisnez Light was visible. When both the upper and lower lights were visible, the upper (except in four cases) is said to be twice the power of the lower; in the four cases it is called equal. There was one exception on April 3rd at 10 o'clock p. m., when the upper light is stated to have *gone out*. The report does not say whether it came in again, but

the weather at 11 o'clock is returned as very thick and misty. Generally the light is said to be very steady.

The keepers at the Goodwin and South Sand head floating lights appear to have had a fair view of both upper and lower lights. If both were visible, the upper was much the most powerful; the upper was always visible. The lights were generally very steady; if wavering, both the upper and lower wavered at the same time, as if the cause were in the air, not in the lights. The reports from the Gull Stream Light are very poor and afford no instruction. There is no comparison or mention of the upper and lower lights.

In fulfilment of this part of my duty I beg to state that, in my opinion, Professor Holmes has practically established the fitness and sufficiency of the magneto-electric light for lighthouse purposes, so far as its nature and management are concerned. The light produced is powerful beyond any other that I have yet seen so applied, and in principle may be accumulated to any degree; its regularity in the lanthorn is great, its management easy; and its care there may be confided to attentive keepers of the ordinary degree of intellect and knowledge.

There are many considerations, beyond this establishment of the fitness of the light in principle and management for lighthouse purposes, regarding its introduction into lighthouses generally, on which I should hesitate to speak before those who are far more competent to judge of these matters than I am, were it not for the encouragement which the Brethren of the Trinity House give me, and especially as regards *this light* in respect of a letter from Professor Holmes, dated 28th April 1857, which I considered in my letter of the 1st of May³. I will therefore venture to enumerate some points which are *against* and others in *favour* of the light, and of a change in the present system. In the first place, the simplicity of the present system is very great compared with that of the electric light. Only two keepers are required to a lighthouse, they need possess no special knowledge, ordinary attention is all that is necessary; and thus failures of the light are almost impossible. In the new system a second set of men will be required to attend the engines; and there must be amongst them one or more who understand the principle and construction of the lamp in the lanthorn, of the magneto-electric machines, the steam engines, and the condensers below, and be able to make *effectively* the repairs necessary to the apparatus; or, as I think is more probable, a competent resident intelligent engineer, with his stock of tools and means, will be required.

In the next place, the expense of the new system must be large, compared to that of the present system. As *outfit*, there must be the cost of the two if not three magneto-electric machines, with the corresponding steam engines; of the houses to contain them; of the cisterns, condensers, and water apparatus, and supply associated with them; of the electric lamps in the lighthouses, with their connexions; and of the lodging houses for the extra men. And in respect of *current expenditure*, additional provision must be made for the wages of the extra men, with coals and such things as are allowed them; the fuel for the

engines; the supply of water; and the repairs required by the wear and tear of all the apparatus. To these causes of expense must be added the claims of the Patentee. In the matter of expense for any given amount of light, I may say that the letters I have already referred to of the 28th April and 1st May 1857, can scarcely be accepted as giving sufficient information. I conclude that by this time Professor Holmes is in a position to give an amended statement.

Amongst partial objections it may be stated that the light cannot be expected to apply to *all* lighthouses, or receive general adoption. I am not aware how many it might be fit for; but there are evidently some at extreme situations where it would be objectionable in its present state. An objection has also been made, of which I cannot see the force; namely, that the light is too bright; that it gives a false impression of the distance of the lighthouse, and that it blinds the eyes of the mariners to the perception of the lights on board vessels between it and them. These objections, if they have any force, must be judged by mariners themselves.

The points *in favour* of the magneto-electric light, now that its practicability has been established, are strong and clear in relation to the increase of light and the advancement of lighthouses in power. In cases where the light is from lamp flames fed by oil, no increase of light at or near the focus or foci of the apparatus is possible, beyond a certain degree, because of the size of the flames; but in the electric lamp any amount of light may be accumulated at the focus and sent abroad, at, of course, an increased expense. In consequence of the evolution of the light in so limited a focal space, it may be directed seaward, diverging either more or less, or in a vertical or horizontal direction, at pleasure, with the utmost facility. The enormous shadow under the light produced by the oil flame burner, which absorbs and renders useless the descending rays to a very large extent, does not occur in the magneto-electric lamp; all the light proceeding in that direction is turned to account. The optical part of the arrangement, whether dioptric or reflecting, might be very small in comparison with those in use. It is probable that the system either of reflectors or refractors need not be above 18 inches in diameter, perhaps much less, and probably a foot high. The glass of the lanthorn through which the rays would pass would not then require to be above 12 inches radius, i.e. from the light. This would allow that part of the lantern to be constructed of carved plates of glass, framed above and below, and requiring no opaque, intervening, shadow-making uprights. There appears no reason why such a lanthorn should not enclose the electric lamp round seven-eighths of the horizon, and yet allow of all necessary attendance on, and change of the lamps. Such arrangements, however, could only be made when the lamp is (if ever) perfectly established; for they would preclude the substitution of an oil lamp for the electric lamp, if any accident occurred to the latter.

Before concluding this report, I must bear my testimony to the perfect openness, candour, and honour of Professor Holmes. He has answered every

question; concealed no weak point; explained every applied principle; given every reason for a change either in this or that direction, during several periods of close questioning, in a manner that was very agreeable to one whose duty it was to search for real faults or possible objections, in respect both of the present time and the future⁴.

(signed) M. Faraday

*Philosophical Transactions 1832, p. 131, Faraday⁵

1. Not found.

2. Robert Gordon.

3. Letter 3279.

4. This letter was mentioned at Trinity House Court, 3 May 1859, GL MS 30004/27, p. 314 and ordered to lie on the table. At Trinity House Court, 5 July 1859, GL MS 30004/27, p. 336 it ordered that a copy be sent to the Board of Trade.

5. Faraday (1832), ERE1, p. 131.

Letter 3591

Faraday to Henry Enfield Roscoe

2 May 1859

From the original in Houghton Library, Harvard University, FALF

[Royal Institution embossed letterhead], Albemarle St. W |

2 May 1859

My dear Sir,

I am very sorry I was out of town when you called. But you left pleasant remembrances in the paper on light chemical action¹ for which I thank you very much. You must not suppose however that I have not read it long ago & with great interest[.]

Ever Truly Yours | M. Faraday

H.E. Roscoe Esqr | &c &c &c

1. Bunsen and Roscoe (1857a, b, c).

Letter 3592

Thomas Henry Farrer to Faraday

9 May 1859

From the original press copy in TNA MT4/55, p. 2060

4822 | 9 May 1859

Sir,

to request your assistance under the following circumstances.

The Officers of the Board of Trade are charged with the duty of surveying Passenger Steamers periodically. If they report the Hull & Machinery of a Vessel to be safe the Board of Trade issue a Certificate or Licence which enables

her to ply. If they do not so report the License is withheld and the Vessel is prohibited from plying with Passengers.

A case has recently arisen in which a River Steamer –: “Waterman 3” – has been fitted with a new form of Apparatus for superheating steam. The Engineer Surveyors of Steam Vessels for the Port of London have forwarded a report thereon, a copy of which is inclosed, together with a sketch of the invention¹.

From this report it will be seen doubts are entertained whether the apparatus is free from danger. The apparatus it is said gives the means of saving a great deal of fuel and whilst it is of course of paramount importance that the safety of the public should be duly cared for, it is also very important not unnecessarily to discourage an invention which promised valuable results in the way of economy

The Board of Trade are under these circumstances desirous before they decide whether to grant or withhold a certificate of having the best scientific opinion they can procure on the question whether the apparatus in question is or is not such as is likely to cause danger to passengers, and they therefore direct me to apply to you in the hope that it may be in your power to inquire into the matter and give them the benefit of your advice.

A copy of a report from Mr. Atherton² (Chief Engineer at Woolwich Dock yard) is inclosed for your information.

My Lords are making arrangements for the inspection of the Osprey and will write to you again as to time and place

I have the honor to be | Sir | Your obedient Servant | T.H. Farrer
Professor Faraday F.R.S. | &c &c &c

1. A press copy of this report follows this letter.

2. Charles Atherton (1805–1875, B1). Chief engineer at Woolwich dockyard, 1851–1862.

Letter 3593

Faraday to Peter Henry Berthon

11 May 1859

From the original copy in GL MS 30108/3/95

Royal Institution | 11 May 1859

Sir,

I have examined the *Tunis* apparatus – I find it quite satisfactory. The glass is good in colour and well wrought & fixed_[.] The optical & frame work both satisfactory. The position of the lamp required a correction which is easily made & I ordered it to be done¹_[.]

I am Sir | Your Very Obedient humble Servant | M. Faraday
P.H. Berthon Esqr | &c &c &c

1. This letter was read to the Trinity House By Board, 17 May 1859, GL MS 30010/42, p. 226.

Letter 3594**Angela Georgina Burdett Coutts to Faraday****11 May 1859****From the original in IET MS SC 2**

Torquay | 11 May 59

Dear Mr. Faraday,

I am most obliged by the Ozonometer which quite answers for all I want but I cannot detect the least trace of any Ozone here – We have been so sorry to hear of Mr. Barlows Indisposition and sincerely hope he is now convalescent. We are neither of us quite well & Mrs. Brown who says to be kindly remembered, very far from well and in consequence our leaving this place is delayed. As we have some visits to pay en route to London when we shd move I am very much afraid I shall not be present at my friend Mr. Pengelly's¹ Lecture on the 27th² which I much regret, for I take much interest in the subject and still more in himself his history, (I mean that of his mind) is so extremely interesting, and he is one of those Philosophical & refreshing instances of perseverance under difficulty in the acquirement of knowledge, *from a love* of the thing itself & not as a means of getting on in life which is so Insistently put forward by Educationalists. I am really inflicting a long letter instead of a simple note of thanks so conclude only adding Mrs. Browns remembrances

Very Sincerely | A.G. Coutts

Would you say to Mrs. Faraday with my kind remembrances that there is I hear a very nice Opera at Drury Lane³ and if she would like my Box *any* night next week would she let me [know] and I will send an order.

1. William Pengelly (1812–1894, ODNB). Devon geologist.

2. Pengelly, W. (1859), Friday Evening Discourse of 27 May 1859.

3. This was 'Il Trovatore' by Verdi. *The Times*, 17 May 1859, p. 6, col. b.

Letter 3595**Faraday to Angela Georgina Burdett Coutts****13 May 1859****From the original in BL Burdett-Coutts papers**[Royal Institution embossed letterhead],
Albemarle St. W | 13 May 1859

Dear Miss Coutts,

The end of your letter¹ is truly the beginning of mine[.] My wife thanks you most sincerely for your remembrance of her and my niece would thank you if she were here, if not in words yet in thoughts:– for I purpose to accept your kindness in offering us your box at Drury Lane if it is convenient to you *next Tuesday Evening*². We had it once before I remember for a pantomime³ which is always interesting to me because of the minimum concentration

of means which it requires. You must not expect to find Ozone *in the house* or amongst close inhabited houses[.] I used to obtain it by the very same ozonometer *outside* of windows on the Kings Road Brighton when the wind was from the sea – not when from the land⁴.

My kindest remembrances to Mrs. Brown – If my wishes could make her strong she would be strong indeed. With all respect I am most truly

Your obliged Servant | M. Faraday

Address: Miss Coutts | Torquay | Devonshire

1. Letter 3594.

2. That is 17 May 1859 when he would have heard Verdi's 'Il Trovatore'. *The Times*, 17 May 1859, p. 6, col. b.

3. See letter 3219.

4. See Faraday to Schoenbein, 13 December 1850, letter 2356, volume 4.

Letter 3596

Thomas Henry Farrer to Faraday

14 May 1859

From the original press copy in TNA MT4/55, p. 2152

14 May 1859

5219

Sir,

With reference to my letter of the <9th> Instant¹, I am directed by the Lords of the Committee of Privy Council for Trade to state that the Steamer "Waterman 3" will be ready for inspection any day after Wednesday the 18th Instant.

I am therefore to request that you will have the goodness to name some day and hour after Wednesday when it will be convenient for you to make the inspection, in order that directions may be given to an Officer of this Board to accompany you and that notice may be sent to the Company – I also enclose for your information the accompanying copies of reports made by Professor Taylor² & Professor Brand[e] on the subject of Messrs. Parsons³ and Pilgrim⁴ Apparatus for Superheating Steam which have been forwarded to this Board by the Secretary to the Waterman Steam Packet company – I have the honor to be | Sir | Your Obedient Servant | T.H. Farrer
Professor Faraday F.R.S. | &c &c &c

1. Letter 3592.

2. Alfred Swaine Taylor (1806–1880, ODNB). Medical jurist.

3. George Parson of 5 Duke Street, Adelphi, POD. Patentee (number 1858-422) of a superheated steam apparatus.
4. Thomas Pilgrim (1800-1871, B2). Marine engineer who patented (number 1858-411) a superheated steam apparatus.

Letter 3597**Faraday to Angela Georgina Burdett Coutts****18 May 1859****From the original in BL Burdett-Coutts papers**

[Royal Institution embossed letterhead], London,
Albemarle St. W | 18 May 1859

Dear Miss Coutts,

Let me thank you for a very pleasant evening last night¹. My wife could not go but I took two nieces & a nephew & we enjoyed the performance very much.

I suppose you treat Schoenbeins test rightly. Stick a piece up by a pin, outside the window so that it is free in the air – leave it all night – next morning it may shew nothing particular but damp the tip by a touch of clean water (do not dip it into water) and then if there be any ozone action the colour will change to blue faint or stronger according to the action of the ozone[.]

Ever Your faithful Servant | M. Faraday

Address: Miss Coutts | Torquay

1. See letter 3595.

Letter 3598**Thomas Henry Farrer¹ to Faraday****24 May 1859****From the original press copy in TNA MT4/55, p. 2283**

24 May

5381.

19th Instant containing your report on the method for Superheating Steam as adopted for the Steam Vessel "Waterman 3"²: and I am to express to you their Lordships thanks for having so kindly assisted them with your advice on this subject, which it is their intention at once to act upon.

I have the honor to be | Sir | Your obedient Servant | [No signature]
Professor Faraday F.R.S. | &c &c &c | Royal Institution

1. Writer identified on the basis that this follows letter 3596 in sequence.
2. Not found.

Letter 3599**Jacob Bell¹ to Faraday****2 June 1859****From the original in RI MS**

15 Langham Place | 2 Jun 1859

My dear Dr. Faraday,

I have the works of John Gould² F.R.S. from the commencement up to the present time³. Understanding that the Library of the Royal Institution is not provided with these works I am desirous of offering them as a donation.—

The volumes up to a certain period are bound, and I regret my inability to included [sic] the entire works in the same state, but I find on enquiry that three of the works are still in progress & therefore not yet in a condition for binding.—

As the state of my health gives me no prospect of living to see the works completed⁴, and I could not impose on my Executors an indefinite trouble and responsibility, I have only the alternative of requesting you to accept on behalf of the Institution all that it is in my power to give.

I remain | My dear Dr. Faraday | Yours very Sincerely | Jacob Bell
Professor Faraday D.CL. FRS. &c

1. Jacob Bell (1810–1859, ODNB). Pharmacist.

2. John Gould (1804–1881, ODNB). Ornithologist.

3. Gould (1834, 1836, 1837, 1838, 1845–58, 1848, 1849–58, 1850, 1850–9, 1851–5, 1854, 1858). These are the references as given in RI MM, 6 June 1859, 11: 286–7 which notes acceptance of the gift. See Sauer (1982), 13–86 for a detailed bibliography of Gould's works.

4. Bell died on 12 June 1859.

Letter 3600**Faraday to Mary Elizabeth Lyell¹****4 June 1859****From the original in RI MS F1 N/1/31**

[Royal Institution embossed letterhead] | 4 Jun 1859

Dear Lady Lyell,

Though I did not appear in person to thank you last night I cannot omit to do so altogether. The fact is that I was afraid to come for though Babbage² very kindly pressed me (& *you* had asked us) others stood about who knew my rule & would have taken advantage of its relaxation. My wife could not come. She & my niece thank you much for your remembrance of them[.]

Ever Your faithful Servant | M. Faraday

1. Mary Elizabeth Lyell, née Horner (1808–1873, Burkhardt *et al.* (1985–2005), 4: 652). Conchologist. Married Charles Lyell on 12 July 1832.
2. Charles Babbage (1791–1871, ODNB). Mainly worked on inventing mechanical calculating machines.

Letter 3601**Angela Georgina Burdett Coutts to Faraday****15 June 1859****From the original in IET MS SC 2**

Stratton Street | 15 Jun 1859

Dear Mr. Faraday,

I am anxious to speak to you on a little matter of business. Could you look in upon us this evening. We are alone & quite at liberty by $\frac{1}{2}$ eight or I would be at Home any time any time [sic] tomorrow or call upon you.

Yours Sincerely | A.G. Coutts

Letter 3602**Charles Lock Eastlake to Faraday****17 June 1859****From the original copy in NG MS**

National Gallery | 17 Jun 1859

My dear Sir,

I am requested by the Trustees of the National Gallery to ask you whether, in the course of your inquiries respecting gas used for lighting¹, those inquiries have been extended to the effects of such gas on pictures and on gilt frames. Should you have come to any conclusion on that subject I have to request that you will kindly furnish me, for the information of the Trustees, with the result of your observations.

It has been proposed that the Vernon² and Turner³ pictures – which are to be removed in the course of the next two or three months from Marlborough House to some galleries annexed to the Museum at South Kensington – should be exhibited there not only during the day but by gas light, for the benefit and enjoyment of those whose occupations may prevent them from seeing such works by day⁴.

This object must in itself be regarded as most desirable; The chief question for the Trustees to consider in connexion with it is, whether the lighting with gas would be in any way prejudicial to the pictures. The effects of gas on gilt frames, though not to be overlooked, would be of less importance, and as regards any doubt arising from conditions of the bequests, such doubt would,

I understand, resolve itself into the main question respecting the safety of the pictures.

It would be desirable to ascertain, as far as possible, the effects of gas:

1. On oil, as used in painting.
2. On glutinous substances, such as size &c.
3. On resins dissolved in essential oils and used as varnishes.
4. On mineral and other substances from which colours are prepared.
5. On recently painted pictures as compared with pictures long since executed.

Lastly it would be important to know whether any of the contrivances which profess to remove or to neutralize the deleterious effects of gas can be considered altogether efficacious.

I am, My dear Sir | Yours faithfully | C.L.E

Professor Faraday F.R.S. | &c &c &c | Royal Institution | Albemarle St.

1. Undertaken by Faraday as a member of the commission established by the Department of Science and Art at South Kensington on 6 June 1859 (TNA ED28/10, p. 54) to consider gas lighting in galleries. The report of the commission, dated 20 July 1859, is in *Parliamentary Papers*, 1859, 2nd session (106), XV.

2. Robert Vernon (c.1774–1849, ODNB). Art collector many of whose pictures came into the possession of the National Gallery.

3. Joseph Mallord William Turner (1755–1851, ODNB). English painter.

4. See the papers relating to the move of the Turner and Vernon galleries in *Parliamentary Papers*, 1859, 2nd session (233), XV.

Letter 3603

Faraday to Charles Lock Eastlake

18 June 1859

From the original in NG MS

[Royal Institution embossed letterhead],
Albemarle St. W | 18 Jun 1859

My dear Sir Charles,

I am on a Committee appointed to consider the lighting of picture galleries by gas¹ – and I think I had better not give any answer to your enquiries² until this Committee has met – which it will do next week[.]

Ever faithfully Yours | M. Faraday

Sir Charles Eastlake | P.R.A. | &c &c &c

1. See note 1, letter 3602.

2. In letter 3602.

Letter 3604**Faraday to Henry Wentworth Acland¹****20 June 1859****From the original in Manchester Public Library MS Griffith 29**[Royal Institution embossed letterhead],
Albemarle St. W | 20 Jun 1859

My dear Dr. Acland,

I have received your letter – also your book – and seen your friend – I thank you humbly for the first & second – I have lent a Galvanometer of my own to your friend Mr. Griffiths² & if it does not serve his purpose altogether I hope it will enable him to judge of what kind would be most useful[.]

Ever Yours | M. Faraday

1. Henry Wentworth Acland (1815–1900, ODNB). Regius Professor of Medicine at the University of Oxford, 1857–1894.

2. Probably John Griffiths (1806–1885, ODNB). Keeper of the archives of the University of Oxford, 1857–1885.

Letter 3605**Peter Henry Berthon to Faraday****21 June 1859****From the original in GL MS 30108/3/96**

Trinity House | 21 Jun 1859

My dear Sir,

I enclose you letters respecting a new Electric Light and am to say that the Deputy Master¹ & some of the Brethren intend going *tomorrow Evening* at 9 o'clock to Welbeck Street² for the purpose of seeing it in operation, and would be glad if you could conveniently meet them there, that they may have the benefit of your experience in these matters.

Yours most truly | P.H. Berthon

M. Faraday Esq | &c &c

Please return the enclosed

1. Robert Gordon.

2. The address of John Thomas Way. See letter 3608.

Letter 3606**James South to Faraday****22 June 1859****From the original in IET MS SC 2**

Obsy Kensington | Wednesday, 22 Jun 1859

My dear Faraday,

A Neighbour of mine¹ had during the storm of *Sunday*² an Acacia Tree of large size, struck by Lightning – The trunk of the tree was split, and the Exposed Surfaces Singed, & the stem most neatly decorticated.

The tree, or rather the shattered part of it has unfortunately been cut into lengths of 4 or 5 feet, otherwise it might have given a fine opportunity for getting a photographic sketch, for it must have been a fine specimen before it was mutilated, by the Saw.

I scarcely think it will repay you for coming purposely so far to see it, but if you think otherwise, I shall have great pleasure in accompanying you to it at any time you please – it is not more than a minutes' walk from my house – I called yesterday at the Institution to tell you this but was not fortunate in finding you –

With kind regards to Mrs. Faraday | Yours Ever sincerely | J. South

PS/I purposed calling in Albemarle Street to day but find myself really too unwell to leave my home

1. William Banting (c.1796–1878, ODNB). Undertaker of St James's Street.

2. That is 12 June 1859. See letter 3607 and *The Times*, 14 June 1859, p. 11, col. f.

Letter 3607**William Banting¹ to Faraday****after 22 June 1859²****From the original in IET MS SC 2**

4 The Terrace, Kensington

Dear Sir,

I am delighted to have interested you sufficiently to honour me with a visit and that was not an unprofitable waste of your valuable time.– I take much interest in natures great works or indeed any of her works for all are great but this instant destruction of my Accacia Tree which I was privileged to witness seemed to me most wonderful – I am very glad to be able to offer you a particular specimen which you did not possess and as your observation imparted an increased interest have had the parts photographed of which I have the greatest pleasure in offering you 3 copies with the parts of the tree you desired to possess. Also my own and my neighbours impression on

witnessing this great work of nature in case it may be useful[.] The Photograph of the tree standing after it was struck was not taken whilst it was standing but you know the pains taken to obtain its relative position in reference to the surrounding trees and its altitude therefore it is a copy of a Photograph with its introduction by hand photographed and is really true and perfect as I could desire. I regret not having the tree photographed before it was cut down but have done my best to mend the omission – I have sent a copy to Sir James South and Sir Willm. Hooker³ who will have one piece of the Tree at Kew and if you would like 2 or 3 more copies you are most heartily welcome – I cannot hope ever to hear your lecture or opinion upon it but should be very glad to know if you think it was so riven by the power of steam so instantaneously generated – this alone seems astounding & inexplicable to me and yet I cannot imagine any other cause[.]

I have the honour to be | Sir | Your obedient Servant | Wm. Banting
Mr. Faraday Esqr

Account of an Accacia struck by Lightning in the garden of Mr. Banting at Kensington at 1.30. P.M. on Sunday 12th June 1859.

For a few seconds there was a perfect lull in the Storm which came from the east and reached Kensington about 1 o'clock[.] The Atmosphere was dull heavy and peculiar giving me an idea of what I had read about on the approach of a Tornado Typhoon or other great natural convulsion which rather alarmed me though not generally nervous about Storms.– An exceedingly vivid and brilliant double flash of lightning accompanied by a crashing peal of Thunder and a crushing sound like falling Trees and very heavy rain quite alarmed me particularly as the Atmosphere was still further darkened by falling black masses which I found were pieces of bark[.] My Accacia Tree was shivered and its expansion had blown the bark to a distance of nearly 100 feet in every direction – such parts as were not so thrown off being in ribbons when the wood collapsed – This Tree one of the largest of its kind in this locality was in full perfect vigour stood nearly 50 feet high and branched out from a sort of fork 32 feet above the ground at which point the electric fluid seemed to have entered it passed down the center ripped it open charred the riven surfaces from the center to the outside and left the Tree at about 12 feet above the ground in a South easterly direction without any further trace of its progress in the Neighbourhood[.] The Tree measured 20 inches diameter at 3 feet above the ground but the trunk was not injured below that part when the electric fluid passed out at a part where a heavy or large branch had been formerly cut off, yet, little moisture was left in any part and none whatever where the electric fluid had passed through the body. The tips of the branches of most of the surrounding trees seemed scorched – A gooseberry Bush 150 feet from the Accacia with the fruit upon

it was scorched or withered to the extent of two thirds of its entirety – An espalier cherry tree in another direction was entirely withered, all tending to show “as I fancy” how greatly the Atmosphere was charged with electricity and a marvellous work of nature beautifully but fearfully accomplished in one instant[.]

Willm. Banting | 4 The Terrace | Kensington

See Photograph of Tree after it was struck[.]
 Photograph of all the parts when cut down
 Letter from my Neighbour in corroboration –

1. William Banting (c.1796–1878, ODNB). Undertaker of St James’s Street.
2. Dated on the basis that Faraday’s visit occurred after he had been told about the lightning strike by South in letter 3606.
3. William Jackson Hooker (1785–1865, ODNB). Botanist and Director of the Royal Botanic Gardens, Kew, 1841–1865.

Letter 3608

Peter Henry Berthon to Faraday

23 June 1859

From the original in GL MS 30108/3/96

Trinity House | 23 Jun 1859

My dear Sir,

The Deputy Master¹ desires me to say that he received your note last night, and that he regrets you were not with him, as he considers there is more than “something” in the Light patented by Professor Way², he wishes you would take an opportunity of visiting Welbeck Street, and seeing it in operation, and favor the Board with your opinion on it.

Believe me | My dear Sir | Yours very truly | P.H. Berthon
 M. Faraday Esq

1. Robert Gordon.
2. Patent number 1857-2841 entitled ‘Obtaining Light by Electricity’, dated 11 November 1857.

Letter 3609

Faraday report to Trinity House

27 June 1859

From the original copy in GL MS 30108/3/96

Royal Institution | 27 Jun 1859

Report on Professor Way’s electric lamp

The electric light of Mr. Way has its peculiarity in the nature of the lamp. The electricity is obtained from a Voltaic battery and is so far subject to the same remarks as those which arose respecting Dr. Watsons¹ proposition &

which need not be repeated here². I will therefore confine my observations to the lamp alone. I have received no written statement of the advantages of the lamp – of the amount of its lighting power – of the price of the light – of its proportion compared to any known standard – or of its proportion compared with that produced by an equal amount of electricity passing between carbon poles in the ordinary way nor any experimental data from which these points can be ascertained. I learn only by verbal communication with Dr. Way that the place of the light is unchangeable and that it has yet to be studied in respect of the above points and of its final construction dimension & adjustments[.]

A fine thread of Mercury is beautifully arranged within a glass chamber so that when an electric current of sufficient strength is passed through it the thread is broken up & a very luminous discharge occurs. The mercury which is partly dispersed in drops & partly volatalized is collected within the chamber & conducted to a reservoir below & this is done without any derangement of the place of discharge so that that important point is *perfectly obtained* and (when the apparatus is in order) without any issue of the mercury from the lamp into the outer space. The light differs much in character from that between carbon points, being as compared even to white or sun light very feeble in the red rays[.] Nevertheless it is a very beautiful light and not the less luminous or powerful because of its peculiar colour.

The nature of the lamp includes the following points in respect of its service and management in the lanthorn of a light house. It requires an adjustment of the force of the battery to the stream of mercury in the lamp a certain stream requiring for its best effect a certain battery force & vice versa – As the battery by continued action, falls in power, I conclude it will require adjustment – I understand the battery will last four hours and then requires a change – The lamp is externally a small thin glass cylinder fitted with iron ends & joining it may by time & use perhaps permit the accidental escape of mercury into the lanthorn – The glass itself may be broken by the heat within (as lamp glasses are) or by mechanical accidents – and such an event would derange the whole adjustment & throw the lamp out of service – The replacement of one lamp by another could not be instantaneous (like the Magneto electric lamp) but would take time & be rather complicated for a lighthouse keeper – The person or persons in charge of the lamp must have a thorough knowledge of the conditions necessary for its maximum action & of the nature & construction of the lamp & also of the associated Voltaic battery & its connexions[.]

With respect to the considerations which arise regarding the battery itself & the necessity of a place for it & its materials & a staff for its care & repair, I may refer generally to the like considerations at the end of my report on Dr. Watsons Electric light, 14 Aug 1854, and others about the middle of my report on Professor Holmes Magneto electric light 29th April 1859³.

I do not know whether the author of the lamp proposes its trial in a lighthouse or whether he is aware what the Trinity house would require in that case[.] On a former occasion I stated that a light was required – *Sufficient* in intensity – *Equal* at different periods – Of *uninterrupted continuance* for 16 hours – *Certain* in its management by ordinary hands – Not dependent on processes or Machinery liable to *occasional failure* and especially not to *sudden failure* – Not dependent on arrangements requiring *much room* or *much attention*. The Trinity house requires a *perfected* application and an estimate of the outlay & the current expense for a given amount of light – for it *cannot undertake* to develop or perfect any principle & it *cannot introduce* any imperfect or uncertain arrangement into the lighthouse or any which it has not previously witnessed in action to the fullest extent⁴[.]

M. Faraday

1. Joseph John William Watson (d.1886, age 55, GRO). Worked on electrical light; described as a civil engineer in 1871 census, TNA RG10/207, f. 8, p. 8. See also James (1997), 294.

2. Faraday report to Trinity House, 15 August 1854, letter 2878, volume 4.

3. Letter 3590.

4. This letter was read to Trinity House By Board, 28 June 1859, GL MS 30010/42, p. 270. It was ordered to be entered in the book of scientific reports.

Letter 3610

Faraday to Henry Fitzroy

1 July 1859

From *Parliamentary Papers*, 1860 (309), XL, p. 1

Hampton Court Green | 1 Jul 1859

Sir,

I had the opportunity yesterday of seeing Mr. Szerelmey, who communicated to me, *in confidence*, his process, with a slight reservation. I find nothing in his mode of procedure which is objectionable or otherwise than good, or which, upon *principle*, cause me to doubt the success of his process when properly applied¹. I may say the same also of other processes, founded by other persons upon the application of alkaline silicates.

Such being the case, we then turn to the results of observations, trial, and experience, without which no method can be judged of or finally approved. The trials at the Houses of Parliament have extended to periods of two or three years². That the stone of the building requires protection upon the external and weather faces is very manifest; nor can that surprise us when the condition of the London atmosphere, especially in relation to the sulphur acids derived from the coal, and the calcareous nature of the stone, are considered. In my opinion, the results of the trials (and I may say large experiments) made by Mr. Szerelmey are better than any of the rest; and, so far as one can form a judgment upon the experience of two years, justify the expectation that the

process will sustain its character for a lengthened period of time. I think it necessary to say that a process, good in principle, may fail if carried carelessly into practice. Mr. Szerelmey's process has had the advantage of being carried out under the guidance and inspection of himself, the person most fitted for that purpose, and most interested in its success.

I am, &c | (signed) M. Faraday
The Right Hon. H. FitzRoy, M.P.

1. This is the first reference with Faraday's involvement in the problems caused by the decay of the stone of the relatively new Houses of Parliament on which he was asked to comment. His correspondence on the subject was published as a report in *Parliamentary Papers*. However, the problems continued and the result was a major inquiry during early 1861 in which Faraday played no role. The report of the enquiry is in *Parliamentary Papers*, 1861 (504), XXXV.

2. Since the Spring of 1857 Szerelmey had been coating the stone to try and prevent its further deterioration. See the report on the decay of stone, *Parliamentary Papers*, 1861 (504), XXXV, question 1.

Letter 3611

Faraday to John Hall Gladstone

2 July 1859

From the original in RS MS 743.85

[Royal Institution embossed letterhead] | 2 Jul 1859

My dear Gladstone,

Although I have frequently observed lights from the sea the only thing I have learnt in relation to their *relative brilliancy* is that the average of a very great number of observations would be required for the attainment of a moderate approximation to truth. One has to be some miles off at sea or else the observation is not made in the chief ray & then one does not know the state of the atmosphere about a given lighthouse^[.] Strong lights like that of Cape Gri[s]nez have been invisible when it should have been strong^[.] feeble lights by comparison have risen up in force when one might have expected them to be relatively weak – and after enquiry has not shewn a state of the air at the lighthouse explaining such differences.– It is probable that the cause of difference often exists at sea – Besides these difficulties there is that other great one of not seeing the two lights to be compared, in the field of view at the same time & same distance. If the eye has to turn 90° from one to the other I have no confidence in the comparison. And if both be in the field of sight at once still unexpected & unexplained causes of difference occur. The two lights at the South Foreland are beautifully situated for comparison & yet sometimes the upper did not equal the lower when it ought to have surpassed it. This I referred at the time to an Upper Stratum of haze – but on shore they knew nothing of the kind nor had any such or other reason to expect particular effects.

Ever Truly Yours | M. Faraday
Dr. Gladstone | &c &c &c

Letter 3612

Julius Plücker to Faraday

3 July 1859

From the original in IET MS SC 2

Bonn | 3 Jul 1859

My dear Sir,

I thank you very much for your last kind letter¹, allow me to answer it in giving a short notice on a new paper, printed just now in Poggendorffs Annalen². I get a series of beautiful electric spectra by conducting the discharge of Ruhmkorff's Apparatus through a capillar tube. Two larger spheres or cylinders, into which the electrodes enter, communicate by means of such a tube. The apparatus contains traces only of any gas or vapour. The general fact is that such a spectrum consists of a certain number of distinct coloured bands, having each the same largeness as the aperture has, when directly observed, without the interposition of the prism: i.e. the light in the spectrum is expanded in a certain number of discontinuous rays, each of which has a certain refrangibility, a certain length of wave [sic].

In hydrogen-gas there are only three such rays, which I called $H\alpha$, $H\beta$ and $H\gamma$. $H\beta$ exactly coincides with Fraunhofer's³ dark line F. $H\alpha$ and $H\gamma$ are very near to C and G. The new bright lines may be most easily observed and their position measured with the greatest accuracy. *) They replace with great advantage Fraunhofer's dark lines in determining the indices of refraction &c.

The bright bands of Oxygen are quite different ones, but not so easily obtained. Most brilliant are these bands in Chlorine, Bromine, Iodine, Mercury, and especially in Chloride of tin (which partially is decomposed). In the larger parts of the apparatus the discharge through the Chloride is of a deep blue colour, in the capillar tube it shows the finest colour of pure gold. (When a larger tube is placed upon the Electromagnet golden lightnings, directed by the magnetic force continually move through the blue coloured space – a most beautiful and striking experiment.) Metallic Sodium, within an atmosphere of rarified hydrogen-gas presents, when heated only one brilliant yellow ray. The spectrum of Nitrogen is the richest in colours. The space of the Red Orange and Yellow is divided by 17 dark lines in 18 bands of the same largeness. The violet bands are very brilliant. Accordingly this gas produces fluorescence in a high degree (Hydrogen-gas does not). Phosphorus, when heated in an atmosphere of rarified hydrogen, extinguished the spectrum of this gas, without producing new bands. &c &c &c.

The spaces between the bright bands is either absolutely black or of a greyish colour, or they are faintly coloured according to their place in the spectrum.

The aperture of Goniometer, illuminated by the gas, appears in my observations under an angle of 3 minutes, so does every single bright band in the spectrum. Smaller bands are never observed, larger bands are frequently.

In the midst of bands, smaler then 6' a bright line is often seen. Bands larger then 6' are commonly divided by dark lines in two or more single ones, according to their largeness. Nearly all the violet part of the Nitrogen-spectrum offers striking instances of this case.

By diminishing the aperture of the Goniometer separated bright lines are finally obtained, some of them constituting groups. The refraction of these lines exactly equals to the refraction of the middle line of the former bands.

There is no doubt that the questioned spectra belong to the *gaz* only, the metal of the electrodes has nothing to do with them. I think, properly speaking, there exists no "electric light." The gazes become luminous by the current, Indeed what ought to be the enormous heat of the traces of the gaz within the capillar tube of the apparatus by which the temperature of the thik glas of this tube is increased 20°C and more. The difference of colour in smaler and larger parts of the same apparatus depends on the difference of heat.

I dont mention here any of the curious chemical actions produced within the spectra tubes and indicated by the spectra themselves.—

With regard to the stratification of light I observed many new phenomena, but my theoretical views are rather not promoted by them. The most curious fact is this. The *luminous discharge* which passes through certain tubes (one of them was about 6" long and 1" thik) is in all its length, by the Magnet, transformed into a *dark discharge of the same intensity*. This is easily shown, if such a tube, placed upon the iron pieces of an Electromagnet, communicates with another tube in some distance from it. Then both tubes become luminous by the same electric discharge as long as the Electromagnet does not act, under its magnetic action the light in the first tube disappears, in the second tube it remains quite unaltered.

But all such phenomena are very difficult to describe: I wished I could *show* them to you. Unhappily our most uncertain political situation⁴ is till now not at all favourable to visit England this year.

Pray, dear Sir, present my respect, to Mad. Faraday. With the sincerest feelings of veneration

Yours | Plücker

* I allways used a larger Goniometer comonly called of Babinet⁵.

1. Not found.

2. Plücker (1859b).

3. Joseph Fraunhofer (1787–1826, DSB). Bavarian glass maker and discoverer of the eponymous lines in the solar spectrum.

4. A reference to the war between France and Austria over the control of Italy. See *Ann. Reg.*, 1859, 101: 188–261.

5. Jacques Babinet (1794–1872, DSB). French physicist.

Letter 3613**Alfred Bonham-Carter¹ to Faraday****5 July 1859****From *Parliamentary Papers*, 1860 (475), XL, p. 1**

Office of Works, &c | 5 Jul 1859

Sir,

In acknowledging the receipt of your letter of the 1st instant², I am desired by Mr. Fitzroy to request that you will kindly inform him whether Ransome's process³ for the preservation of stone is inferior to that of M. Szerelmey⁴.

Upon the personal inspection which you made in the company of the First Commissioner, the merits of the two processes appeared so equally balanced that Mr. Fitzroy trusts you will permit him to ask whether, having upon mature consideration decided in favour of the latter, it be by intention that you have omitted all mention of the former in your present communication.

In expressing to you Mr. Fitzroy's regret for giving you further trouble in this matter,

I am, &c | (signed) A. Bonham-Carter
Professor Faraday

1. Alfred Bonham-Carter (1825–1910, WWW1). Private Secretary to the First Commissioner of Works, 1859–1866.

2. Letter 3610 and note 1.

3. Ransome's process (see his Patent 1856-2267) had also been used on the stone of the Houses of Parliament. See the report of on the decay of stone, *Parliamentary Papers*, 1861 (504), XXXV, question 196.

4. See note 2, letter 3610.

Letter 3614**Faraday to Alfred Bonham-Carter¹****8 July 1859****From *Parliamentary Papers*, 1860 (475), XL, p. 1**

Hampton Court Green | 8 Jul 1859

Sir,

In reply to the Chief Commissioner's² further inquiry³, I beg to say that I do not wish to give an opinion, as only between the processes of two persons, Mr. Ransome and M. Szerelmey, both of which are founded upon the process of M. Fuchs⁴, and the use of substances alike in their nature and principle with the water glass and silicates; and whilst thinking over and comparing the results at the Houses of Parliament, I took into consideration other experimental results regarding silicates, which had on former occasions come under my view.

The consequence is that if I were responsible in the present state of the evidence for the selection of one process only for application to the Houses of

Parliament, I should select that of Mr. Szerelmey; but that if I could set apart a fair portion of the building (fair I mean as to exposure and future comparison) for the application of the silicate process (which might be Ransome's) I would do it; and according to the weight of the evidence upon my own mind at present, I would try to make this proportion about a third or fourth part of the whole.

I am, &c | (signed) M. Faraday
A. Bonham-Carter, Esq.

1. Alfred Bonham-Carter (1825–1910, WWW1). Private Secretary to the First Commissioner of Works, 1859–1866.
2. Henry Fitzroy.
3. In letter 3613.
4. Johann Nepomuk Fuchs (1774–1856, DSB). Bavarian chemist. See Fuchs (1859) for his process.

Letter 3615

Thomas Stevenson to Faraday

8 July 1859

From the original in IET MS SC 2

Edinb. | 8 Jul 1859

Dear Sir,

I lately discovered by accident an erroneous date at page 35 of my volume on Lighthouse Illumination¹, as to the early history of lens Lights. This date was given on the authority of papers in the Edinburgh Transactions² & Phil. Journal³. Although the mistake has no reference to any of my own improvements it nevertheless materially affects the relative claims of others and I have therefore considered it to be only justice to them to cancel the remaining copies of the book still on hand and to substitute a corrected issue⁴. If you will be so good as to leave out the copy which I lately took the liberty of sending to you⁵ one of the new issue will be given in its place.

I hope that your labours in connection with the electric light may be soon crowned with success. I imagine however that where the flame is so small it would be necessary to employ the spherico-cylindric Lens which I proposed in the volume on Lt. H. illumination for increasing the duration of the flash in Revolving Dioptric Lights. By grinding the back of a common annular lens I think any amount of divergence could very easily be secured without taking it to pieces.

Trusting you will excuse this trouble

I remain | Dear Sir | Yours faithfully | Thomas Stevenson
Dr. Faraday | &c &c

1. Stevenson (1859a), 35 noted 'So far back as 1812 Sir David Brewster suggested most important improvements in the illumination of lighthouses ... Though the earliest improvement with which I am acquainted, it is in some respects the most perfect'.

2. Brewster (1827).
3. Brewster (1823).
4. Stevenson (1859b). On p. 35 he noted the error and withdrawal of the first issue and in the text replaced the sentences quoted in note 1 with 'In 1823 Sir David Brewster published in the 'Edinburgh Philosophical Journal' a design for lighthouses [Brewster (1823)], which is in some respects more perfect than those which are to be afterwards mentioned. He originally proposed this optical arrangement for burning in 1812'.
5. See letter 3581.

Letter 3616

Faraday to Thomas Stevenson

13 July 1859

From the original in NLS MS 785, f. 47

The Green Hampton Court | 13 Jul 1859

My dear Sir,

In breaking up my town establishment & coming here I have mislaid your volume¹ which was to come here & for the moment cannot find it. It will turn up soon & shall be put apart²[.]

As for the Electric light it remains with Trinity House & Government to decide on its application. That Professor Holmes sustained a light beyond any other I have seen in a lighthouse is an established fact, but there are certain points respecting security – expence &c which are serious³. The light is not mine any further than as Professor Holmes employs the Magneto-electric current *which I discovered* so the Electricity thus obtained is often named after me & I suppose you have in that way heard my name associated with the light. The *first* Magneto-electric spark was mine⁴ but it was very different to the spark in the South Foreland lighthouse[.]

I am | My dear Sir | Very Truly Yours | M. Faraday
Thos. Stevenson Esqr. | &c &c &c

1. Stevenson (1859a).
2. See letter 3615.
3. See letter 3590.
4. See Faraday to Forbes, 19 May 1832, letter 581, volume 2.

Letter 3617

François Napoleon Marie Moigno to Faraday

13 July 1859

From the original in IET MS SC 2

2 rue Servandoni | 13 juillet 1859

Mon cher Monsieur et Maitre illustre

Permettez moi d'introduire près de vous Mr. Samuel Phipson¹, père d'un jeune docteur anglais² qui habite Paris avec moi depuis trois ans et qui

cultive les sciences physiques et chimiques avec la plus grande ardeur, et un veritable succès.

Mr. Phipson père veut appeler votre attention sur un systeme de ventilation très excellent à mon avis, très simple, très efficace et que je crois riche d'un grand avenir. Il desirerait faire l'application du système à l'amphithéâtre de Royal Institution; et j'appelle cet espoir de mes vœux les plus ardens.

Je suis un peu en retard avec quelques unes de vos dernières publications, mais je reprenai bientôt le temps perdu. J'ai admiré dans l'Athenaeum de cette semaine la manière sympathique dont vous avez introduit dans le monde scientifique anglais les belles experiences de Mr. E. Becquerel³.

Je suis dans les sentimens reconnaissans | d'une considération très distinguée | votre très humble et tout | dévoué serviteur | l'abbé F. Moigno

TRANSLATION

2 rue Servandoni | 13 Jul 1859

My dear Sir and illustrious master,

Permit me to introduce to you Mr. Samuel Phipson¹, the father of a young English doctor² who has lived in Paris with me for three years and who cultivates the physical and chemical sciences with great ardour and veritable success.

Mr. Phipson senior would like to draw your attention to a system of ventilation which, in my opinion, is excellent, very simple, very effective and which I believe to have a great future. He would like to show the application of the system in the theatre of the Royal Institution; and I call this desire one of my most ardent wishes.

I am a little delayed in reading some of your last publications, but I shall make up lost time soon. I admired in this week's Athenaeum the sympathetic way in which you introduced to the English scientific world the beautiful experiments of Mr. E. Becquerel³.

I am with grateful sentiments | and with most distinguished consideration | your most humble and most | devoted servant | abbé F. Moigno

Endorsement: Ventilation

Address: Monsieur | Monsieur le Professeur Monsieur Faraday | Royal Institution | Albemarle Street | London

1. Samuel Ryland Phipson (d.1887, age 84, GRO). Manufacturer and businessman. Bouverie (1884), 4–5.

2. Thomas Lamb Phipson (d.1908, age 74, GRO). Chemist, violinist and *Cosmos* correspondent. Bouverie (1884).

3. Faraday (1859c), Friday Evening Discourse of 17 June 1859, reported in *Athenaeum*, 9 July 1859, pp. 54–6.

Letter 3618**Faraday to John Fretcheville Dykes Donnelly¹****15 July 1859****From a photocopy in RI MS**[Royal Institution embossed letterhead],
Green Hampton Court | Friday, 15 Jul 59

My dear Sir,

I fear we are getting into confusion with our triangular correspondence[.] Dr. Tyndall says next Monday Tuesday or Wednesday² at 11 P.M. I had engaged the beginning of next week counting upon our meeting either today or tomorrow[.] To settle matters let us say next Wednesday 2 o'clk P.M. Will you summon Committee for that day & hour?³

Ever Truly Yours | M. Faraday
Captn. Conolly [sic] RE | &c &c &c

1. John Fretcheville Dykes Donnelly (1834–1902, ODNB). Captain in the Royal Engineers and an official of the Department of Science and Art at South Kensington. This identification is made on the basis that there was no Captain Conolly (or Connolly) in the Royal Engineers at this time and on Donnelly's position at the Department of Science and Art.

2. That is 18, 19 or 20 July 1859.

3. This refers to a meeting of the commission established by the Department of Science and Art to consider the lighting of galleries by gas (see note 1, letter 3602). Both Faraday and Tyndall were members. The report of the commission, dated 20 July 1859, is in *Parliamentary Papers*, 1859, 2nd session (106), XV.

Letter 3619**Christian Friedrich Schoenbein to Faraday****19 July 1859****From the original in UB MS NS 449**

Bâsle | 19 Jul 1859

My dear Faraday,

The long silence I have kept to you is, I am afraid, the most palpable proof of your friend's having become a poor man indeed. Formerly it was a real treat to me to write you a letter, now I have to make the greatest effort to take up my pen and fulfill even the most urgent duties of a correspondent, and it is hardly necessary to tell you the cause of that change: my mind is no more, what it was a short time ago, its former cheerfulness is gone and melancholy feelings and sadness have taken possession of it, weighing the more heavily upon me, than Mrs. Schoenbein is very far from being comforted and consoled about our grievous loss. Indeed time has as yet proved to us all a very poor healer of the deep wound, which was inflicted upon us four months ago.

To distract a little my mind from domestic sorrow and to forget the highly deplorable state of affairs of Europe¹ I have these last three months shut up myself in my laboratory and I may say turned my back upon the rest of the world, avoiding even to touch a newspaper or to hear a syllable spoken about politics. Dry and stale as the subject must be to a mind grievously affected, I mean oxygen, I have taken it up again and worked upon it harder than I ever did. And I think not quite for nothing. First I ascertained the hypochlorites, manganates and ferrates (or rather the acids of those salts) to be "Ozonides", i.e. decomposable by the Antozonides: HO_2 , KO_3 , BaO_2 &c. Then I tried to show, that the nascent state of Oxygen as such has nothing to do with the oxidizing powers of that element, and during the last six weeks I have almost exclusively occupied myself with what call "the chemical polarization of neutral oxygen". After having once ascertained a number of facts (known to you) from which I drew the inference, that there are two active kinds of oxygen standing to each other in the relation of $+$ to $-$, I thought it possible, even likely, that both kinds of active oxygen are at the same time produced out of inactive O, as often as one of them makes its appearance. Proceeding from those notions I first looked for $\text{HO} + \oplus$ as a production of the slow combustion of phosphorus, during which process, as it is well known, ozonized oxygen = \ominus is engendered. My conjecture proved fully correct, peroxide of hydrogen being produced and contained in the sour fluid called phosphatic acid. And so closely are the two facts connected with one another, namely the ozonisation of inactive oxygen and the formation of HO_2 , that you will never obtain the one substance without the other. Being once sure of that important coincidence, I extended my researches to the productions of the slow combustion of Ether and found to my no small satisfaction, that in this case too notable quantities of peroxide of hydrogen (the type of the Antozonides) are engendered conjointly with another compound containing \ominus (or Ozonide). After having ascertained those facts, my attention was directed to the electrolysis of water and I think, there can be entertained no doubt, that not only \ominus but also HO_2 i.e. \oplus makes its appearance at the positive electrode. Under proper precautions I have reduced permanganic acid to MnO , CrO_3 to Cr_2O_3 &c. in fact desoxidized a number of bodies at that electrode. Reducing oxy-compounds at the electrode, where oxygen is disengaged seems to be paradoxical enough. As to the small quantities of ozonized oxygen disengaged and HO_2 formed during the electrolysis of water at the positive Electrode, I think, they must be considered as the surviving witnesses of the chemical polarization of the O of HO_2 , which O is transformed by the current into \oplus and \ominus . The inactive Oxygen disengaged during the Electrolysis of water is most likely a secondary production proceeding from the depolarisation or neutralization of \oplus i.e. \ominus . Before long my papers on those queer subjects will be published and you shall

have them as soon as possible, as I flatter myself, that the matter will interest you. If I have correctly accounted for the novel facts lately discovered by me i.e. if neutral oxygen be capable of being chemically polarized, or thrown into opposite states of chemical activity at the same time, well, I should think, I had done something to advance a little our knowledge of that mysterious and important element.

Our Midsummer-holidays haveing commenced I intend to go one of these days to the "Berner oberland" to fetch my two eldest girls, who have for some weeks been staying at a watering-place for the use of a mineral spring there and returning we have a notion to visit a retiring part of the black forest.

I confidently hope, you, Mrs. Faraday and your Niece are doing well and as to Miss Hornblower I was very sorry indeed to learn from you, that she had been obliged to undergo a painful operation². I ardently wish that by this time, she will be entirely cured and enjoy perfect health. Pray remember me most friendly to all of them, excuse my pale, stale and insipid letter and write soon to

Your | poor friend | C.F. Schoenbein
Dr. M. Faraday | &c &c &c

1. A reference to the war between France and Austria over the control of Italy. See *Ann. Reg.*, 1859, 101: 188–261.
2. See letter 3588.

Letter 3620

Faraday to Charles Lock Eastlake

25 July 1859

From the original in NG MS

The Green Hampton Court | 25 Jul 1859

My dear Sir Charles,

Having come to a conclusion respecting the lighting of picture galleries by gas and presented our report to the Lords of the Privy council I now send you a copy¹. It will answer the enquiries in your letter² as far as I can at present answer them (for as to the special action you refer to they would require long & laborious investigation) & the answer will be the better for being the joint conclusion of those whose names are appended to the Report[.]

Ever My dear Sir Charles | Your Very faithful | Servant | M. Faraday

1. The report of the commission to consider gas lighting in galleries, dated 20 July 1859, is in *Parliamentary Papers*, 1859, 2nd session (106), XV.
2. In letter 3602.

Letter 3621**Edward Sabine to Faraday****5 August 1859¹****From the original in IET MS SC 2**

15, Ashley Place, S.W | 5 Aug

Dear Faraday,

Dr. Robinson² is staying with me for a few days whilst he works daily with Gassiot in the latter's laboratory. Gassiot dines with him here on Tuesday next (the 9th) at $\frac{1}{2}$ past six, when it would be a very great gratification to Mrs. Sabine³ & I if we could prevail on you to meet them; or if you do not like dining out (as I have heard is the case) if you would come to us for an hour or two in the evening; suiting Yourself as to Time, as we shall be otherwise quite alone.

Sincerely yours | Edward Sabine
Watermark: 1858

Endorsed by Faraday: H.C. 7.15 W 10.⁴

1. Dated on the basis of the watermark and that 9 August 1859 was a Tuesday.

2. Thomas Romney Robinson (1792–1882, ODNB). Director of the Armagh Observatory, 1823–1882.

3. Elizabeth Juliana Sabine, née Leves (1807–1879, ODNB under E. Sabine). Scientific translator. Married Sabine in 1826.

4. These are the train times from Hampton Court station and return from Waterloo station as given in *Surrey Comet*, 6 August 1859, p. 1, col. f.

Letter 3622**Faraday and Sarah Faraday to John Barlow****10 August 1859****From the original in University of London Library MS AL 49b**

Hampton Court Green | 10 Aug 1859

My dear Barlow,

It was very kind of Mrs. Barlow to write to my wife, and I think that a letter from me to you, is hardly the fit way to acknowledge it:– yet as I write to you you must say something fit for us, with our kindest remembrances, and I expect my wife will write soon. We are exceedingly glad to hear that your journey has been good.– your company a happy one – & your present life healthy & joyous.– Before I forget remember me to Mr. Esmead¹ – I cannot tell whether I have been at Spa², or have any friends there; if there are such I dare say you will find them out. We are jogging on or rather we are *not* jogging on:– for on Monday last³ the workmen ceased to come, the builders strike being on⁴. As far as I can learn, both Masters & Men enter *quietly* into it; so that though it seems as if it might come to an end in a few days, it may

perhaps go on for some time & cause great distress and trouble. We must just wait.

I am not able to give Mrs. Barlow's message to Tyndall, for I think he started last Monday Morning for Chamounie [sic], and I have not seen him since Saturday⁵. Frankland goes with him. He spoke of taking the higher optics for his next course of lectures⁶, and I agreed with him in the propriety (if he saw occasion) of spending £20 on apparatus fitted for the course and for investigation. I do not suppose that he will come across any clear good optical *rock salt*, but we find great difficulty in raising it from the mines in England. Indeed it is rather an impossibility than a difficulty.

As I have been out here with only runs into town I really know very little of what is going on there:— and what I learn I forget. The Senate of the University accepted & approved of the Report of the Committee for Scientific degrees, so that that will go forward — (if the Government approve) and will come into work next year⁷. It seems to give much satisfaction to all who have seen it, though the subject is beset with difficulties:— for when the depth & breadth of Science came to be considered, and an estimate was made of how much a man ought to know to obtain a right to a degree in it, the amount in words seemed to be so enormous as to make one hesitate in demanding it from the student:— and though in the D.S one could divide the matter and claim eminence in one branch of Science, rather than good general knowledge in all — still in the B.S. which is a progressive degree a more extended though a more superficial acquaintance seemed to be required. In fact the matter is so new & there is so little that can serve as previous experience in the founding & arranging these degrees, that one must leave the whole endeavour to shape itself as the practice & experience accumulates.

We have had very hot weather but it is & has been cooler for a few days. The Thames has very rarely been seen so low in water here, as it is now. In London its bad condition seems to increase; or else my nose accustomed to a better odour here feels it more. The harvest all round us is nearly in; being very early; but they tell me the corn is not so heavy as was to be desired. Want of rain has kept it small.

Ever My dear Barlow | Very Truly Yours | M. Faraday

Dear Mr. Barlow

My right hand *Jane* being absent in Scotland I am afraid my husband has promised more for me than I can perform so Mrs. Barlow will kindly take Mr. Faradays letter as an answer with my best remembrances

Sincerely yours | S. Faraday

1. Graham Moore Michael Esmeade (d.1883, age 77, GRO). Gentleman and member of the Royal Institution.

2. It is very unlikely that Faraday ever visited Spa.

3. That is 8 August 1859.

4. That is 6 August 1859.

5. This strike had started on 24 July 1859 and lasted ten weeks. See *Ann. Reg.*, 1859, **101**: 105–7. The strike occurred while considerable building work was being undertaken at the Royal Institution. See RI MM, 6 July 1859, **11**: 293–9. Work was delayed and was still not completed by November, RI MM, 7 November 1859, **11**: 302.

6. Tyndall delivered a course of twelve lectures on ‘Light Including its Higher Phenomena’ before Easter 1860. RI MS Le4/228.

7. This refers to the approval of the detailed content of the University of London new science degrees by the senate at its meeting on 3 August 1859 which Faraday attended. University of London Senate Minutes, 3 August 1859, pp. 71–89.

Letter 3623

Faraday to Caroline Deacon

12 August 1859

From the original in the possession of Elizabeth M. Milton

The Green S.W. Hampton Court | 12 Aug 1859

My dear Caroline,

I am a little tired dull and unable to work or even to read; so I write to you. I have your letter before me & so that is a moving cause, and it is rather grave & that renders the cause more effectual, I never heard of the saying that separation is the brother of death, I think that it does death an injustice, at least in the mind of the Christian – Separation simply implies no reunion – death has to the Christian everything hoped for contained in the idea of reunion – I cannot think that death has to the Christian anything in it that should make it a rare or other than a constant thought; out of the view of death comes the view of the life beyond the grave as out of the view of sin that true & real view which the Holy spirit alone can give to a man comes the glorious hope; without the conviction of sin there is no ground of hope to the Christian[.]. As far as he is permitted for the trial of his faith to forget the conviction of sin he forgets his hope he forgets the need of him who became sin or a sin offering for his people & overcame death by dying and though death be repugnant to the flesh yet where the Spirit is given, to die is gain¹[.]. What a wonderful transition it is, for as the apostle says even whilst having the first fruits of the spirit the people of God groan within themselves waiting for the adoption to wit the redemption of the body². Elsewhere he says that whilst in the earthly house of this tabernacle we groan earnestly desiring to be clothed upon with our house which is from heaven³[.].

It is permitted to the Christian to think of death – he is even represented as praying that God would teach him to number his days – Words are given to him, Oh grave where is thy sting? Oh death where is thy victory?⁴ and the answer is given him thanks be unto God who giveth us the victory through our Lord Jesus Christ⁵. – And though the thought of death – brings the thought of judgment which is far above all the trouble that arises from the breaking of mere earthly ties – it also brings to the Christian the thought of him who died was judged and who rose again for the justification of those

who believe in him. Though the fear of death be a great thought, the hope of Eternal life is a far greater. Much more is the phrase the apostle uses in such comparisons though Sin hath reigned unto death⁶. Much more is the hope of Eternal life through Jesus Christ⁷. Though we may well fear for ourselves & our faith much more may we trust in him who is faithful – & though we have the treasure in earthen vessels⁸ & so are surrounded by the infirmities of the flesh with all the accompanying hesitation – temptations & the attacks of the adversary – yet it is that the excellency of the power of God may be with us.

What a long grave wording I have given you but I do not think you will be angry with me. It cannot make you sad, the troubles are but for a moment there is a far more exceeding & eternal weight of glory for them who through Gods power look not at the things which are seen but at the things which are not seen⁹. For we are utterly insufficient for these things but the sufficiency is of God¹⁰ & that makes it fit for his people – his strength perfect in their weakness¹¹[.]

You see I chat now & then with you as if my thoughts were running openly before us on the paper & so it is. My worldly faculties are slipping away day by day happy is it for all of us that the true good lies not in them. As they ebb may they leave us as little children trusting in the Father of mercies and accepting his unspeakable gift[.]

I must conclude for I cannot otherwise get out of this strain but not without love to Constance & kindest remembrances to Mr. Deacon.

Ever Your Affectionate Uncle | M. Faraday

1. Philippians 1: 21.
2. Romans 8: 23.
3. 2 Corinthians 5: 1.
4. 1 Corinthians 15: 55 (where the quotation is in the reverse order).
5. 1 Corinthians 15: 57.
6. Romans 5: 21.
7. Romans 6: 23.
8. 2 Corinthians 4: 7.
9. 2 Corinthians 4: 18.
10. 2 Corinthians 3: 5.
11. See 2 Corinthians 12: 9.

Letter 3624

John Barlow to Faraday

13 August 1859

From the original in IET MS SC 2

Hotel de York Spa | 13 Aug 59

My dear Faraday,

You could have had no misgiving when you were writing your letter¹, as to the amount of pleasure it would afford us. We are glad to think of you &

Mrs. Faraday enjoying pure air and quiet, & hearing, as I trust you do, good news from Miss Barnard. You will be glad that my wife is well. I am rather feeble, my right eye is not yet restored. My right hand is very slow in writing, and I am soon tired in a walk.. Luckily these infirmities do not diminish the pleasure I experience from intercourse with valued friends.

– Of course we shall gladly acquiesce in your outlay for Tyndall's Lectures. I only hope that his optics may not be "higher" next year than the intellects of his hearers². I think it very important that our Lectures should be original, & such as can only be given by the original research of the Lecturer; that they should be illustrated by striking experiments, so as to present a beautiful outline-map of the subject, such as any one, who would give continuous intelligent attention to the Lecture, would both apprehend and retain. Such were your own Saturday Lectures in old time. Now I dread the tendency of Tyndall's Lectures to become abstruse – Illness and the meetings of the R. Soc. Council, deprived me of many of them last Spring³ but I thought some of those I did hear, difficult, especially as there was no text book for the student to refer to. This remark I would apply, with greater force, to the chemical lectures except the last⁴. Many of these have been quite out of the comprehension of any but chemists.– Within the last few years Jermyn Street⁵ has supplied the wants of those who require detailed & deep instruction.

But I have prosed too long on this subject. I was very glad to get a cheerful note from Tyndall on the eve of his departure. He said nothing about Lectures, but he talked hopefully of the issue of his researches into the gases' interference with the waves of heat⁶.

I do not know that there has been any enterprise of yours in which I have more cordially bidden you "God speed" than in your efforts for the scientific degree. It will be the motive for high powers, now too much neglected, as judgment and accuracy, being cultivated; and it will constitute an order of men, much needed now, to fulfil very important purposes in society. Then it will enormously add to the usefulness of mathematical proficient, who may be induced to compete for it. Mr. Hudson⁷, one of the Assistant Tutors of Trinity, is here: he tells me that, as yet, little has been done in the scientific Tripos at Cambridge⁸.

You would have few sympathizers at Spa. There are pretty walks & drives; & the geological structure of the hills is interesting – But nothing is thought of except gambling. Though, for many reasons, I abstain from playing, I often go to the Tables to watch the curious succession of events which occur, and to listen to the still stranger inferences which are drawn from them. As, during the plagues infallible preventives and cures were sold, so here people profess to have devised schemes which *must* win. Some of these are extremely plausible, and it has been an occupation to me to detect their fallacy, and to try to demonstrate the mathematical certainty that those, who play long enough, must lose. There have been wonderful games – an Englishman won £1200 two days ago. This is as good for "the bank" as a

fire is for an insurance office. It is already said to have made £60,000 and the season is little more than half over . . .

I hear that the same cause which you assign has diminished the harvests all over central Europe.

Mr. Esmeade⁹ sends his cordial remembrances. Mrs. Barlow her love to Mrs. Faraday with my thanks for her few lines.

Ever, dear Faraday | your attached friend | John Barlow

1. Letter 3624.

2. Tyndall delivered a course of twelve lectures on 'Light Including its Higher Phenomena' before Easter 1860. RI MS Le4/228.

3. Tyndall delivered a course of twelve lectures on the 'Force of Gravity' before Easter 1859. RI MS Le4/217.

4. These were delivered by William Allen Miller (1817–1870, ODNB), Professor of Chemistry at King's College, London, 1845–1870. See RI MS Le4/214 and 218.

5. That is the Royal School of Mines.

6. See Tyndall (1859).

7. Thomas Percy Hudson (d.1921, age 88, AC). Tutor of Trinity College, Cambridge, 1856–1870.

8. Roberts (1980); Wilson, D.B. (1982).

9. Graham Moore Michael Esmeade (d.1883, age 77, GRO). Gentleman and member of the Royal Institution.

Letter 3625

Faraday to Henry Bence Jones

15 August 1859

From the original in RI MS F1 E19

The Green Hampton Court S.W | 15 Aug 1859

My dear friend,

If you permit I am thinking of bringing my wife up to you some time in the week; and yet as the case may not require it I will tell you what it is. The left leg, as you know, is weak & I suppose partially paralysed:– the right has therefore more to do:– the left has become thinner & the right stouter, for some time past the right has been enlarging in front on the inside of the bone & has been of a *higher temperature* there, than elsewhere, or than the corresponding part on the left leg – but all this may be natural & the consequence of the different degrees of use of the two limbs. If you think so, could you by a single line say “Dont come up” – or else “come up”. We could be with you at $\frac{1}{2}$ p 10 or after – either on Wednesday or Thursday Morning¹ – or later in the week if you liked.

Ever Truly Yours | M. Faraday

Dr. B. Jones | &c &c &c

There is *no* appearance of inflammation or redness;– but a little heat shooting.

1. That is 17 or 18 August 1859.

Letter 3626

George Biddell Airy to Faraday

16 August 1859

From the original in IET MS SC 2

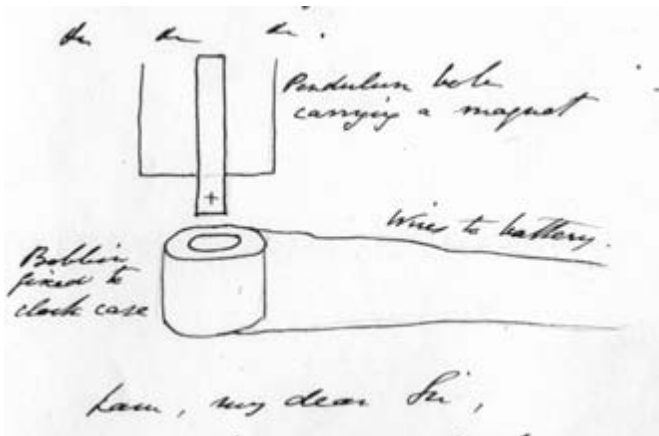
Royal Observatory, Greenwich, London, S.E | 16 Aug 1859

My dear Sir,

I want your assistance in a practical galvanic matter.

I am about to introduce a method of altering the rate of a clock by what is in fact a diminution or an increase of the gravity of the pendulum: thus effected. To the pendulum is to be lashed a bar-magnet pointing up-and-down. To the clock case is to be fastened an ordinary galvano-magnet bobbin without its iron core, so that one pole of the magnet will, in the vibrations of the pendulum, sweep close over the pole of the bobbin. And when we send one current through the bobbin, it attracts the magnet and accelerates the clock: with the opposite current, the clock is retarded. All this works well.

Now I want your information as to the best form of the bobbin or other convolution of wires. Ought it to be a bobbin? Ought there to be any particular relation of inside diameter to outside diameter? Ought the bobbin be long or short? &c &c &c.



I am, my dear Sir | Yours very truly | G.B. Airy
Professor Faraday

Letter 3627

Faraday to George Biddell Airy

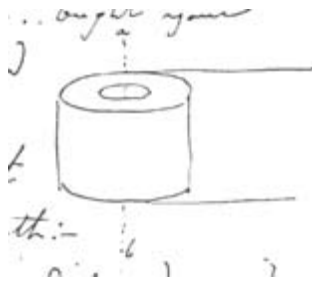
18 August 1859

From the original in RGO6/629, f. 448-9

The Green Hampton Court | 18 Aug 1859

My dear Sir,

Your subject is a difficult one for a note it wants a conversation¹. A note will carry a *precise* answer but only conversation would permit such a development of an idea (as yet vague in its form) as to allow something like an answer at the end. Ought your controlling power to be compressed as much as might be, into the line a. b.?



then ought the helix to be a solid bobbin & have length:— or ought the former to be diffused and approach more towards equality over a somewhat large field? then ought the helix to be a ring:— shut & having its internal & external diameter not very different:— between which ring however & the full bobbin their proportion might vary in every degree according to your requirement. I cannot answer these points without knowing what you want: or without knowing what any given arrangement gives you & how far it falls short of your requirements.

You speak of a bobbin only, perhaps because its force will vary in *constant* proportion to the force of the Electric current through it. I suppose you have thought of the use of an *iron* core with the bobbin? I suppose there is no doubt that such a combination would be more uncertain in the proportion of its force to the current sent into the wire than the former, if the current *were strong*, because of the retention of magnetism by the core. But if the size of the core were so large that the iron was never much magnetized, & the iron itself very soft & well annealed, I think it likely that you would have a field of magnetic power as *strong* as the former, as *true* in its variation with the varying current, and yet with a current so much less in power as to be more regular & constant in other respects that might be of importance and advantage in practice.

But I am talking quite at random so will say no more at present[.]
 Ever Very Truly Yours | M Faraday
 Astronomer Royal | &c &c &c

1. Letter 3626.

Letter 3628

George Herbert to Faraday

18 August 1859

From the original in GL MS 30108/3/90.23

Trinity House | 18 Aug 1859

My dear sir,

I enclose you a copy (M) of letter received from Major Fitzmaurice¹ and of my proposed reply² thereto,– will you kindly tell me whether the letter meets your view,– before I bring it forward for approval.

Yours very faithfully | George Herbert
 Professor Faraday

1. Fitzmaurice to Berthon, 9 August 1859, GL MS 30108/3/90.24.
2. Trinity House to Fitzmaurice, undated draft, GL MS 30108/3/90.25.

Letter 3629

James Clark to Faraday

18 August 1859

From the original in RI MS Conybeare Album, f. 21

22 L. Brook Street. Grosvenor Square. W | 18 Aug 1859

Dear Mr. Faraday,

I hope you mean to attend the meeting of the British Association at Aberdeen, and that you will pay me a visit at Birk Hall. A few days on Dee side will have you for the meeting, and I promise to shew you some fine mountain scenery. Sir B. Brodie is coming to Birkhall to spend a few days before the meeting, &, if convenient to both, you might come together, and I will give you instruction how to perform the journey most conveniently.

I called this afternoon at the Royal Institution but found that you had left, and was expected again on Saturday¹ – on that day I am going to Osborne, otherwise I would have endeavoured to see you.

Very faithfully | Yours | Jas Clark

1. That is 20 August 1859.

Letter 3630

George Biddell Airy to Faraday

19 August 1859

From the original in IET MS SC 2

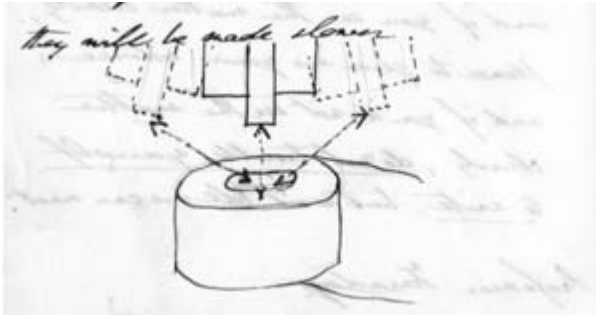
Royal Observatory, Greenwich, London, S.E | 19 Aug 1859

My dear Sir,

Your note¹ shews clearly how difficult it is for a person who is possessed of all circumstances to think of detailing them sufficiently to any body else. I will supply my deficiencies as well as I can: and if you see the matter clearly, please to give me your advice: and if you do not see the matter clearly, *do not trouble yourself to write*, but wait till we can meet.

1. The pendulum carrying the magnet is to vibrate through an arc of $2\frac{3}{4}$ inches, or thereabouts.

2. Through the whole of this arc, the magnet pole is to be (a) attracted to, or (b) repelled from, the coil which is below. In the case (a), the pendulum's vibrations will be made quicker; in the case (b) they will be made slower.



3. The dotted lines in the figure shew the extreme positions of the pendulum. The barbed lines shew the direction of force required in different parts of the vibration.

4. The case (a) is to be produced by turning on the battery in one direction, and the case (b) by turning on the battery in the opposite direction.

5. I expressly exclude iron core, because there would, when no battery is in action, be an attraction between the magnet and the iron core independent of battery, at times when I demand to have no attraction at all.

6. So the questions now are, Is the coil the most advantageous form of employment of current?

7. And if so, what shape and dimensions may be best?

I am, my dear Sir | Yours very truly | G.B. Airy

Professor Faraday

1. Letter 3627.

Letter 3631**William Thomson to Faraday****19 August 1859****From the original in IET MS SC 2**

Invercloy, Arran by Ardrossan | 19 Aug 1859

My dear Sir,

Is there any possibility that you will be present at the approaching meeting of the British Association? I write to say that Mr. James Crum, an uncle of my wife's has taken a house at Aberdeen for the time, and that it would be considered a great honor if you would join our party and take up your quarters with him. I fear that there is little chance of your not being otherwise engaged if you intend to be present, but it would be so great a pleasure to have you along with us that I would not like to lose the chance, small as it may be[.]

Believe me | Yours very truly | William Thomson
Professor Faraday

Letter 3632**Faraday to George Biddell Airy****20 August 1859****From the original in RGO6/629, f. 452-3**

[Royal Institution embossed letterhead] | 20 Aug 1859

My dear Sir,

I should begin the experimental trials with a solid bobbin because by lowering that you would obtain the general distribution of force supplied by a hollow cylindrical helix whereas no elevation of the latter would give you the same intense central force of a near bobbin¹. The size & force of the bobbin must have relation to the magnet in the pendulum bob if that were powerful the bobbin might be small perhaps an inch in diameter & 2 inches long or if the magnet were small the bobbin might be larger[.] The magnet should be very hard & not I think nearly saturated or the continual alternations of the power to which it would be subject might leave it permanently. I think I should make the bobbin of rather large wire using a large-plated battery to give quantity.

And so I guess at a first set of conditions not knowing without trial what would be the amount of effect on a pendulum of a given weight & a given magnet or in which direction any one of these conditions would have to change to produce the desired effect.

Ever My dear Sir | Very Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. See letter 3630.

Letter 3633

Faraday to George Herbert

20 August 1859

From the original copy in GL MS 30108/3/90.26

[Royal Institution embossed letterhead],

Royal Institution | 20 Aug 1859

My dear Sir,

You will see at the close of a letter of mine to Mr. Berthon date of 20 Aug 1858¹ that I think it is the Inventor that should say what he *can* or *wants* to do and that he should of himself work out all the data which he or the subject may require before he asks the Trinity house for a trial. It is not the place of the Trinity house to develop the propositions that come before it but to examine, check & criticize them. I have said this fully in my letter of the dates of 21 July² and 20 Aug 1858 to Mr. Berthon – and mine of 16 Aug 1858 to Major Fitzmaurice³. I have seen the light at Major Fitzmaurices own house under I presume every advantage and seeing it again in the same way would give me no further information. In the letter already referred to I have said “that the proof (required by the TH) must in its nature be complete; the mere exhibition of the light in a parabolic reflector is in my eyes of no lighthouse value”_[.]

Major Fitzmaurice in a letter of the 12 Aug 1858. to the Trinity house (of which I have no copy) says that his lights can be *practically applied as central lights for dioptric arrangements*_[.] I said in my letter of the 20 Aug to the TH that this is the proper form for comparison with the Fresnel lamp &c_[.] Such a trial must be made, before any useful opinion could be formed & it could only be made properly at such a place as the Upper South Foreland lighthouse where the light could be compared at sea with the lower light house as a Standard – could be carried on for a few months – and finally where the real liabilities & experiences of the plan could be ascertained and not left as they now are matters of mere expectation or supposition_[.]

It seems to me as if the enquiries I made in former letters for data &c have been forgotten for I have never obtained the data_[.] It is a weariness to look back for M.S. letters so if they should require to be referred to again & again I think I would ask that they should be set up in print and a score or two of copies struck off.

The season is just over & I am going off to Scotland next week for a month or so during which time I am sure the Deputy Master⁴ & Brethren will excuse my absence_[.]

Ever My dear Sir | Very Truly Yours | M. Faraday
Geo. Herbert Esqr | &c &c &c

1. Letter 3505.

2. Letter 3484.

3. Letter 3504.
4. Robert Gordon.

Letter 3634**George Biddell Airy to Faraday****23 August 1859****From the original press copy in RGO6/629, f. 454**

23 Aug 1859

My dear Sir,

Thank you for the very clear instructions in your letter of 20th¹, which will be taken by me as basis of my operation for adjustment of the pendulum of the Observatory Motor Clock – the Primary Clock of British Time.

The adjustment in question is not unimportant, for upon the successful management of it depends the possibility of extending the system of making the Primary Clock to regulate numerous clocks, which in succession will regulate more numerous clocks.

I am, my dear Sir | Yours very truly | G.B. Airy
Professor Faraday

1. Letter 3632.

Letter 3635**Faraday to William Thomson****25 August 1859****From the original in TNA PRO30/69/949**

The Green Hampton Court | 25 Aug 1859

My dear Sir,

My warmest thanks to you & Mr. Crum for your hospitality¹. I did not expect to be at Aberdeen² and yet the probability is that I shall be there for a few days. I shall be passing a couple of days with Sir James Clark³ on the Wednesday I expect to go into Aberdeen so as to be at the meetings on Wednesday & Thursday & on the Friday I must leave for home again. So if I am not abusing Mr. Crum's hospitality by taking two days only I will accept it and proceed according[ly] to enquire at the rooms for his address when I go in to Aberdeen[.]

Ever My Dear Sir | Yours Truly | M. Faraday
Wm. Thomson Esq | &c &c &c

1. See letter 3631.
2. For the meeting of the British Association.
3. See letter 3629.

Letter 3636**William Thomson to Faraday****27 August 1859****From the original in IET MS SC 2**

Invercloy Arran, by Ardrossan | 27 Aug 1859

My dear Sir,

I am very much pleased to hear that we shall see you at Aberdeen¹, and Mr. Crum, to whom I shall communicate your reply², will certainly feel much gratified by your acceptance of his invitation.

If you would be inclined to make a little detour on your way to Aberdeen and see the beauties of the Clyde, it would give Mrs. Thomson and myself great pleasure if you would remain with us for a few days here. We expect a visit from Helmholtz³ during the week preceding that of the association, and he would value highly the privilege of meeting you – I shall send you Mr. Crum's Aberdeen address as soon as I receive it.

Believe me | Yours very truly | William Thomson

P.S. I have set up an atmospheric electrometer here on a new plan & find curious results.

1. For the meeting of the British Association.

2. Letter 3635.

3. Hermann von Helmholtz (1821–1894, DSB). Professor of Physiology at Heidelberg, 1858–1871.

Letter 3637**Willem Vrolik¹ to Faraday****8 September 1859****From the copy in Rijksarchief in Noord-Holland, archief Koninklijk Instituut van Wetenschappen, 34/137**

Amsterdam ce | 8 Septembr. 1859

Mon Cher et très honoré Collègue,

A mon très grand regret, je vois par le N° de Septembre du museum de Mr. *B Quaritch*², que notre Collegue *Horsfield*³ est mort. – J'ai l'habitude de publier dans notre annuaire une notice biographique des membres, que nous avons le malheur de perdre⁴. Je voudrais la donner aussi de notre respectable Ami. Mais je suis dans une ignorance complète de tout ce qui le regarde plus spécialement Je Connais un peu ses ouvrages; mais toutes les dates me manquent: 1° Celui de Sa naissance, par conséquent son âge; 2° Sa position aux Indes; 3° Ses relations avec Sr.Th. Raffles⁵; 4° Ses fonctions auprès de la Compagnie des Indes; 5° les détails sur la jeunesse et ses études etc. – Probablement on aura publié quelque biographie de lui en Angleterre? Voudriez vous avoir l'extrême obligeance de me l'envoyer. Si vous avez d'autres détails à me donner, veuillez ne pas me les refuser. Votre position élevée vous met à même de les obtenir facilement.

D'avance je vous prie d'accepter l'assurance et de me croire avec les
 Sentiments les plus affectueux
 F. a. V. | Signé W. Vrolik
 A | Mr. M. Faraday | a Londres

TRANSLATION

Amsterdam | 8 Sep 1859

My dear and most honourable colleague,

To my great regret, I see from the September issue of the *Museum* of Mr. B. Quaritch's², that our colleague Horsefield³ has died. I usually publish in our annual a biographical note on the members that we have had the misfortune to lose⁴. I would like to include one on our respectable friend. But I am completely ignorant of everything more particular about him. I know his work a little; but I lack all the dates: 1st His date of birth and thus his age; 2nd His position in the Indies; 3rd His relationship with Sir Th. Raffles⁵; 4th His role with the Indies Company; 5th Details of his youth and studies etc.—Perhaps his biography has been published in England? Would you have the extreme kindness of sending it to me? If you have any other details to give me, please do not refuse them. Your elevated position gives you the means to obtain these easily.

I ask you in advance to accept the assurance and to believe me, with the most affectionate sentiments

Your | Signed W. Vrolik
 To | Mr. M. Faraday | London

1. Willem Vrolik (1801–1863, NNBW). Dutch physiologist and Secretary of Royal Institute of Sciences, Amsterdam.
2. Bernard Alexander Christian Quaritch (1819–1899, ODNB). Bookseller and publisher. The *Museum* was Quaritch's catalogue of books; a copy of this particular issue has not been located.
3. Thomas Horsefield (1773–1859, B1). Keeper of the Museum, India House, 1820–1859.
4. This seems not to have been published.
5. Thomas Stamford Bingley Raffles (1781–1826, ODNB): Colonial governor in service of the East India Company.

Letter 3638

William Thomson to Faraday

9 September 1859

From the original in IET MS SC 2

Invercloy, Arran | 9 Sep 1859

My dear Sir,

I enclose a letter I have received from Dr. Fleming¹, our professor of Moral Philosophy, in answer to petitions regarding the correspondence between Leibnitz² and Dr. Samuel Clarke³.

Mr. Crum's address in Aberdeen is

Mrs. Gray's⁴ | 15 Union Place | Aberdeen

I go there along with Mrs. Thomson, on Monday⁵, and we shall hope to see you on your arrival in Aberdeen⁶.

Believe me | Yours very truly | William Thomson

Professor Faraday

1. William Fleming (1791–1866, B5). Professor of Moral Philosophy at the University of Glasgow, 1839–1866.
2. Gottfried Wilhelm Leibnitz (1646–1716, DSB). German mathematician and philosopher.
3. Samuel Clarke (1675–1729, ODNB). Theologian and philosopher.
4. Margaret Jane Gray, née Brown (d.1883, age 67, SRO). Widow of David Gray (d.1856, age 45, B1), Professor of Natural Philosophy, Marischal College, Aberdeen, 1845–1856.
5. That is 12 September 1859.
6. For the meeting of the British Association.

Letter 3639

Faraday to John Tyndall

10 September 1859¹

From the typescript in RI MS JT TS Volume 12, p. 4157

Dundee Saturday

My dear Tyndall,

I heard of the offer to you on Loch Lomond². I hope you will accept it & have no time to say more or I shall lose the post[.]

Ever yours | M. Faraday

1. Dated on the basis of the reference to Loch Lomond.
2. This was for the post of Professor of Physics at the School of Mines, Jermyn Street (Eve and Creasey (1945), 81). Faraday had heard of the offer from Huxley when they met on Loch Lomond on 4 September 1859. Huxley to Tyndall, 5 September 1859, RI MS JT/1/TYP/9, pp. 2893–4.

Letter 3640

William Thomson to Faraday

13 September 1859

From the original in IET MS SC 2

Royal Hotel, Aberdeen | 13 Sep 1859

My dear Sir,

We only heard of your being in Aberdeen¹is afternoon, and Mr. Crum hopes you will take up your quarters with him as long as you are in Aberdeen. He regrets that he did not know of your arrival last night. I wrote from Arran last week² but could not get my letter off because of the weather, and I have carried it about me trying to learn your address. I leave it along with this for you.

We dine at Mr. Crum's, 15 Union Place (Mrs. Gray's³) at 6 today, and shall hope to see you at dinner. We shall be at home the whole evening and

if this does not reach you in time for dinner I hope you will come as soon as you are disengaged. Your room is ready at Mr. Crum's.

Believe me | Yours very truly | W. Thomson

1. For the meeting of the British Association.

2. Letter 3638.

3. Margaret Jane Gray, née Brown (d.1883, age 67, SRO). Widow of David Gray (d.1856, age 45, B1), Professor of Natural Philosophy, Marischal College, Aberdeen, 1845–1856.

Letter 3641

Faraday to Caroline Deacon

20 September 1859

From the original in RI MS F1 N/1/32

Hampton Court Green | 20 Sep 1859

My dear friend & niece,

Let our heart-felt congratulations be the acknowledgement of your hopeful letter to me. Thanks be unto God who hath fulfilled your hopes and who is able to keep you & yours & us in the full assurance of hope unto the end. See what was his purpose:– his chastenings are as merciful as his blessings;– for they are blessings. My wife and I join You and Thomas in love, we hope for the truths sake. I cannot use many words, as if this blessing were a common thing:– it is the Lords work and we may be encouraged to use the concluding words of the 103rd Psalm as saying all that can be said. Bless the Lord O my soul¹[.]

Your Affectionate Uncle | M. Faraday

Mrs. T. Deacon

1. Psalms 103: 22.

Letter 3642

Faraday and Sarah Faraday to Agnes Crum

20 September 1859

From the original in FSL MS Y.c. 889/1

Hampton Court Green | 20 Sep 1859

My dear Mrs. Crum,

Now that I have returned home and can look back on my visit to the North¹, there is no circumstance which comes back in my remembrance with more surprise: than the kindness which you and Mr. Crum shewed me. I very deeply thank you for it. Perhaps it was too much:– for it caused me to be disturbed in the leaving you, and gave me a divided mind during my return home. I am now settling again;– but I never can forget it, nor those who helped to make me happy at your house, by shewing and sharing in those acts of

kindness which are to me very dear. My wife is surprized at my account: and though she esteems me very much and loves me very dearly, wonders what others can see in me, at such short notice, as to make their attentions more than what is called for by respect for themselves. I have told her of your kind invitations for another time, both for her & me; and she believes them because I believe them:— but our hopes are not in the same proportion, for we doubt whether either of us shall ever see Scotland again. Our thanks are yours as much as if we had just spent a holiday with you.

Say what you can for me to Mr. Crum.— Say what you may for me to the group containing our vocalist;— not for the music only, but for all that passed:— and believe me to be My dear Mrs. Crum

Your deeply indebted & grateful | M. Faraday

My dear Madam,

I am embolden[ed] by my dear husbands account of all your kindness to add a few words to his letter, for I feel that he has said more for me than I can fully agree to, he would fain make me as modest as himself, but as regards him I must confess that is not the case, for I am *not surprised* at the kindness he has received though very grateful for it; I am sure you will not think me too vainglorious when I say, I believe he has the qualities which draw forth kindly feelings[.]

Trusting you will excuse this wife like epistle

I remain My dear Madam | Very sincerely yours | S. Faraday

1. From the meeting of the British Association.

Letter 3643

Faraday to William Crawford

20 September 1859

From a photocopy in RI MS

The Green Hampton Court | 20 Sep 1859

My dear Sir,

Amongst all the recollections that flow in upon my mind now that I have returned home, none is more pleasant than those which relate to our visit to you at Glasgow; and I owe very deep thanks to you and your dear wife¹ for the kindness shewed towards us. I scarcely knew who to write to in the expression of them but my wife is writing to yours. I write to you:— I cannot however separate you in this. Besides that, Your boys came for a large share in my remembrance & the way in which they made manifest their good will was most delicate & therefore the more acceptable.

I had a pleasant time at Aberdeen² & became for two days the guest of Mr. & Mrs. James Crum – Mr. Walter Crum³ & his daughters⁴ joined us:— and we were all happy together. I found they had a house at Dunoon also – but it is

well I did not know them or that before: for it would only have interrupted the happiness we had.

Janie left me at Dundee with her two brothers yesterday week. This morning I have had a letter from her from Dunkeld. She speaks of the Glasgow happiness with great pleasure. I hope Mrs. Crawford will tell our friends where we are i.e. if they feel any interest in our whereabouts. Our kindest love to her & Miss Crum[.]

Believe me My dear Mr. Crawford | Your grateful friend | M. Faraday

1. Janet Crawford, née Cree (d.1892, age 80, SRO).

2. At the meeting of the British Association.

3. Walter Crum (1796–1867, B1). Glasgow chemist and manufacturer. William Thomson's father-in-law.

4. Crum's daughters are listed in NRA 41114.

Letter 3644

Faraday to the Editor of The Times

22 September 1859

From *The Times*, 24 September 1859, p. 9, col. d

Royal Institution | 22 Sep

Sir,

The Trinity-house, in its care for the health of the people engaged under it in the superintendence of lighthouses, has at different times sent to me, as its scientific adviser, certain specimens of waters, which were supposed to be injurious to the persons using them. Lighthouses are, of necessity, often placed in situations where water is obtained with difficulty, and they are frequently dependent more or less, upon that which is gathered from rain falling upon the leaden roofs, galleries, and gutters of the towers and cottages occupied by the keepers. Now, the salt of the sea spray, which often reaches these roofs, &c., even when they are half a mile or more from the shore, causes the rain water which falls upon them to dissolve a portion of the lead, which is larger or smaller under different circumstances, and at times rises up to a quantity injurious to health, and poisonous¹. The water thus contaminated by lead, or rather chloride of lead, is peculiar in this, that it does not lose the poisoning substance either by boiling or by exposure to air, for the metal remains soluble after one or both of these processes. I ascertained that if a little whiting, or pulverized chalk (carbonate of lime), were added to such water, and the whole shaken or stirred together, the lead immediately assumed the insoluble state; so that when the water was either filtered or left to settle the clear fluid was obtained in a perfectly pure and salubrious condition. The process of purification is, therefore, exceedingly simple, for if some powdered chalk or whiting is put into the cistern in which such rain water is collected, and stirred up occasionally after rain, the water may, with the greatest facility, be obtained in a perfectly fit state for all culinary and domestic purposes.

The Trinity-house has supplied this information to all the cases needing it which have come to its knowledge, but I find that some cases occur not under its charge, that there are others not connected with lighthouses, and others again in other countries, in all of which this piece of simple practical knowledge may be useful. Under these circumstances I have thought that you, Sir, would not refuse the service of that special and extensive power of publication and instruction which *The Times* possesses, but use it to carry this knowledge to the many dispersed persons who may greatly need and yet have no other means of obtaining it.

I am, Sir, your obliged and faithful servant | M. Faraday

1. See letters 3562, 3565 and 3566.

Letter 3645

Faraday to Christian Friedrich Schoenbein

23 September 1859

From the original in UB MS NS 450

Royal Institution | 23 Sep 1859

My dear Schoenbein,

The state to which you consider that grief has reduced you, must, I think, be mine by course of nature & years; for I am just as you describe, weary, unwilling to write, and have nothing to say, really nothing to say; or else, surely I should have written sooner to you. Yesterday was my birth day & I then completed my 68th Year_[.] Well! many men are at that time of life far stronger than I am, either in body, memory, or mind; but surely I ought to remember how many pass away *before* that age,— and how plentiful & wonderful have been the mercies & goodness I have enjoyed during this long series of years.— Indeed, I think it is only when I have to fulfil some expectation, as in giving a discourse, or writing to a friend like you, that I wish my powers were more than they are:— and yet the very wish is ungrateful and brings to my mind a reproach.—

I was very glad to hear of you, and I hope the journey you were about to undertake to fetch your daughters home, with the intended little episode in the Black forest¹, will have done you all good. I have just had a little piece of enjoyment amongst fine scenery, for I have been in Scotland for a fortnight, & passed a few days among the lochs & mountains in the western parts.— I have also been *two* days at the British Association at Aberdeen; but was glad to leave it quickly and before the visits to Balmoral came on²:— for pleasant & happy as the occasions are they are by their excitement a weariness to me:— yet I was for the 48 hours with very kind friends.— The whole matter would have suited you better than me_[.]

Our friend Miss Hornblower continues in great pain; & I think we may consider the operation as a failure. Certainly it has failed to give the relief that

was hoped for.– She cannot move without crutches, nor without great pain.– My wife & niece are pretty well:– the former desires her kindest remembrance to you,– the latter is still in Scotland[.]

Very many thanks for your scientific news – I see you will carry oxygen much farther yet, and expect, with every letter, some new point:– As for me I am barren;– the best I have are some negative results about Electricity heat & gravity³.

Good bye My dear Schoenbein | Ever faithfully Yours | M. Faraday

1. See letter 3619.

2. The visit by two hundred members of the British Association to Balmoral took place on 15 September 1859. *Athenaeum*, 24 September 1859, p. 399.

3. Faraday had worked on this subject during the first half of 1859. See Faraday, *Diary*, 10 February to 9 July 1859, 7: 15785–15998.

Letter 3646

Faraday to William Henry Sykes¹

24 September 1859

From the original in WIHM MS FALF

[Royal Institution embossed letterhead],
Albemarle St. W | 24 Sep 1859

My dear Coll Sykes,

You will see by the enclosed² that Vrolik³ needs some data about Dr. Horsefield⁴. Can you help me to refer him to proper sources – I have no knowledge of my own that can be useful – but if you know of any biographical account that would seem to be what he requires[.]

Ever Truly Yours | M. Faraday

1. William Henry Sykes (1790–1872, ODNB). Member of the Board of Directors of the East India Company.

2. Letter 3637.

3. Willem Vrolik (1801–1863, NNBW). Dutch physiologist and Secretary of Royal Institute of Sciences, Amsterdam.

4. Thomas Horsefield (1773–1859, B1). Keeper of the Museum, India House, 1820–1859.

Letter 3647

Faraday to Henry Bence Jones

1 October 1859

From the original in RI MS F1 E20

R Institution | 1 Oct 1859

My dear friend,

I hear you think of coming to see us at Hampton Court on Monday¹. We shall be there by 1 o'clk or soon after & very glad to see you. Jeannie is still in

Scotland moving about & I expect will come home better for it. She longs to come back & we long to have her. I hope you will give us a good account of Lady Millicent on Monday[.]

Ever Yours Truly | M. Faraday
Dr. B. Jones | &c &c

1. That is 3 October 1859.

Letter 3648

Peter Henry Berthon to Faraday

4 October 1859

From the original in GL MS 30108/3/98

Trinity House, London, EC | 4 Oct 1859

Sir,

I am directed to transmit to you the enclosed Copy of a Letter addressed by this Board to Messrs Chance, Brothers, & Co. of Birmingham, relative to the modifications proposed by them, suitable to a Second Order Revolving Light with half-minute intervals consisting of 8 Holophotal Lenses, and their reply thereto, together with the accompanying drawing,— and to request that you will favor the Elder Brethren with your opinion thereon¹.—

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday | &c &c &c

1. This was for the Trincomalee light. See Trinity House Wardens Committee, 4 October 1859, GL MS 30025/27, p. 178.

Letter 3649

Faraday to Chance Brothers

6 October 1859

From the original copy in GL MS 30108/3/98

Royal Institution | 6 Oct 1859

Gentlemen,

I have received from the Trinity House letters from you, and a drawing, respecting a second order revolving light¹; they refer to a proposal by you of *certain modifications* &c, mentioned particularly in your letter of the 29th September last. I do not feel as if I saw all your reasons for the proposed modifications & therefore write to ask you. The first modification consists in placing 16 upper & 6 lower catadioptric holophotal prisms in the space occupied by 12 upper & 5 lower prisms in the old system. What is the improvement expected here? Is it in the greater accuracy of workmanship?— or the greater strength of construction?— or the greater proportion of rays gathered in?— or in any other effect?—

Then the second modification seems to be to construct the apparatus with 12 annular half lenses & 12 pannels of prisms – instead of 6 or 8.– I am not sure that I understand what a *half* lens means; but suppose it means a narrow one – one with the two sides cut away– You seem to consider 12 segments the best number for a light revolving at half minute intervals but as I understand you only in relation to speed of motion. An apparatus with six pannels would have to revolve once in three minutes for such intervals:– whilst with the 12 pannels it would revolve once in six minutes. Does the expected improvement consist in this difference in the velocity of motion? or is there any *other* effect that is contemplated, because of the difference in the size of the lenses?

Is there any other modification of effect beyond these two which I have to consider before I reply to the Trinity Board?

I am Gentlemen | Your Very faithful Servant | M. Faraday
Messrs Chance Brothers & Co | &c &c &c

1. See letter 3648, relating to the Trincomalee light.

Letter 3650

Peter Henry Berthon to Faraday

6 October 1859

From the original in GL MS 30108/3/99

Trinity House, London, E.C | 6 Oct 1859

Sir,

Referring to my personal communication with you on the subject of the proposed Visit of the Elder Brethren to Calais,– I am directed to acquaint you that it has been determined to accept the Invitation of the French Light House Authorities of which I apprized you, and to proceed to that place as invited, on the 14th Instant;– and I am to express the hope of the Elder Brethren that it will suit your convenience to accompany them on that occasion¹

In the mean time I am to forward, for your perusal and consideration, a copy of Notes upon Experiments, which have been made by the Light House Authorities in France, as to the application of moulded Lenses for the purposes of Light House Illumination.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq | &c &c &c | Royal Institution.

P.S. I shall be obliged by your returning me the enclosed Notes as soon as convenient. You shall have them again if you wish it.

1. For this see Trinity House Court, 4 October 1859, GL MS 30004/28, p. 15.

Letter 3651**Faraday to John Fox Burgoyne¹****7 October 1859****From [Burgoyne] (1860), 47–8**

Royal Institution | 7 Oct 1859

My dear Sir John,

I consider your request relating to the leaded water an honour, and in replying may add an observation or two to the original matter². The case at first was simply that of certain waters, which, having been collected from rain by roofs, gutters, pipes, or cisterns of lead, were contaminated more or less with the metal. All water so obtained has not been found thus affected, and there is much difference and uncertainty about the mutual action of lead and water in different cases. When rain water falls upon surfaces of lead it is apt to act on them, and the water thus contaminated, by standing exposed to air, generally clears itself from the dissolved lead, the metal separating as a carbonated precipitate, and falling to the bottom. But when the sea-spray has access to the leaded surfaces, the action of the rain water is such that the dissolved lead does not separate in this way, or if it does, only after a much longer time. It is such water as this that I recommend to be treated with carbonate of lime. Enough whitening or levigated chalk is to be mixed with the fluid to make it of the consistency of good milk (though more will do no harm), and the whole is either to be filtered or to stand until clear. I have never yet found any sample of water poisoned as above that was not freed from the lead by this process; and from the actions that occur in the laboratory I have no doubt that if two or three pounds of such powdered chalk were put into a cistern, and stirred up occasionally after rain, it would keep the water free from lead.

Now my consideration was entirely confined to cases of the above kind, and to the service of the Trinity House. I might say much more to you about the modes of testing for lead in water, so as to discover its presence, and, within certain limits, its proportion, and also about the clearance of lead from all domestic waters by filtration or otherwise, but I have always found that chemical practice was required to make such knowledge available, and that for that reason it was nearly useless in the hands of the public. When, too, a particular case becomes mixed up with the numerous cases that may be associated with it, I think it often disappears from view, and the whole are after a time forgotten. Hence I prefer adhering to the case of adulteration arising from the joint action of salt water, or sea spray, and lead; and I have the full confidence that if it arise at any of your military posts at home or abroad, no difficulty will be found in the effective application of the remedy.

I am | My dear Sir John | Your very faithful servant | M. Faraday
To Sir J.F. Burgoyne | &c &c &c

1. John Fox Burgoyne (1782–1871, ODNB). Inspector General of Fortifications, 1845–1868.
2. See letter 3644.

Letter 3652**Armand Masselin to Faraday****7 October 1859****From the original in GL MS 30108/3/98**

Glass Works near Birmingham | 7 Oct 1859

Dear Sir,

We beg to acknowledge receipt of your letter¹ concerning the proposed modifications we had lately mentioned in our letter to the Trinity House & will now answer your questions in the order you put them[.]

As regards the 16 upper & 6 Lower Catadioptric prisms forming the new Sections instead of the 12 upper & 5 Lower forming the old ones

“What is the Improvement Expected here[.] Is it in the greater accuracy of workmanship or the greater proportion of rays gathered in or in any other effect[.]” With a given unavoidable & practical inaccuracy of workmanship in any of the Surfaces or Curves, the Error becomes of a much lesser Consequence as the Size of prisms decrease because the Curves & the straight lines approach nearer[.] There is also a greater diminution of Substance of glass & therefore a lesser absorption of light & consequently also diminution of Color. There is little or no difference in the proportion of rays of Light gathered in & the Strength of Construction of the apparatus is in no way whatever affected by the modifications.

By annular half Lenses we mean lenses having only about half the breadth of the Lenses of an apparatus mounted with 6 Sides[.] These lenses are Mounted in the same way, but in a narrower frame & Contain the Same number of rings, only the outer ones are Cut vertically[.]

Referring now to the number of lenses in the apparatus & to the speed of revolution We beg to State that,

“Then the second modification seems to be to construct the apparatus with 12 annular $\frac{1}{2}$ Lenses & 12 panels of prisms instead of 6 or 8. You seem to consider 12 segments the best number for a light revolving at half minute intervals, but as I understand you only in relation to speed of motion Does the Expected improvement Consist in this difference in the Velocity of motion or is there any other effect that is contemplated because of the difference in the Size of the lenses.” A certain given ratio or speed of revolution is Supposed to be the most desirable & in order to increase flashes we prefer to increase the number of faces rather than the speed, or in other words prefer to decrease the total absolute power of each flash rather than to decrease the duration of the flash in relation to the eye[.] We must also beg to remark that the Same Size of Lamps being used, the divergence of the lenses will be the same in both the 12 & 6 sided arrangements only that with a 12 sided apparatus the

duration of the flashes will be increased whilst the duration of the eclipse is shortened to the same extent. An apparatus with 8 sides could be made as well as an apparatus with 6 or 12 sides, but the 6 & the 12 are the usual divisions practically used_[.] We beg to enclose in this letter 2 tracings giving the Sections of the apparatus the old 12 & 5 Sections as well as our new 16 & 6 Sections as recently Calculated by our Mr. James T. Chance, which Sections may assist you in your decision_[.]

We may also add that these Sections with increased numbers of prisms are against our interests as manufacturers as the expense of Constructing them increase nearly in proportion to the number of prisms, but we felt bound to give to the Trinity House all Suggestions in our Power_[.]

Of Late years the first order Lights have been nearly all Constructed upon the new Sections Containing 18 & 8 prisms instead of 13 & 6_[.]

We Remain Dear Sir | Yours faithfully | Chance Bros. & Co | Per A. Masselin
Professor Faraday &c &c.

1. Letter 3649, relating to the Trincomalee light.

Letter 3653

Faraday to Agnes Crum

8 October 1859

From the original in RI MS F1 N/1/49

[Royal Institution embossed letterhead],
London W | 8 Oct 1859

My dear Mrs. Crum,

Will you accept a letter from me on the part of my wife for she is not very strong this morning and I cannot consent to delay for 24 hours acknowledging the great pleasure you have given us by the Portraits¹. They are excellent though I have not yet put them into the Stereoscope being in reality at Hampton Court but I am on the point of carrying them to London. They remind me of a great deal of pleasure & how much I have to thank you for it. As for my return of it that is not likely for such events cannot be arranged with a view to an expected end. Still the past is by memory always ours & though I forget much I shall not forget Aberdeen².

My wife joins me in hearty thanks for your note & the portraits and in kindest remembrance to you & Mr. Crum_[.]

Ever faithfully Yours | M. Faraday

1. Not found.

2. Where Faraday had attended the annual meeting of the British Association.

Letter 3654**Armand Masselin to Faraday****8 October 1859****From the original in GL MS 30108/3/98**

Glass Works near Birmingham | 8 Oct 1859

Dear Sir,

We forwarded to you yesterday in our letter¹ in answer to your questions concerning Second order Lights, two tracings of the Sections of the 12 & 5 & 16 & 6 Lights². These tracings were the only ones we had by us then, but it occurred to us since that you might have to deliver in the hands of Trinity House these 2 tracings & that from there they might accidentally happen to fall in the hands of our foreign Competitors – as the 16 & 6 Prisms Section recently Calculated here by Mr. James T. Chance may justly be considered our private property we should prefer that this Section with the Complete measurements should not be delivered to the Trinity House. On Monday³ therefore we will send you two similar tracings but with only a portion of the measurements.

If however you felt it absolutely necessary that the drawing Complete should be delivered to the Trinity House, of course we should submit to it[.]

We Remain Dear Sir | Yours faithfully | Chance Bros & Co | Per.
A. Masselin
Professor Faraday &c &c.

1. Letter 3652.

2. Letter 3649.

3. That is 10 October 1859. See letter 3655.

Letter 3655**James Kenward¹ to Faraday****10 October 1859****From the original in GL MS 30108/3/98**

Glass Works near Birmingham | 10 Oct 1859

Dear Sir,

We now have the pleasure of sending you the accompanying drawing of the comparative sections of a second order Light, which please to consider as for the use of the Trinity House – retaining the former drawings for your own use².

Hoping that we have expressed the character and appearance of the improved second order Light in a satisfactory & intelligible manner

We are yours faithfully | for Chance Bros. & Co | J. Kenward
Professor Faraday

1. James Kenward (d.1906, age 77, GRO). Chance Bros employee, writer on lighthouses and antiquarian.
2. See letter 3654.

Letter 3656

Faraday to Peter Henry Berthon

10 October 1859

From the original copy in GL MS 30108/3/98

Royal Institution | 10 Oct 1859

Sir,

Referring to the copy of a letter to Messrs Chance which I sent to you on the 8th instant¹ and which had reference to your communication of the 4th² to me have received replies from M.M. Chance³ and I am now in a condition to answer your enquiries. The *first modification* proposed consists in an increase of the *number* of the catadioptric prisms each prism being made smaller than before. The increase of number is considered favourable because the planes of the more numerous prisms approach nearer to the form of the curve of a perfect optical apparatus than the planes of a lesser number of larger prisms do. Considering the size of the flame & the divergence required by the light house service I am not aware that this is of much importance but *no harm* can be produced by it. Another effect of *increased number* is that the rays would have to travel through a lesser thickness of glass and that, therefore, less light would be lost by absorption. This is undoubtedly true and in that respect the change would be of advantage. No other point regarding the *number* arises, either as to strength, accuracy, or fitting; the change would be rather disadvantageous to the maker as there would be more faces to work. Messrs Chance tell me that of late years the first order lights have been nearly all constructed upon the new section containing 18 & 8 prisms instead of 13 and 6.

The *second modification* is to construct the apparatus with 12 faces to revolve in six minutes, instead of with 6 faces to revolve in three minutes;—the intention being to have half minute intervals. I do not find that there is any ground of objection to a revolution of the whole apparatus in either three or six minutes, independent of its effect on the light: and if the light is to be limited to half minute intervals, with either 12 or 6 faces, then the effect will be as follows. The flash from 12 faces will have only half (or nearly half) the intensity of that from 6 faces, but it will last twice as long; therefore the choice is between a quick strong flash with a long interval of darkness, & a slower weaker flash with a shorter interval of darkness. As to the nautical value of these two results I am not a practical or proper judge; though I can understand that the increased brightness in the one case is an advantage; and that the increased duration in the other may also be a benefit at times. Of course half minute time may be given by apparatus with either 6. 8. 10 or 12 faces. I conclude there are revolving lights in existence by which the

question of time may in some degree be judged of – i.e where a face passes though 30° or 60° of revolution in one interval but I must refer that point to the decision of the Elder Brethren.

I return the plan & letters which you sent me⁴[.]

I have the honour to be | Sir | Your Very Obedient faithful Servant |
M. Faraday
P.H. Berthon Esqr. | &c &c &c

1. Letter 3649.

2. Letter 3648.

3. Letter 3652.

4. This letter, which refers to the Trincomalee light, was read to the Trinity House Wardens Committee, 11 October 1859, GL MS 30025/27, pp. 188–9. A decision was deferred. At Trinity House Wardens Committee, 18 October 1859, GL MS 30025/27, p. 197 it was agreed to implement Chance's proposal with minor modifications.

Letter 3657

Faraday to Armand Masselin

11 October 1859

From the original in UU EW

[Royal Institution embossed letterhead],
Albemarle St. W | 11 Oct 1859

Dear Sir,

I have just received yours of the 8th¹ and send you both the drawings back at once. You must excuse my marks on them[.]

Ever Truly Yours | M. Faraday
A. Masselin Esqr | &c &c &c

1. Letter 3654.

Letter 3658

Charles William Pasley to Faraday

17 October 1859

From the original in IET MS SC 2

High Elms, Hampton Court, SW |
Monday Morning the 17 Oct 1859

My dear Sir,

I should like to have a few minutes conversation with you on a subject very interesting to me, and of some importance to the Service of the Country.

It relates to Pontoons, which have always been my hobby horse, as you may perhaps recollect. I proposed decked copper canoes shaped like boats

and the late General Blanshard¹ also of the Engineers proposed tin cylinders with paraboloidal ends. The midsections of the two are as below



When a heavy weight such as a Gun presses them down to within 4 inches of the surface of the water, Pasley's pontoon requires more than a Ton of additional weight to press it down to the water's edge, whilst Blanshard's from having so little capacity in the upper part would be in great danger of sinking, and after each heavy gun whether drawn by men or horses passes over any *one cylinder*, the moment that this is relieved of the weight there is a violent rebound of the superstructure in rear of the gun that flies up as if it were forced by a powerful spring. Thus a very dangerous oscillation or rather undulation of the roadway is occasioned, that has a tendency to frighten horses, and was the cause of an accident in the Pontoon Bridge formed over the Thames at Runnymede, when a 9 Pounder Gun of a field battery went over into the river with Drivers and Sappers employed as Pontoneers on the same side, because they cannot stand in safety on a cylindrical surface, so that they were obliged to jump in to save themselves. On this occasion 2 out of 6 horses (those in the shafts) were drowned the others saved by cutting the traces. The Men some of whom could not swim escaped. I should have mentioned before, that both sorts were sent out to Bombay for trial, by order of the Court of Directors of the East India Company in 1826 and were tried in a creek subject to the action of the Tides for a short time, in two or three years after which, Blanshards tin cylinders having become a mass of rust and dust were thrown away and mine were adopted as the standard Pontoon for India and are still established as such.

I now wish to bring them forward again to supersede the tin cylinders, and my object is to consult you about the properties of copper, iron and tin.—The latter has been proved to be destructible even in the brackish water of the Medway to guard against which, the men are obliged to carry the cylinders down to the river and gently deposit them on the surface of the water, instead of launching them like boats, and in landing them, on leaving off work, they lift them out with equal care, and in the meantime my Exercise has been adopted by Blanshard whilst his are afloat. Part of the men having mud boots for wading into the river manage this business of the launching and landing.

The others carry up or down the bank which may be steep. In fact whenever the paint is rubbed off the thin iron follows it and holes are eaten through it by the action of the water. Even on grass or on the smoothest fine sand, the same effect takes place, so that they must never touch the ground².

If you take a walk this morning will you do me the favor to call, or I will call on you at one o'clock P.M. which is our luncheon time, for which as you know I do not care, or only employ a quarter of an hour at most[.]

I remain, My dear Sir | Yours very faithfully | C.W. Pasley

1. Thomas Blanshard (d.1859 age 70, B1). Royal Engineer officer from 1807; general 1854.

2. For Pasley's work on pontoons see Playfair, I.S.O. [1930].

Letter 3659

Faraday to John Phillips¹

21 October 1859

From the original in UMO MS John Phillips Papers

[Royal Institution embossed letterhead],
Albemarle St. London W | 21 Oct 1859

My dear Phillips,

I have received your letter of the 15th September². I have several times considered, very carefully, the matter it refers to, namely the Humboldt testimonial³, and I cannot bring myself to think that it is a step in the right direction. Humboldt's memory cannot, according to my view, be honored by any act of the kind. A feeling of the highest and finest character belongs to the name; and in my opinion that feeling is lowered by the association of the name with any thing partaking of the character of a testimonial. No such act can, in any shape, do honor to Humboldt's memory; for that stands alone in its glory. On the contrary, the system has been abused so frequently of late, that I, for one, feel Humboldt's name would be hurt by association in any way with it.

As to the aid that may come to science by the means proposed, I do not think that any who may be willing to yield it, would not do so as freely for science's own sake, as for the sake of an oblique and posthumous association with the name of Humboldt. Indeed I cannot bring my mind to the conclusion that, Humboldt himself would, if he were in the flesh, approve of such a motive and manifestation.

Nevertheless, doubting my own judgment, and seeing how many appear to be in favour of the proceeding, of whose judgment and feeling I cannot but think most highly, I beg to fill up the paper you have sent me for the sum of £5; and enclose it in this letter. I hope you will bear with these remarks. I should not have felt true to you and myself if I had not made them[.]

Ever My dear Phillips | Most truly Yours | M. Faraday

1. John Phillips (1800–1874, ODNB). One of the founders of the British Association and its Assistant General Secretary until 1859.
2. Not found.
3. Humboldt had died on 6 May 1859. On the Humboldt testimonial, organised by Phillips, see Morrell (2005), 333.

Letter 3660

Faraday to Agnes Crum

22 October 1859

From the original in Glasgow City Archives MS TD 1073

[Royal Institution embossed letterhead] |

22 Oct 1859

My dear Mrs. Crum,

Help me to convey my thanks to those to whom I am indebted for the music sent me and make me still more than before

Yours Most truly | M. Faraday

[Royal Institution embossed letterhead] | 22 Oct 1859

The deepest thanks are hereby tendered by one, who having been greatly indebted for much free and gentle kindness shewn him at Aberdeen¹, has now the delight of having it continually called back, by the remembrances of the sweet melody which overcame his ear & mind. The music can recur; but the charm of that evening hour will not return. A feeling as fine may, as we hope, cheer our future lives; but *the* feeling of an occasion like that was from the circumstances and the manner in which they are used and cannot be repeated.

M. Faraday

1. At the meeting of the British Association.

Letter 3661

Faraday to Robert Gordon

24 October 1859

From the original copy in GL MS 30108/3/99

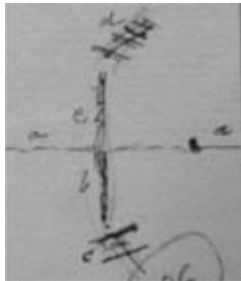
Royal Institution | 24 Oct 1859

To the Deputy Master | of the Trinity House | &c &c &c

You have done me the honour to request that I would communicate to you such observations as arose in my mind in relation the special points which came before us during the late visit to the coast of France¹ (i.e. to Calais, Cape Grisnez and Boulogne) these points being the pile light house at the Point du Walle;– the use of paraffine in a light house lamp;– and the effect of a sound bell placed in a reflector :– I will now endeavour to meet your desire.

The screw pile light house placed on the edge of the sands at the point du Walle is supplied with an apparatus which is nearly of the third order having a double wicked lamp burning Colza oil. The intention is to produce a continuous light characterized by varying from the red to white and white to red. Viewed at night from the Pier at Calais about 6 miles off it had this character; and as far as I could judge the intensity of the red light & the white light were about equal to the eye.

The manner in which this effect is obtained is as follows. Consider a horizontal plane *a* as passing through the focus or middle of the flame of the lamp; it will divide the light into two portions, the upper & the lower hemispheres, of which the lower will be the weaker because the burner interferes & cuts off part of the light proceeding towards the dioptric apparatus around. From this horizontal plane *downwards* the lamp is surrounded by fixed dioptric pannels *b* & below them by fixed catadioptric reflectors *c* which refract & reflect the light upwards and collect it into the horizontal plane; so that as far as that part of the apparatus is conceived the light is a fixed light all round. On the same fixed apparatus and above the usual place of the pannels are the catadioptric reflectors *d*, and these also add to the constant white light from the lower part for they also send their rays in a horizontal direction all round[.]



Thus the white light which is seen from a distance is obtained.

The part above the horizontal plane *a* & below the catadioptric reflectors *d* is diverted to the production of red light. Suppose a lens placed in the usual way before a lamp; it would collect all the rays falling on to it into a horizontal beam, and if fixed on to a revolving frame would give flashes:— But if, without disturbing the position of the lens, it were cut in half by the horizontal line *a* & the lower half removed, then only rays from the upper hemisphere would be collected in the flash & the lower rays would be left to go through the prism pannels *b*. If the lower half of the lens cut off were turned bottom upwards & placed above the horizontal line, it would act just as the half lens

left undisturbed; and there are, indeed, eight such half lenses *e* filling up the space round the circle and fixed on to a revolving frame so as to give eight flashes in one revolution of the apparatus.

If the apparatus were used in this state it would give a constant white light from the fixed part of the dioptric arrangement to which would be super added the flashes of white light from the moving lenses; but the object is to convert these flashes into red light and this is done in the following manner. The moving frame carries 24 panes of red glass. Eight of these are placed before the eight half lenses; they occupy the whole of the circle & convert what would be white flashes into red flashes. These red flashes would however be mingled with white light from the lower & upper fixed apparatus which would increase the light but diminish the red colour; So to convert this white light into red light, whilst the flash from the lenses is passing to the observer, a pane of red glass for each lens descends from the moving frame before the lower glass, and another ascends from it before the upper catadioptric reflectors; but their width is only enough to subtend an angle of 4° or 5° , by which however they convert the white constant light into red whilst the red lens is sending its rays to the observer thus adding red light from the fixed pannels to the red light from the moving lenses[.] So the alternate red & white lights are produced[.]

As the visible effect at Calais Pier of the two lights was to me nearly equal, it gives occasion to remark on the serious loss of light produced by any attempt to colour it effectually. In the present case when the white light appears it comes from parts of the apparatus which are equally open to view when the red light is on; but the narrow red panes are interposed and only the red light came through this being however greatly increased because at that moment the light from the lenses is brought in aid. As there are only eight lenses round the circle, & the ray from each may be considered as having a divergence of 7° or 8° (for the lenses are of moulded glass) it is probable that if all the red glass were away they would give conjointly with the light from the fixed apparatus a beam of white light 4 or 5 times as strong as that which appears in the white beam of the apparatus as it is now arranged; yet, being reddened by the interposed glass that beam was not at all to my view stronger in illumination[.]

I may say here that I understood M. Reynauds strong desire to establish a distinctive character by change in the colour of the light is greatly founded upon the tendency mariners have to confound alternating lights of different intervals with each other. Rotating lights with quarter & half minute intervals have been mistaken for those of half & whole minutes, & vice versa; the tendency is, to observe & distinguish a rotating light from a fixed light; but to neglect the *time*, & so lose the distinction between one & another.

All the glass of this apparatus has been *moulded* instead of being cut & wrought to shape by machinery. However the inner surface of the pieces & some of the larger surfaces of the outer part, have been wrought. The moulded glass did duty; but it could not be properly examined in its present position; & as I expect to have specimens in my hands for particular investigation, I will say no more on the matter at present.

Paraffine or the oil of schest is a fluid very rich in carbon; often applied in lamps economically; & yielding a very beautiful light if carefully burned. When carelessly burned it produces much smoke with a bad flame. It has been applied to the Harbour light at Boulogne and appears to answer there perfectly. The flame was very regular and, by the deflector, was expanded horizontally until its width was an inch & a half, whilst its vertical height was not more than half an inch. This shape is necessary for the perfect combustion, & is the best shape also for lighthouse use; a tall narrow flame being just as objectionable as the one described is advantageous. The lamp has but one wick & this substance cannot be burnt in lamps with two or more concentric wicks, the necessary deflector being then inapplicable to them. The substance may probably come into use for Harbour lights but scarcely for lights of a superior order.[1]

Fog bell. A bell weighing perhaps 3 cwt has been fixed up at the end of the western pier of Boulogne harbour. It is placed in a species of reflector consisting of Iron with a regular face of cement; & is furnished with three hammers & a mechanical apparatus by which it can be stuck in times of need. It was to have been sounded as we left the harbour; but in place of that, a smaller open bell was rung; & by reason of the wind & waves its sound was lost at a very small distance outside. Whilst experimenting on the pier head with the bell in the reflector, I found that there were great interferences in the strength & direction of the sound. The sound was best right in front;— passing right or left in a circle at the same distance from the bell, the sound fell & was a minimum when about 45° had been gone through. Going round still further it rose again in intensity until the edges of the reflector interfered, & then as one moved behind the bell, it fell to the lowest degree. It will require more & extended observation before any advantage the arrangement may possess can be tried & known.

Two *first order lights* came under our observation that at *Calais* and that at *Cape Grinez*. Each of these lights has a four wicked lamp and the oil is worked by pumps. I am satisfied that the fourth or central wick is an advantage. When the flame, which ought never to be more than $3\frac{1}{2}$ inches high & always without smoking tongues, is perfect this central wick gives as clear & definite a flame as the others. The draught of the chimneys is powerful. The cottons are thin. The oil overflows abundantly:— not more than one fourth of that which is raised is burned the rest returns to the cistern, but in overflowing,

preserves the cottons in excellent condition. The cottons in fact rise out of a burner of oil – The consumption of oil is from 750 10875gr to 800 grammes 12347gr (1.32 pints) per hour and that is well burned.

The light at Calais is a fixed light with the occurrence of a flash every four minutes_[.] It consists, first of all, of a fixed catadioptric apparatus which produces the constant light;– then outside of this, is a moveable frame, carrying three dioptric pannels, equidistant from each other, & which, as they refract in a horizontal plane, produce with the former apparatus the effect of lenses. This frame revolves in 12 minutes & this produces the final result. The motion is very slow & easy; but the arrangement, though it has no objection in principle, is not in favour, & is the only one in France. The Grinez light consists of 16 lenses which revolve; above & below there are the old captoptric [sic] reflectors which give a constant light. The lens frame revolves in 8 minutes, so that there is a flash every half minute; and the time of its duration is estimated at 8 seconds.

A consideration often occurs in respect of revolving lights, whether it is most important to make a strong quick flash or a weaker longer one? It is easy to see, that, if there were only 8 lenses in the Grinez circle & the whole revolved in 4 minutes, that there would be an equal number of flashes in the same time, i.e. one every half minute; and that the flash would be of double the strength for it would contain twice the light gathered by the half sized lens;– but, then, this greater flash would only endure 4 seconds instead of 8. Now the impression of M. Reynaud & the other authorities, is, that increase in *time* up to a certain amount is more important than increase in *brightness*; and that 8 seconds is the least time during which the flash ought to endure. So strong is this impression, that, where in some cases the reflectors above & below the lenses have been made holophotal, they have not been so placed as to add their power to that of the lenses so as to increase their flash in *brightness*, but are so arranged as to precede by a little the flash of the lenses, & so increase it in *duration*_[.] This is an important question which can only be settled by much observation_[.]

The South Foreland Electric light: As far as I heard & saw by the reports all the observers on the coast of France opposite report well of the power & constancy of the Electric light at this place_[.]

I do not think there is any other matter on which I need speak²_[.]

I have the honour to be | Sir | Your Very faithful humble Servant |
M. Faraday

1. See letter 3650.

2. This letter was read to Trinity House Court, 1 November 1859, GL MS 30004/28, p. 26 and it was ordered to be entered in the Book of Select Papers.

Letter 3662**Faraday to Henry Bence Jones****26 October 1859¹****From the original in RI MS F1 G13**

[Royal Institution embossed letterhead] | Wednesday 26 Oct

My dear Bence Jones,

I did think of calling on you; but went to Sir B. Brodies instead and as I am told to keep in I write *least you should hear of my matters otherwise* & not that they are serious. This morning I awoke without pain & found I was lying in blood which had issued from one or two small punctures or apertures in the scrotum, the scrotum itself being also black & charged with blood. Mr. Cutler² refers it to the breaking of a small blood vessel during my violent recent sea sickness. All is going on well now & there is no more effusion. I have only to take care. We have come home for good so all is right with us & I hope with you. I shall see Sir B. Brodie on Friday morning³ but expect there is nothing to do.

Ever Yours | M. Faraday

1. Dated on the basis of the reference to Faraday's sea sickness from his journey to inspect the French lighthouses (see letters 3650 and 3661) which is also mentioned in letter 3670.

2. Edward Cutler (1796–1874, Plarr (1930), 1: 312–3). Surgeon at St George's Hospital.

3. That is 28 October 1859.

Letter 3663**Christian Friedrich Schoenbein to Faraday****26 October 1859****From the original in UB MS NS 451**

Bâle | 26 Oct 1859

My dear Faraday,

My having spent the autumnal vacations partly at Neuchâtel, partly at Presinge the seat of our friend de la Rive on the frontiers of Savoy and returned to Basle but a few days ago, must excuse this late answer of mine to your last kind letter¹. Hard working had made such a temporary relaxation quite necessary to my mind. First of all, allow me to offer my most heartfelt congratulation to you on account of the celebration of your 68th birthday and let us hope, that it may please Him, who is the sovereign Lord of our life, to grant the return of many more. Generally speaking a long age is rather an equivocal gift and in the most favourable case accompaniaied with many evils, which human flesh is heir to², amongst which not the slightest one is

the feeling and consciousness that we have lost the bouyancy of Youth. But there are some privileged men, whose mind, in spite of carrying a heavy load of years upon their back, remain elastic and green, continuing to take the liveliest interest with every thing regarding the higher and nobler aims of Mankind. Either I am entirely mistaken, or you are such a man. May your body be a little broken down, your hairs have turned grey or white, your countenance be furrowed by wrinkles, perhaps even your walk and step somewhat tottering, what is that to you, who are still soaring in the highest regions of philosophy, whilst youngsters replete with bodily powers are crawling upon the lowest ground. A little more or less of memory, precious as this gift is, does not matter much and after all, according to what you have accomplished during your career of life, you are more than any other man entitled to enjoy the "*otium cum dignitate*"³. There is a german saying "*Fünfzig Jahre Stillestand, sechzig Jahr fängt's Alter an*" and according to it, your friend must now also be called an old man, having the eighteenth of this month accomplished his sixtieth year. It is certainly with some reluctance that I acknowledge myself to be a "*senex*", but my grey hairs give but too obvious an evidence in favor of the truth and I must submit to what I cannot change. Although far advanced in the career of life I nevertheless feel still rather youthly and have not yet lost to a perceptible degree my ancient love for science and philosophical research and that I consider as an invaluable boon received from him, who is the Giver of all good things, and as calculated to cheer up the evening of my life. Like you I have every reason to be most grateful to kind Providence for what fell to my lot, modest as it has been and not always made up of smiles and sunshine. But now enough of birthday reflections. During the summer gone by I have been rather active in my laboratory and trust my doings will not have been quite useless. Pray, listen now a little to my random talkings about philosophical matters. First of all know, that I continued to work upon what I have called "*the chemical polarization of neutral Oxygen*" of which subject I communicated you something in my last letter and from it you will recollect, that during the slow combustion of Phosphorus and Aether as well as the Electrolysis of water, both kinds of active Oxygen (\oplus and \ominus) make their appearance, the former in the shape of $\text{HO} + \oplus$.

Having these many years considered the said slow combustion of phosphorus as the type of all the slow oxidations, which inorganic and organic bodies undergo in the moist atmospheric air or pure common oxygen, I suspected, that the peroxide of hydrogen might be produced, if not in all (from secondary reasons) at least in a great number of cases and directed therefore my attention first to the slow oxidation of the more readily oxidable metallic bodies. My conjecture proved correct, having already found out that half a dozen of metals during their slow oxidation give rise to the formation of very

appreciable quantities of HO_2 , as you will perceive from the statements to follow. To ascertain with full certainty small quantities of that compound, I first wanted proper i.e. most delicate tests for HO_2 and I fully succeeded in finding out more than one of that description, in corroboration of which I may tell you, that by the means of them I am able to detect the millionth part of the said peroxide contained in water and even less than that. These tests depend upon the oxidizing and reducing effects produced by HO_2 upon certain substances. Dilute paste of starch containing some jodide of potassium, if it be mixt up with water containing but half a millionth of HO_2 is within a very short time colored dark blue on adding some drops of a weak dissolution of any protoxide salt of iron to the mixture. The dilute dissolution of HO_2 slightly acidulated by SO_3 discharges the red color of an acidulated dissolution of the permanganate of potash (by reducing the acid of that salt to the protoxide of manganese). HO_2 even in a most dilute state throws down prussian blue out of a mixture of most dilute dissolutions of the red cyanide of potassium and any peroxide salt of iron (by reducing Fe_2O_3 to FeO). Most dilute HO_2 colored blue by some Indigo solution is rapidly discolored on adding some drops of a dilute solution of iron vitriol to the mixture. A dilute solution of chromic acid is certainly a less delicate test for HO_2 , than the mentioned ones are, but its property of being colored azureblue by water containing but $\frac{1}{20000}$ of HO_2 makes it in many cases a valuable and practical test, which I always use when I have to deal with water somewhat rich in HO_2 . Now by the means of those tests I have of late ascertained that during the slow oxidation of Zinc, Cadmium, Lead, Tin, Bismuthum and Copper (effected by moist common oxygen or atmospheric air) perceptible quantities of HO_2 are always formed conjointly with the oxides of those metals. To produce HO_2 , some of the metals being in a state of mechanical division as Zinc, Cadmium and Lead, have but to be put in contact with pure water and atmospheric air for a very short time, but I find it more convenient to amalgamate first the metals with mercury. Take for instance 100 grammes of Zinc filings, and the same quantity of Mercury, put them into a tumbler filled with dilute sulphuric acid, stir up the metals by a glass rod and you will soon have a grossly powdered Amalgama. Now, after having that metallic mixture washed with water, put it loosely into a funnel, set upon a bottle, let a very thin vein of distilled water run over the amalgama, and by the means of dilute paste of starch containing Jodide of potassium, you will already detect peroxide of hydrogen in the water having passed (in the manner indicated) only once over the amalgama, if you add to a mixture of both some drops of a solution of Ironvitriol &c.— If you shake for a few seconds the said amalgama together with air and 100 grammes of distilled water, the latter will have the property of striking blue the paste of starch on adding to it a couple of drops of a dilute solution of any protoxide salt of iron. Water containing

1% of SO_3 , all circumstances being the same, produces more HO_2 than pure water does. You may satisfy yourself with one instance. Take 100 grammes of a still liquid amalgama of Lead, shake it with 100 grammes of the mentioned acidulated water and atmospheric air for 5–6 minutes, separate by filtering the sulfate formed from the water, add to the latter some drops of a dilute solution of Chromic acid and your liquid will be transiently turned azureblue, a proof of the presence of HO_2 . If you shake one volume of the said acidulated water, two volumes of pure Ether and some drops of a dilute solution of CrO_3 together, the ether assumes a still deeper blue color. The same acidulated water of course discharges the color of the permanganate solution &c. In saying so much about this matter, I must not omit to add that the quantity of HO_2 formed under the circumstances, reaches soon its maximum which does not go beyond $\frac{1}{6000}$ of the quantity of acidulated water employed. The reason of this fact is obvious. I shall not enter into any more details about the subject, hoping to find soon an opportunity for sending you a paper containing all the particulars about this highly interesting formation of HO_2 . From the facts above stated and others not mentioned I am led to conjecture, that all the slow oxidations taking place in the moist atmospheric air depend upon what I call “the chemical polarisation of neutral oxygen” i.e. that this act always precedes that of real oxidataion. The oxidable matter being eager to combine with \ominus and water with \oplus to produce $\text{HO} + \oplus$, determine that mysterious polarisation of O in a similar manner as HO_2 is sometimes decomposed, if placed between two substances, one of which attracts the oxygen, the other the hydrogen of the compound. But be that as it may, perfectly sure is now the fact, that in a number of cases of slow oxidation the counterpart or antipode of \ominus makes its appearance in the shape of $\text{HO} + \oplus$ and that the latter compound also acts an important part in those slow oxidations. I am inclined to suspect, that the chemical polarisation of O is deeply concerned in animal respiration and many other chemical actions going on in nature, but I will not yet talk about these matters. It seems that the late results of my researches tend to increase a little our insight in the workings of our chemical Hero, and you may therefore easily imagine, that I pursue my investigations on that really wonderful body with a zeal bordering upon mental excitement.

Mrs. Schoenbien and my children are, as to body, tolerably well, but the severe loss of our dearest Emilia still presses very heavily upon us all and most particularly upon the mind of my poor wife. That great physician Time has not yet healed much.

With the deepest regret I learn from you, that poor Miss Hornblower is far from having obtained the desired result from the painful operation she was obliged to undergo some months ago. Pray, remember me most kindly to her and express to the suffering Lady my fullest sympathy. It requires certainly

an uncommon degree of moral strength and before all a most absolute submission to the will of God, to maintain herself in a tolerable condition of mind and spirits. Before closing my long letter, I ask you the favor to present my kindest regards to Mrs. Faraday, your Niece and relations, who were so very kind to my beloved daughter who is now no more.

Pray, don't be too long in writing me and be assured, that every word coming from you is of the highest value to

Your | most attached friend | C.F. Schoenbein

Address: Doctor Michael Faraday | Royal Institution | Albemarle Street | London

1. Letter 3645.
2. A slight misquotation of William Shakespeare, *Hamlet*, III, 1, 65.
3. 'Leisure with dignity'.
4. Schoenbein (1860).

Letter 3664

Faraday to William Thomson

28 October 1859

From the original in BL add MS 48983, f. 3

[Royal Institution embossed letterhead], Albemarle St. W |
28 Oct 1859

My dear Thomson,

I have received by post a number of diagrams – apparently lines of magnetic force with transverse lines – perhaps lines of direction of motion – but there is no explanation of their particular object – nor any note with or about them – Are they from you? I have a vague notion that you spoke to me about some such thing but have forgotten all about it[.]

Is your delicate static electrometer manufactured yet? and if so what is the *price* of a most delicate one?– It ought to be visible at a distance. If we require one can you order its making?

I think I heard you speak of some other electrometer employed by Dellman¹ & another employed in the investigation of Ohm's² law – or am I mistaken[?]³

Ever Truly Yours | M. Faraday

1. Johann Friedrich Georg Dellmann (1805–1870, P1, 3). Professor of Mathematics at Kreuznach.
2. Georg Simon Ohm (1789–1854, DSB). German physicist.
3. Thomson (1856c, 1859, 1860a).

Letter 3665

William Thomson to Faraday

31 October 1859

From the original in NUL MS Misc 48

2 College, Glasgow | 31 Oct 1859

My dear Sir,

The diagrams which came to you by post¹ were from Professor Clerk Maxwell. He wished to speak of them to you at Aberdeen² but I suppose did not find an opportunity in the closely packed time.

The curves represent lines of force, or lines of motion of a fluid, for various cases in which their directions are all parallel to one plane. In every such case the set of curves which cut a set of lines of force perpendicularly are themselves a set of lines of force for another case; and therefore each of the diagrams bears a double interpretation. The best electrical application is to lines of atmospheric-electric force in the neighbourhood of a long straight mound, or mountain ridge, or trench, with a section of any form – In such a case you will see that, except near the ends of the line of elevation or depression, the lines of force will be all parallel to one plane, namely to any plane perpendicular to that line. Among Maxwell's diagrams you will find one, for instance, which shows the lines of electric force in the neighbourhood of a long semicylindrical mound. The surfaces which we mathematicians call surfaces of equal potential (after George Green's³ definition) will be shown in the diagram by the lines perpendicular to the lines of force. In the case of the semicylindrical mound, the surfaces of equal potential commence, at the earth,



with the plane & curved surface of the earth, & approximate to parallel planes, at greater & greater heights. The curves showing these surfaces, are [like] lines of motion of water round a long cylindrical bar carried in a direction perpendicular to its length.

I am now having a reflection electrometer made for Kew, to act as a self recording instrument for atmospheric observation. The same kind of instrument will I believe be the best for general experimental purposes in which an electrometer of the highest sensibility is required. It will also answer for lecture illustrations, but for elementary instruction perhaps one such as you saw at Aberdeen⁴ showing its whole action and construction, and distracting

the mind less from the electrical topic, may be preferred. I could have an instrument of either kind constructed for you if you please, and it would be a great pleasure to me to take charge of it should you wish to have one made. The last which I had made (much improved on the one you saw) cost £11. I do not think the appliances for reflection will add much to the cost, but I shall soon be able to have an exact estimate sent should you desire it.

Believe me | Yours very truly | William Thomson
Professor Faraday

Endorsed by Faraday: Thomson on Dellmans⁵ mode of observing electricity – British Association Aug 1856 p17 of notices | *Athenaeum* – No. 1505⁶

1. See letter 3664.
2. At the meeting of the British Association.
3. George Green (1793–1841, ODNB). Mathematician in Nottingham and then Cambridge.
4. See Thomson (1859, 1860a).
5. Johann Friedrich Georg Dellmann (1805–1870, P1, 3). Professor of Mathematics at Kreuznach.
6. Thomson (1856c) reported in *Athenaeum*, 30 August 1856, pp. 1091–2.

Letter 3666

William Hallowes Miller to Faraday

c.31 October 1859¹

From the original in RI MS Conybeare Album, f. 19

My dear Faraday

Some time since you asked me if I knew whether General Laval Count Nugent² whose name stands in the list of members of the Royal Institution was alive or not³.

It appears from an extract from Sir Bernard Burke's⁴ vicissitudes of families 2nd Edition⁵, quoted in the *Illustrated London News* of Oct 15, 1859 page 376⁶ that he was alive very recently – since the death of F.M. Radetzky⁷ – It is not a week since I inquired about him in a letter to Haidinger⁸.

very truly your | W.H. Miller

1. Dated on the basis of the reference to the *Illustrated London News*.
2. Lavall, Count Nugent (1777–1862, ODNB). Irish-born Austrian field marshal.
3. He was elected an Honorary Member of the Royal Institution in 1829. RI MS GM, 2 November 1829, 4: 54.
4. John Bernard Burke (1814–1892, ODNB). Ulster King of Arms and Keeper of the State Papers in Ireland.
5. Burke (1859), 8–9.
6. 'An Irish Field Marshall', *Ill. Lond. News*, 15 October 1859, 35: 376.
7. Johann Josef Wenzel Radetzky (1766–1858, OBL). Austrian army officer.
8. Wilhelm Karl Haidinger (1795–1871, DSB). Austrian mineralogist.

Letter 3667**Faraday to William Thomson****2 November 1859****From the original in BL add MS 48983, f. 4**[Royal Institution embossed letterhead], London,
W. Royal Institution | 2 Nov 1859

My dear Sir,

I am very much obliged for your last¹, and have been looking over the curves with its assistance. I do not find the particular case of a long mound in the earth which you refer to but I think I have understood all except perhaps one. By the writing of the address I think they came to me from *you* so I return them to you by post with many thanks to you and Professor Maxwell.

I am very glad too to hear of the Electrometer^[.] My reason for writing to you was as follows. Our Managers have a fund founded by Sir Henry Holland² for the purchase of certain pieces of apparatus³ it is limited in extent but if in consideration I find that one of your electrometers is a suitable object I mean to propose it⁴. I think we should need the reflexion appliances inasmuch as we ought to make all our apparatus serve to demonstrate to the Members the facts & discoveries in science. When your instrument comes to Kew and you have as you say a more exact estimate I should be very glad to know.

Both your instrument and your application of it to atmospheric electricity interests me most deeply. Are you inclined (supposing it would suit with your convenience) to tell the story to our Members on one of the Friday Evenings of the next season⁵? It would be a very great pleasure to me to hear you here

I often think of Mrs. Thomson & her sisters & the friends at Aberdeen⁶^[.]
Pray remember me earnestly as well as kindly to Your wife^[.]

Ever Very Truly Yours | M. Faraday

Professor W. Thomson

1. Letter 3665.

2. Henry Holland (1788–1873, ODNB). Fashionable physician. Physician Extraordinary to Queen Victoria from 1837 and Physician in Ordinary to Prince Albert from 1840.

3. See RI MM, 4 April 1859, 11: 273–4.

4. This is not listed as having been purchased from the Holland Fund. RI MS F5B, p. 128.

5. Thomson (1860b), Friday Evening Discourse of 18 May 1860.

6. Where Faraday stayed while attending the meeting of the British Association.

Letter 3668**Faraday to Carlo Matteucci****5 November 1859¹****From an extract in Bianchi (1874), 218–9**

La condizione della mi salute s'aggrava sempre più, e non so come procedono gli afari, perchè quando leggo, non posso rammentarmi le cose lette, per cui tralascio di leggere quello che dovrei. Ultimamente ho lavorato per sei settimane intiere, sforzandomi d'ottenere dei risultati che infatti ottenni,

ma tutti negativi. Ma il peggio si è che, guardando le mie note passate, trovo d'avere constatato cogli esperimenti, otto e più mesi fa, gli stessi fatti, ed io li avevo completamente dimenticati². Ciò mi dà qualche fastidio, non già il lavoro, ma la dimenticanza, perchè il lavoro senza memoria riesce inutile.

Con tutto ciò, ho migliaia di motivi d'essere soddisfatto, e se parlo di questa condizione non è per lagnarmi, ma per spiegarla. Se potessi fare a modo mio, non vi scriverei mai una lettera senza che contenesse qualche soggetto scientifico. Invece, il caso vuole che siano tutte prive d'interesse come la presente. Pure, sinchè non vi dia noia, continuerò a scrivervi, non fosse per altro che per ringraziarvi delle vostre importanti notizie, e per dirvi che sono sempre l'amico vostro.

TRANSLATION

The condition of my health becomes ever more serious and I do not know how things go on, because when I read, I cannot remember what I have read and thus I omit to read what I should. Lately, I worked for six whole weeks, straining to obtain results which I in fact obtained, but all negative. But the worst of it is that, looking over my previous notes, I find that I had verified by experimentation, eight or more months ago, the same facts, and I had completely forgotten them². This vexed me somewhat, not just the work but the forgetfulness, because work without memory proves to be useless.

Despite all this, I have thousands of reasons to be satisfied, and if I speak of my condition, it is not to complain but to explain myself. If I could do as I wished, I would not write a letter to you unless it contained some scientific subject. Instead, the reality is that they are all devoid of interest, like the present one. However, if you do not mind, I shall continue to write to you, if for no other reason, than to thank you for your important news and to tell you that I am still your friend.

1. Date as given in Branchi (1874), 218.

2. This would appear to refer to Faraday's work on the relationship gravity to other forces that he undertook in March 1859 (Faraday, *Diary*, 4–28 March 1859, 7: 15822–906). Though there is no record of any experiments in his *Diary* for October 1859, it is possible that he may have been writing his paper on the subject (see letters 3788 and 3790).

Letter 3669

Faraday to George Biddell Airy

7 November 1859

From the original in RGO6/408, f. 388

[Royal Institution embossed letterhead], Albemarle St. W |
7 Nov 1859

My dear Sir,

I would rather you should think me a little intrusive than think that I and all ours are not charmed to hear you. But I do not wish to intrude; I am

only anxious that if you are inclined to favour us in the coming season, such inclination should not fail through the want of that attention which takes the form of asking. In fact I am trying to ask for the Institution in such a manner as not in the least to trouble you if you say no and yet to express our sense of pleasure & thankfulness if you say yes¹.

Sometimes friends who are inclined ask as to what subject. It is not for me to suggest to you but I know I should like to hear about *time* the reasons derived both from Railway & Shipping why it should be alike (or not) all over our islands & the means of making it accurately so[.]

Ever My dear Sir | Very faithfully Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. Airy did not deliver a Friday Evening Discourse during 1860.

Letter 3670

William Thomson to Faraday

7 November 1859

From the original in NUL MS Misc 48

2 College, Glasgow | 7 Nov 1859

My dear Sir,

The diagrams were drawn for you¹ by Clerk Maxwell but they chanced to be addressed by Mr. Macfarlane, my assistant, whose hand is often mistaken for mine. They were left for him to look at for a day at Aberdeen², as he had been engaged in drawing some similar sets of curves to illustrate atmospheric electricity.

I now send you two of them which show the lines of atmospheric force in the neighbourhood of elevations and depressions of various forms[.]

If you cover the lower part of No (1) placed with "Air" up, with the piece of white paper marked "Earth", you will see what I said regarding the semicircular mound. If you take the same diagram with one of the sides up, and cover the lower part with the piece of paper with the curved indentation, placing it in the position indicated by the marks # and *, you will see the aërial field of electric force over a ravine.

If you cover the central circle of the same diagram with the circle of paper, placing the two points which are marked on one side of it in the positions in which curves meet making angles outside the circle, you will see the lines of force about a conducting cylinder insulated in a uniform field of electric force. The curves cutting those lines of force at right angles, are the lines of flow of water meeting and bending round a cylinder held perpendicular to the stream: or the lines of magnetic force about a cylinder of infinitely diamagnetic substance placed in a uniform field of magnetic force.

I showed the corresponding set of diagrams for a sphere, instead of a cylinder, at the meeting of the British Assn. at Belfast in 1852.

If you cover part of No 2 with the paper marked "Tableland" &c in the manner indicated, you will see the lines of force over a piece of stratified sea coast, with land rising vertically and sloping up to a level according to a regular curve. The same diagram turned the other way shows lines of force over a straight mountain ridge, or elevated mound, of the form shown in section by the marked curve.

All these illustrations are applicable only when there are no electrified clouds or masses of air in the neighbourhood.

I must ask you to pardon me for troubling you with all this, which I do only because, from your letter, I thought you might be interested in illustrations of atmospheric electricity[.]

If you desire it I shall send you all the other diagrams immediately, or I shall bring them with me to give you the first time I have an opportunity of seeing you.

To myself it would be a great pleasure, although accompanied with not a small degree of anxiety, to have the prospect of giving a lecture on Atmospheric Electricity to the Royal Institution³. My time during the session of the College here is engaged with scarcely any interval long enough to allow me to undertake anything at a distance, before May. If however it should be desired that I should lecture on Atmospheric Electricity on one of the evenings after the 1st. of May, I shall be willing to do so.

I shall not omit to let you know as soon as possible how I succeed in the way of reflecting electrometers. I hope soon to have one ready to try.

I heard of you yesterday from Mr. Crawford, and was sorry to learn that you had suffered from the rough work in the Channel⁴. I hope now you are feeling quite well again.

With kind regards in which Mrs. Thomson joins, I remain,

Yours very truly | William Thomson

1. See letter 3667.

2. At the meeting of the British Association.

3. Thomson (1860b), Friday Evening Discourse of 18 May 1860.

4. See letter 3662.

Letter 3671

Frederick Augustus Abel¹ to Faraday

7 November 1859

From the original in RI MS Conybeare Album, f. 17

RI Arsenal, Woolwich | 7 Nov 1859

My dear Sir,

I will not fail to bear young Mr. Vincent in mind. I was in hopes, on two occasions, to have procured an engagement for him & am very sorry that I was unsuccessful.

I called upon you with my Brother in Law Mr. Frederick Field – who is just now staying with me and was desirous to have the pleasure of seeing you again, on his return from Chili.

I am, my dear Sir | yours sincerely | F.A. Abel
Dr. Faraday | &c &c &c

1. Frederick Augustus Abel (1827–1902, ODNB). Professor of Chemistry at Royal Military Academy, Woolwich, 1852–1888.

Letter 3672

George Biddell Airy to Faraday

11 November 1859

From the original press copy in RGO6/408, f. 389–90

Royal Observatory Greenwich, SE | 11 Nov 1859

My dear Sir,

I am not ashamed of my delay in answering your letter of 7th¹, because I could not help it, but I am sorry for it. But pity the condition of a poor man who, when he finds several of his natural employments converging on him more than usual, is also suddenly attacked in manner following. The Sale of Gas Act was passed on August 13, and the Treasury had three months given them (to Nov 13) to prepare the necessary apparatus at the Exchequer. The Treasury went out grouse shooting and forgot all about it till an advanced day in October and then called on me, first to say what was to be done (which I had to learn, *ab initio*) secondly to do it. And this has given me abundance of work, not yet finished².

Now to your Royal Institution request. Depend on it that no word from you passes unnoticed by me. But I am rather jaded at present, and do not like much to make a positive engagement. But I will duly think on it, and assuredly will think on the subject which you indicate; and will report to you. My first idea was, that the communication of Accurate Time would make rather meagre subject, but on consideration it widens out a good deal. Thus – simply sympathetic clocks. Clocks maintained by themselves but partially regulated by other clocks. Great Clock at London Bridge for distributing signals. Apparatus at Lothbury for sending simultaneous signals along numerous railways. Primary time signal from Greenwich. Drop of time ball at Deal, and its return signal. Dr. [word illegible] &c. Peculiar adjustment of Post Office clocks &c.

Is this the class of subjects of which you were thinking?

There are two disadvantages 1st. A good deal of apparatus would be required: 2nd. Our galvanic communication is in so bad a state that we cannot at present do all this, and it would then seem absurd to describe it³.

Yours my dear Sir, very truly | G.B. Airy
Professor Faraday

1. Letter 3669.
2. See Airy, W. (1896), 237.
3. Airy did not deliver a Friday Evening Discourse during 1860.

Letter 3673

Georges-Mathilde-Ernest Degrand to Faraday

11 November 1859

From the original in GL MS 30108/3/103

MINISTÈRE | DE L'AGRICULTURE DU COMMERCE | ET DES TRAVAUX
PUBLIQUES | PONTS ET CHAUSSEES | SERVICE CENTRAL | DES |
PHARES ET DU BALISAGE. | ÉTABLISSEMENT CENTRAL | (Quai de
Billy N°. 58)

A Paris, le 11 Novembre 1859 | L'Ingénieur ordinaire
du Service central des Phares et du Balisage |
à Monsieur Le Professeur Faraday

Cher et honoré Monsieur,

Conformément aux ordres de Monsieur l'inspecteur General Reynaud je viens d'adresser à Monsieur l'Amiral Gordon une caisse contenant des échantillons des divers lentilles moulées que nous avons déjà fait exécuter.

Comme les specimens seront sans doute soumis à votre examen, j'ai pensé que vous me pardonneriez la liberté que je prends de venir vous fournir à leur sujet quelques indications.

Vous savez, Monsieur, que le moulage des lentilles n'est certainement pas chose entièrement nouvelle; la première lentille à échelons qui ait existé, celle qui avait appartenu à Buffon¹ était en une seule pièce et avait par conséquent été moulée; sans aller chercher si loin, depuis un certain nombre d'années on a exécuté en France de petits fanaux pour les navires dont l'appareil n'est autre chose qu'une lentille moulée; mais dans les divers essais qui avaient été faits, soit qu'on eut laissé de trop grandes différences d'épaisseur entre les différentes parties des lentilles, soit que les profils eussent été mal calculés, les résultats obtenus avaient été à peu près négatifs.

Le principal mérite des échantillons que nous envoyons à la corporation de Trinity House, c'est donc d'être assez bien réussis pour que l'éclairage qu'ils produisent soit comparable à celui obtenu des verres taillés de dimensions analogues.

Il est probable que ce résultat, indépendamment des dispositions spéciales adoptés pour les moules, tient surtout à ce qu'en multipliant les échelons, les saillies sont devenues beaucoup moindres et que par suite le moulage a pu devenir plus exact.

Ainsi les deux pièces de manchon cylindrique marqués E/1 et E/2 me semblent proportionnellement donner des résultats plus satisfaisants que celles marquées D/1, D/2, D/3, D/4 dont les saillies sont beaucoup plus forts.

Cependant dans le manchon E/1 et E/2 les choses ont été peut être poussées à l'extrême afin d'obtenir une sorte de *glace striée lenticulaire*, et d'après les expériences que nous avons déjà faites, je pense que ce serait un moyen terme entre les pièces E et les pièces D qui donnerait sous le rapport de l'épaisseur de verre et des nombres d'échelons les meilleurs résultats

Il ne faut donc pas considérer les échantillons envoyés comme le dernier mot de ce qu'on peut obtenir par ce procédé, mais bien comme des spécimens de nature à donner une idée de ce qu'on obtiendra certainement lorsqu'on aura acquis une plus grande expérience dans ce genre de travail encore tout nouveau pour la cristallerie qui l'exécute.

Voici la liste de Vos échantillons avec l'indication des repaires qui serviront à réunir les diverses pièces d'une même lentille dans le cas où on voudrait les monter sur des armatures.

II A/1 et A/2 ... sont les deux moitiés d'une lentille annulaire plan-convexe de 0m,255 de largeur sur 0m,51 de hauteur et de 0m,30 de distance focale principale. = Cette lentille est semblable à celles de la partie mobile de l'appareil de Walde et *son effet utile* a été trouvé presque supérieur à celui d'une lentille en verre taillé de dimensions analogues.

III B/1 et B/2 ... sont les deux parties d'un douzième de manchons cylindriques à feu fixe semblable à celui de la partie fixe de l'appareil de Walde = La distance focale principale ou rayon de manchon est de 0,23.

Cette lentille, comme la précédente, est destinée à être illuminée par une lampe à deux mèche[s] concentriques du genre de celles du 3e ordre.

VI & VII C/1, C/2 C/3 ... sont trois spécimens d'anneaux catadioptriques de l'appareil de Walde = ils ont cela de particulier qu'ils sont à échelons, mais du reste ils laissent à désirer; et ceux placés sur l'appareil sont de beaucoup supérieurs.

IV D/1, D/2, D/3, D/4 ... sont les quatre portions d'un quart de manchons à feu fixe de 0m,30 de diamètre = D'après les expériences faites avec M Reynaud ce *simple tambour produit un effet égal à celui d'un appareil entier catadioptrique de 0m,30 en verre taillé* bien que celui-ci soit disposé pour utiliser un angle de lumière plus considérable.

Les échelons extrêmes des pièces marquées D/1 et D/4 ne laissent passer que très peu de lumière et à l'avenir on diminuera les pièces de 0,05 chacune de manière à ne donner au manchon qu'une hauteur totale de 0m,40; ils seront employés dans cet état pour feux de port.

V E/1 et E/2 ... sont les deux parties d'un quart de cylindre à feu fixe de 0m,26 de diamètre intérieur. Les verres de ce genre employés pour feux de port donnent un éclairage de *Sept becs carés*, tandis que les *appareils indiraux* que nous avons employés jusqu'à présent (appareils à réflecteurs) ne donnent que *quatre becs seulement* en dépensant plus d'huile et en coûtant plus cher à établir.

Nous avons déjà en service en France 6 fanaux en verre strié de ce genre et on est très satisfait du résultat obtenu.

Je vous demande mille pardons, Cher et honoré Monsieur, de vous avoir dérangé un instant de vos nombreuses et importantes occupations en venant vous donner d'aussi longs détails au sujet de cet envoi de verre moulés, mais vous nous avez laissé paraître tant de sentiments bienveillants lorsque nous avons eu l'honneur, Monsieur l'inspect. G-al. Reynaud et moi de nous trouver en rapport avec vous, que j'ai pensé que vous m'excuseriez = même si je l'ai fait trop longuement = d'être venu vous entretenir d'un objet qui a paru vous inspirer quelque intérêt lorsque vous avez bien voulu examiner l'appareil du fanal de Walde près de Calais.

Permettez-moi de profiter de cette circonstance pour vous prier, cher et honoré Monsieur, d'agréer l'expression des sentiments de respect et de haute considération avec lesquels je suis | Votre bien dévoué serviteur | Degrand

TRANSLATION

MINISTERE | DE L'AGRICULTURE DU COMMERCE | ET DES TRAVAUX
PUBLIQUES | PONTS ET CHAUSSÉES | SERVICE CENTRAL | DES |
PHARES ET DU BALISAGE. | ÉTABLISSEMENT CENTRAL | (Quai de
Billy N°. 58)

Paris, 11 November 1859 | The senior engineer
of the Central Service of Lights and Beacons |
to Mr. Professor Faraday

Dear and honored Sir,

In accordance with the orders of Inspector General Reynaud, I have just sent to Admiral Gordon a case containing various moulded lentil samples that we have already made.

As the specimens will probably be submitted to your scrutiny, I thought that you would forgive the liberty that I take to provide you with some details about them.

You know, Sir, that the moulding of lentils is certainly not an entirely new thing; the first rib lentil in existence, was the one that belonged to Buffon¹: it was made of one single piece and had therefore been moulded; without searching too far, for a number of years, the apparatus used in small lanterns for French ships has been nothing other than a moulded lentil; but in the various tests that had been done, rather negative results had been obtained, either because too great differences of thickness between the different parts of the lentil had been left, or because their profiles had been badly calculated.

The main merit of the samples that we are sending to the Corporation of Trinity House, is, therefore, that they have been rather well made, so that the light that they produce can be compared to that of cut lenses of similar dimensions.

It is likely that this result, independent of the special care taken with the moulds, comes, above all, from the fact that in multiplying the ribs, the unevenness has become a lot less pronounced and thus the mould has become more precise.

Thus the two pieces of the cylindrical mantle labelled E/1 and E/2 seem to me to give proportionately better results than those marked D/1, D/2, D/3, D/4 of which the unevenness is much more marked.

However, in the mantle E/1 and E/2 things have been pushed to their limit in order to obtain a kind of *lens glass with striae* and following the experiments we have already carried out, I think that somewhere between pieces E and pieces D would, with regard to the thickness of the glass and the number of ribs, produce the best results.

Please do not consider the samples we have sent as the best that can be obtained by this process, but as specimens to give an indication of what will certainly be achieved when greater experience has been acquired in this kind of work, which is still very new to the glassworks that is carrying it out.

Below is a list of the samples with an indication of the pairings that will serve to unite the various pieces of a same lentil should you wish to test them.

II A/1 and A/2 . . . are the two halves of a plano-convex annular lentil of width 25.5 cms, height 51 cms and main focal length of 30 cms. – This lentil is similar to the lentils in the mobile part of the apparatus at Walde and *its useful effect* has been found to be nearly superior to that of a cut glass lentil of similar dimensions.

III B/1 and B/2 . . . are the two parts of a dozen cylindrical mantles with fixed light similar to the fixed part of the apparatus at Walde – the principal focal length or mantle beam is 23 cms.

This lentil, like the previous one, should be lit by a lamp with two concentric wicks of the kind of those of the third order.

VI & VII C/1, C/2, C/3 . . . are three examples of catadioptric rings of the apparatus at Walde – only the fact that they are ribbed is of interest, but, as to the rest, they leave much to be desired; and those mounted in the apparatus are much superior.

IV D/1, D/2, D/3, D/4 . . . are the four quarters of a four-part mantle with fixed light of a diameter of 30 cms – according to the experiments carried out with Mr. Reynaud this *simple hoop produces an effect equal to that of an entire catadioptric apparatus of 30 cms of cut glass* even though the former is disposed to produce a considerably larger angle of light.

The outside ribs of the pieces marked D/1 and D/4 let but a very little light through and in the future each of the pieces will be reduced by 5 cms so as to give the mantle a total height of 40 cms; they will be used in this state as port lights.

V E/1 and E/2 . . . are two pieces of a four-part cylinder with fixed light of an interior diameter of 26 cms. Lenses of this kind used as port lights give a level of light of *seven square burners* whilst the indirect apparatus which we have used up to now (apparatus with reflectors) gives *only [the equivalent of] four burners* whilst using more oil and being more expensive to set up.

We already have in use in France six lights of glass with straires of this type and the results obtained are very satisfactory.

I ask a thousand pardons, dear and honourable Sir, to have taken you away for an instant from your numerous and important occupations, in having just given you such extensive details on the subject of moulded lenses, but you showed us so much kindness when we had the honour, Inspector General Raynaud and I, to be in contact with you, that I thought you would excuse me – even if I have done so at length – for talking to you on a subject which seemed to inspire some interest when you expressed a wish to examine the light apparatus at Walde near Calais.

Permit me to take advantage of this circumstance to ask you, dear and honourable Sir, to accept the sentiments of respect and high esteem, with which I am,

Your very devoted servant | Degrand

1. For an expanded discussion of this, see letter 3687.

Letter 3674

John Tyndall to Faraday

16 November 1859

From the typescript in RI MS JT TS Volume 12, p. 4092

Royal Institution | 16 Nov 1859

My dear Mr. Faraday,

Would you think for a moment at your leisure over the subject of this letter¹. I have already had one from Playfair², to which I have replied in the terms indicated in the first lines of his present letter. I have also been called upon by people of influence from Edinburgh, unconnected with the University³.

I want as much light as possible on the question before deciding and therefore I am anxious to secure the thoughts of yourself and one or two more of my friends – my own feelings are distinct enough. Still a case of the kind is too important to be decided without reflection[.]

Ever yours | J. Tyndall

1. Playfair to Tyndall, 12 November 1859, RI MS JT/1/P/118.
2. Playfair to Tyndall, 9 November 1859, RI MS JT/1/P/117.

3. This refers to Tyndall becoming a candidate for the chair of natural philosophy in Edinburgh. He declined the invitation and Peter Guthrie Tait (1831–1901, ODNB), Professor of Mathematics at Queen's College, Belfast, 1854–1860, was elected. See Eve and Creasey (1945), 82.

Letter 3675

Faraday to John Tyndall

c.17 November 1859¹

From the typescript in RI MS JT TS Volume 12, p. 4150

Private

15 Nov 1859 [sic]

My dear Tyndall,

I really cannot advise you; I can only tell you what I should do, and what I did do, under like circumstances². When the chair of Chemistry was offered me under the strongest private assurances of the authorities, I declined it³. It was all a matter of feeling with me, but the feeling was that, if I had a sufficient moderate income in London, nothing would make me change London for Edinburgh. Others might reverse the terms, and say nothing should bring them to London, so that really I have no advice to give; for I suppose I may now assume that you have a competency in London, and that all beyond will come under the points of honour, prosperity, or pleasure.

Ever, my dear Tyndall | Truly yours | M. Faraday

1. Dated on the basis that this is the reply to letter 3674.

2. See letter 3674 and note 3.

3. See letter 3429.

Letter 3676

Peter Henry Berthon to Faraday

17 November 1859

From the original in GL MS 30108/3/100

Trinity House, London, EC | 17 Nov 1859

Sir,

The Elder Brethren are desirous of your opinion on various points connected with a Fog Bell, which is to be placed on the Start Point, about 30 yards seaward of the Light House and about 76 feet above the level of the Sea.

I herewith enclose a rough outline of the proposed method of suspending the Bell, and also one which will shew the direction in which it is chiefly desirable to throw the sound.

The First Question is, whether a Sounding Board placed over the Bell and one in its rear (i.e. on the land side of it) might not tend to increase the

sound in the desired direction, – and if so, secondly, whether the sounding Board should be of wood or iron. The perishable nature of wood renders iron preferable, if it is likely to prove as efficient in throwing off Sound.

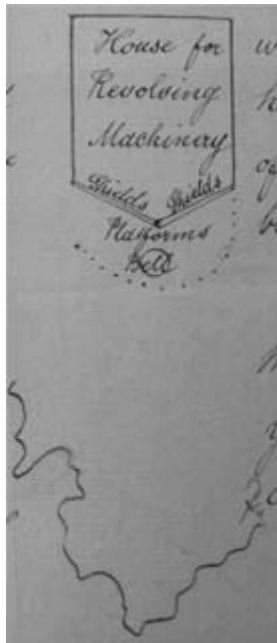
Thirdly, in case you recommend iron, whether there would be any disadvantage in making use of corrugated iron, as being lighter and less liable to bend.

Fourthly, it is proposed to slope the platform beneath the Bell, do you think any advantage would be gained by covering the slope with metal plates in the direction the sound is to be projected.

The size or rather weight of the Bell, whose sound in fogs ought to be heard 3 miles, is another point on which we should be glad to have your advice.

The Bell will be struck by weights or hammers, working on the Ball and Socket Plan at either end of an iron bar, which will be made to revolve by Machinery.

The Revolving Apparatus will be protected from the weather by a house in the rear of the Bell, one end of which will be formed by the sounding boards, as shewn in the Drawing.



These are the chief points the Elder Brethren will be glad to have your opinion on, but any other general observations will also be gladly received.

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq. F.R.S. | &c &c &c

Letter 3677

William Thomson to Faraday

17 November 1859

From the original in IET MS SC 2

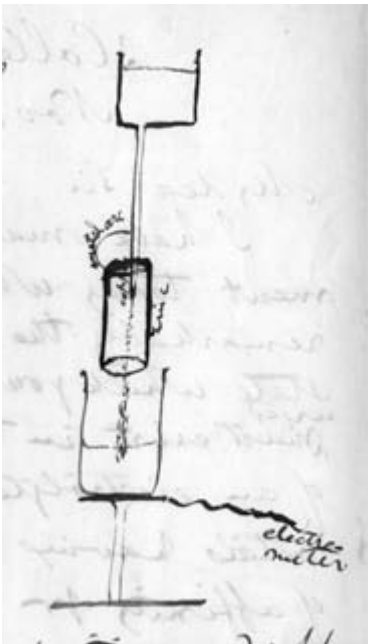
2 College, Glasgow | 17 Nov 1859

My dear Sir,

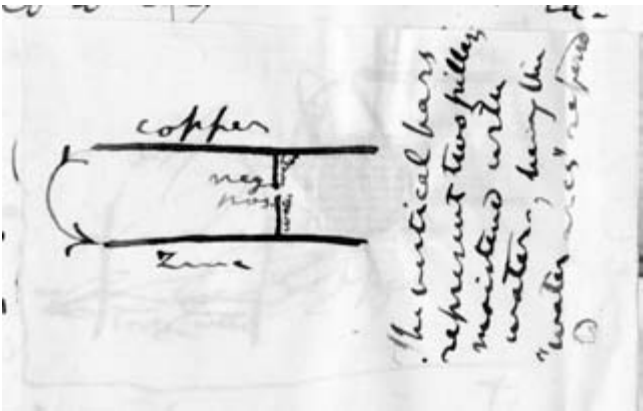
I have made an experiment today which illustrates remarkably the electropolar state which you have always urged must exist in the particles of an electrolyte between two metals having different degrees of affinity for one of its elements; and I cannot deny myself the pleasure of immediately telling you of it.

An uninsulated can of water was placed so as to discharge its contents through a vertical copper pipe and fine nozzle of copper in a stream breaking into drops after about an inch, and falling into an *insulated* jar connected with an electrometer.

A tube of metal, either zinc, composition (chiefly copper), or common sheet copper, was sometimes held round the stream of water and sometimes it was left simply with air and the walls of the room round it. In the last mentioned case the electrometer quickly showed strong negative, because the air of the room and a plate glass electrical machine not far off which had been in use, electrified the *uninsulated* metal tube & issuing stream *negatively* by influence. When a *copper* tube was held round the stream the electrometer showed little or no effect. When a piece of bent sheet zinc was held in the hand round the stream, & the vertical copper pipe was touched by the same hand, the electrometer shewed nothing or slight positive. When a metal wire connected the sheet zinc round the stream with the copper pipe from which the stream issued, the electrometer very quickly gathered a strong negative charge. In 30 secs: it showed 65° negative. When a composition metal (nearly copper) tube was used, the electrometer gathered 7° neg. in the 30 secs. When a wide zinc tube, about 6 inches diameter & 12 inches long was held vertically so that the stream broke into drops about its centre the electrometer gathered negative quickly, provided the zinc was connected by metal with the copper from which the water issued. By using a well *insulated* support a spark may readily be obtained by allowing the charge to gather. I shall write to you when I have got one.

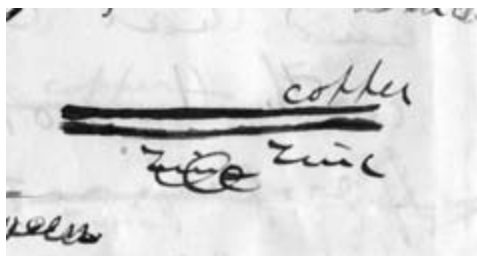


That the result must be as I found it seemed obvious before I made the experiment, from the following considerations.



If the two water arcs (moist cotton wicks for instance) shown on the sketch are brought together a current flows in the direction shown by the arrowhead. Hence before the ends of the water arcs are united it must tend to flow and these opposed ends must be oppositely electrified;— that connected with the zinc positively and that connected with the copper negatively. If drops from the former are allowed to fall through a hole in the zinc they must each carry away, negative electricity, by the dynamical power of the gravitation of the water, and communicate it to any conductor into which they may be allowed to fall.

This explains completely Volta's contact & separation experiment, by electrochemical action, if there be any moisture present between the plates.



For at the instant when the last point of metallic contact still exists the moist film on the copper surface must be negative to the moist film on the zinc surface; and the thinness of the separating air must make the quantity large even for a small difference of potentials between the opposed water surfaces. Immediately & even after contact (the insulation by glass handles being perfect) each metal has and keeps the electricity its (assumed moist) surface had when opposed to the other, and thus the copper shows negative and the zinc positive after separation. It would be important to try volta's experiment in artificially dried atmospheres of various gases.

The experiment which I made today was done by the aid of my divided ring electrometer – Perhaps one cell Daniell would, as the electrometer stood in the experiment, have given about 2° – The effects I observed are capable of indefinite accumulation, &, to the extent I had them, would I believe have shown on a gold leaf electrometer.

Believe me Yours most truly | William Thomson (T.O.)

P.S. I think I must ask that, if wanted to lecture on atmospheric electricity¹, I should have some time later than the 7th of May, as I fear I should have no time for preparation sufficient before the 1st of the month.
Professor Faraday

1. Thomson (1860b), Friday Evening Discourse of 18 May 1860.

Letter 3678**Faraday to George Biddell Airy****18 November 1859****From the original in RGO6/408, f. 391**[Royal Institution embossed letterhead],
London W | 18 Nov 1859

My dear Sir,

I and we thank you most heartily for your kind letter¹[.] The class of subjects you mention is exactly what I was thinking off [sic] and as to what is not yet done your views of the future is as you may believe equal in interest to your doings in the past. As to the much apparatus required I think Latimer Clark would help us all he could and for the illustration &c of a discourse by you I think. I may answer for any expence incurred under ten guineas[.]

If you decide to lay us under obligation and are as yet free in respect of time would any part of March be agreeable? or else settle your own time letting us know when you can²[.]

Ever Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. Letter 3672.

2. Airy did not deliver a Friday Evening Discourse during 1860.

Letter 3679**Faraday to William Thomson****19 November 1859****From the original in BL add MS 48983, f. 6**

[Royal Institution embossed letterhead] | 19 Nov 1859

My dear Sir,

Your experiment is most beautiful & striking¹ and a wonderful proof of the extent to which experiment may be carried by those who engraft it on principle. I think I understand the whole of it and I conclude you can make the dropping water bring away either P or N electricity according as the nozzle from which the jet issues & the cylinder round the jet are of this or that metal.

I conclude I am right in assuming that static induction is set up all the time across the air between the breaking stream & the surrounding cylinder but what a curious variety of cases may be devised in that case since water issuing from the same vessel with different jets & these surrounded by cylinders of different metals would give so many changes from one water source[.]

Suppose Voltas experiment of contact and separation were made in perfectly dry naphtha or out of schist it ought to give no result if contact were made at dry places of the metals. Whereas if a portion of the surface were wetted & then contact made a result should be obtained. I shall be anxious to hear of your educated results[.]

There are five fridays after the 7th of May – but do not delay your evening longer than is necessary²[.]

Respectful remembrances to Mrs. Thomson
from Yours ever truly | M. Faraday

1. See letter 3677.

2. Thomson (1860b), Friday Evening Discourse of 18 May 1860.

Letter 3680

George Biddell Airy to Faraday

19 November 1859

From the original press copy in RGO6/408, f. 392

19 Nov 1859

My dear Sir,

In various regards I should like the time of a proposed lecture¹ to be well delayed, first for my personal comfort, secondly because our Observatory wires are almost inefficient [sic].

Moreover I should be much obliged if you would treat me as a masked battery & not publish me for some time.

I am, my dear Sir | Yours most truly | G.B. Airy
Professor Faraday

1. See letter 3678.

Letter 3681

Donald MacFarlane to Faraday

19 November 1859

From the original in IET MS SC 2

College Glasgow | 19 Nov 1859

Sir,

Professor W. Thomson being at Edinburgh to day he desired me to send you an account of an experiment he made yesterday.

A flat ring, one half copper and the other zinc truly turned was insulated with its plane horizontal, a light aluminium index was suspended, by a fine glass fibre centrally over the ring, the index was connected by a fine platinum wire attached at its centre of motion, the wire hung vertically with its lower end in a dish of sulphuric acid and was thereby in communication with a conductor placed under the ring, the whole was inclosed in a case and the air kept artificially dry, the position of rest of the index was made to be exactly over the Copper & Zinc junction of the ring and the effect observed was, when care was taken that the glass of the enveloping case was completely diselectrified a slight charge of positive electricity communicated to the conductor

below caused the index to be attracted by the copper, and a slight negative charge similarly applied caused the index to move towards the zinc.

I remain | Your Obedient Servant | D. M'Farlane

Letter 3682

Thomas Stevenson to Faraday

19 November 1859

From the original in IET MS SC 2

Private

Edinburgh | 19 Nov 1859

My dear Sir,

Allow me to thank you for your kindness in sending me the notice regarding the polyzonal lens¹. In the Pamphlet² which my brother³ and I were forced although most reluctantly to publish we do not state that *Buffon* proposed the built lens although there is some doubt even as to this. But one thing *is certain* that Condorcet most distinctly described it as well as the means which it afforded of correcting spherical aberration. We know therefore that the polyzonal lens as now used in Lighthouses was first invented (for burning purposes only) by Condorcet⁴ in 1780 while Fresnel was the first to publish and the first to apply it to Lighthouses⁵[.]

With many thanks for your kind attention in which my brother joins

Believe me | ever faithfully yours | Thomas Stevenson

Professor Faraday | &c &c &c

1. Possibly a reference to letter 3673.

2. Stevenson and Stevenson (1859).

3. David Stevenson (1815–1886, ODNB). Engineer to the Northern Lighthouse Board.

4. Marie-Jean-Antoine-Nicholas Caritat, Marquis de Condorcet (1743–1794, DSB). French mathematician.

5. Fresnel (1822).

Letter 3683

Faraday to George Biddell Airy

21 November 1859

From the original in RGO6/408, f. 394

[Royal Institution embossed letterhead] | 21 Nov 1859

My dear Sir,

How our letters must be crossing. I have yours of the 19th¹. You shall be treated as a masked battery – and all your wishes attended to[.]

Ever Truly Yours | M. Faraday

G.B. Airy Esqr | &c &c &c

1. Letter 3680.

Letter 3684**Faraday to John Hall Gladstone****21 November 1859****From the original in RS MS 743.86**

[Royal Institution embossed letterhead] | 21 Nov 1859

My dear Gladstone,

I am much obliged by your letter. A week or two since I passed some days on the French Coast¹ with M Reynaud the Chief of the Lighthouses department & M. Degrand the Engineer & they told me of their experiments at Paris on the Magneto electric light. I think they must have made out a good point or two especially as relates to the nature of the carbons. Kindest regards to Mrs. Gladstone²[.]

Ever Truly Yours | M. Faraday

J.H. Gladstone Esqr | &c &c &c

1. See letters 3650 and 3661.

2. Jane May Gladstone, née Tilt (d.1864, age 33, GRO). Married J.H. Gladstone in 1852. See his ODNB entry.

Letter 3685**Faraday to Peter Henry Berthon****21 November 1859****From the original copy in GL MS 30108/3/100**

Royal Institution | 21 Nov 1859

Sir,

In reply to your letter of the 17th instant¹ respecting the proposed fog bell for the Start Point and the four questions to which you desire answers, I reply, to the first, that in my opinion such sounding boards above & behind the bell would prove of much use: To the second that I think either wood or iron would be efficient; – and to the third that I should incline to plain iron in the doubt that corrugated iron might interfere with the numerous vibrations; – To the fourth, – I think the sloping of the platform would be useful but I am not prepared to say whether the covering of the slope with metal plates would be advantageous or necessary[.] I am not aware of the extent of slope surface.

In March & September of 1853, I had occasion to write to the Trinity House in respect of Fog bells &c² as far as I was able to form a judgement. These papers express my present opinions & I would beg to refer you to them, especially in reference to the uncertainty of the data concerning the application of Fog signals³[.]

I have the honour to be | Sir | Your Very Obedient Servant |

M. Faraday

P.H. Berthon Esqr | &c &c &c

1. Letter 3676.
2. Faraday to Herbert, 24 March 1853 and 22 September 1853, letters 2660 and 2733, volume 4.
3. This letter was read to Trinity House By Board, 22 November 1859, GL MS 30010/42, pp. 430–1. It was ordered that the Committee of Lights should conduct further experiments.

Letter 3686**Faraday to Robert Mallet****28 November 1859****From the original in SI D MS 554 A**

[Royal Institution embossed letterhead],
 Albemarle St. W | 28 Nov 1859

My dear Sir,

I thank you heartily for your notice. The auroral period which it refers to was wonderful & I do not at all doubt the fact. I was at that time in Scotland & the phenomena were wonderful. The auroral influence must have been simultaneous for the whole earth¹[-]

Ever Truly Yours | M. Faraday
 Robt. Mallet Esqr | &c &c &c

Address: Robert Mallet Esq | &c &c &c | 1 Grosvenor Terrace |
 Monckstown | Co. Dublin

1. See *Ann. Reg.*, 1859, 101: 128–9 for this aurora which occurred on 28–29 August 1859.

Letter 3687**Georges-Mathilde-Ernest Degrand to Faraday****28 November 1859****From the original in GL MS 30108/3/103**

MINISTÈRE | DE L'AGRICULTURE DU COMMERCE | ET DES TRAVAUX
 PUBLIQUES | PONTS ET CHAUSSEES | SERVICE CENTRAL | DES |
 PHARES ET DU BALISAGE. | ÉTABLISSEMENT CENTRAL | (Quai de
 Billy N° 58)

A Paris, le 28 Novembre 1859 | L'Ingénieur ordinaire
 du Service central des Phares et du Balisage |
 à Monsieur Le Professeur Faraday &c &c

Cher et Honoré Monsieur,

Avant de répondre, en ce qui concerne la lentille de Buffon, à la lettre que vous avez bien voulu me faire l'honneur de m'écrire le 18 9-e¹, j'ai voulu remonter à la source même du fait que j'avais cité et cela joint à quelques occupations plus nombreuses que d'habitude m'a empêché de vous répondre avant aujourd'hui.

Augustin Fresnel dans son mémoire de 1822, reconnaît que Buffon, avant lui, avait songé aux lentilles à échelons² et que ce fait qu'il ignorait entièrement au moment où il avait proposé lui-même les nouvelles lentilles était consigné dans *l'éloge de Buffon par Condorcet*³.

J'ai consulté en conséquence les œuvres de Condorcet (*oeuvres complètes, tome IV, Brunswick et Paris an XIII, 1804*⁴) et voici le passage que j'y trouve :

"... Peu de temps après, Mr. de Buffon prouva par le fait la possibilité des miroirs d'Archimède⁵ et de Proclus⁶. Tzetzés⁷ en a laissé une description qui montre qu'ils avaient employé des miroirs plans. Les essais tentés par Kirker⁸, avec un petit nombre de miroirs ne laissaient aucun doute sur le succès; Mr. Duffay⁹ avait répété cette expérience; Harsoëker¹⁰ avait commencé une machine, mais il restait à Mr. de Buffon l'honneur d'avoir le premier, parmi les modernes, montré l'expérience extraordinaire d'un incendie allumé à deux cents pieds de distance, expérience qui n'avait été vue avant lui qu'à Syracuse et à Constantinople."

"Bientôt après il proposa l'idée d'une loupe à échelons n'exigeant plus ces énormes masses de verre si difficiles à fondre et à travailler, absorbant une moindre quantité de lumière, parcequ'elle peut n'avoir jamais qu'une petite épaisseur, offrant enfin l'avantage de corriger une grande partie de l'aberration de sphéricité. Cette loupe proposée en 1748 par Mr. de Buffon, n'a été exécutée que par Mr. l'abbé Rochon¹¹, plus de 30 ans après (1778) avec assez de succès pour montrer qu'elle mérite la préférence sur les lentilles ordinaires. On pourrait même comparer de plusieurs pièces ces loupes à échelons; on y gagnerait plus de facilité dans l'exécution, une grande diminution de dépense, l'avantage de pouvoir leur donner plus d'étendue et celui d'employer, suivant le besoin, un nombre de cercles plus ou moins grand et d'obtenir ainsi d'un même instrument différents degrés de force."

Mr. de Buffon étant mort le 16 avril 1788 c'est dans le courant de cette année ou au plus tard en 1789 que Condorcet a dû prononcer son éloge à l'académie des sciences = je ne pense donc pas qu'à cette époque Sir David Brewster eut déjà songé aux lentilles à échelons et décrit aussi complètement que le fait Condorcet les avantages que devaient présenter les nouvelles lentilles. = Il est d'ailleurs parfaitement connu que ce n'est que dans sa jeunesse et avant de se consacrer tout entier à l'histoire naturelle que Mr. de Buffon s'était occupé de physique, il est donc bien probable que c'est en effet à 1748 qu'il faut remonter pour retrouver l'idée première des lentilles à échelons. Ce ne serait qu'en 1788 seulement qu'il n'en faudrait pas moins, pour les physiciens qui vivent encore, reconnaître avec la même modestie que Fresnel qu'ils ne sont pas les premiers à avoir songé à cette utile invention =

Quant à la lentille exécutée en 1778 par l'abbé Rochon, elle a appartenue plus tard à Mr. Charles¹² dont le cabinet de physique est maintenant au Conservatoire des Arts et Métiers à Paris, M l'abbé Moigno m'assure qu'il a eu

occasion de la voir dans le collection de cet établissement. Mais pour ma part je dois avouer que je n'ai pas encore été aussi heureux que lui sous ce rapport¹³.

A l'occasion des expériences qui vont être faites à Trinity-House sur les lentilles moulées je crains de n'avoir pas assez insisté dans ma précédente lettre sur ce point essentiel: qu'il peut se faire que les lentilles à distance focale égale ne produisent pas toujours autant d'effet que les lentilles taillées, mais que leur près étant beaucoup moindre il eut ainsi d'atteindre toujours un effet égale en les exécutant avec un plus grand foyer = Ainsi dans ce moment-ci Mr. Reynaud vient de commander 8 appareils en verre moulé parmi lesquels se trouve un appareil à feu fixe du 3e ordre = En verre taillé cet appareil aurait eu 1m.00 de diamètre intérieur, tandis que je vais le faire exécuter en verre moulé avec un diamètre de 1m.20 = Dans ces conditions il aura certainement, d'après les expériences déjà faites, un effet supérieur à celui d'un appareil de 1m.00 en verre taillé et son près restera cependant de 40% inférieur à celui de ce dernier appareil.

Mr. Reynaud a été extrêmement sensible à votre bon souvenir et se joint à moi, Cher et Honoré Monsieur, pour vous prier d'agréer l'expression de notre haute considération et de nos sentiments les plus dévoués,

Degrand

P.S. En ce qui me concerne en particulier je tiens à vous remercier mille et mille fois de votre indulgente et bienveillante lettre.

TRANSLATION

MINISTERE | DE L'AGRICULTURE DU COMMERCE | ET DES TRAVAUX
PUBLIQUES | PONTS ET CHAUSSÉES | SERVICE CENTRAL | DES |
PHARES ET DU BALISAGE. | ÉTABLISSEMENT CENTRAL | (Quai de
Billy N° 58)

Paris, 28 November 1859, The senior engineer of
the central Service of Lights and Beacons,
to Mr. Professor Faraday &c &c

Dear and Honored Sir,

Before answering the letter that you kindly honoured me by writing on 18th November¹, regarding the lentil of Buffon, I wanted to go back to the very source of the facts that I had cited and this, added to other occupations, more numerous than usual, has prevented me from responding before today.

Augustin Fresnel in his paper of 1822, recognizes that Buffon, before him, had had the idea of rib lentils² and that this fact, of which he had been completely ignorant when he had himself proposed new lentils, was mentioned in *the éloge of Buffon by Condorcet*³.

I therefore consulted the works of Condorcet (*Oeuvres Complètes*, volume IV, Brunswick and Paris, year XIII, 1804⁴) and here is the passage that I found there:

"... A short time after, Mr. Buffon demonstrated the possibility that the mirrors of Archimedes⁵ and Proclus⁶ might have existed. Tzetzes⁷ left a description which shows that they had used plane mirrors. The trials done by Kircher⁸, with a small number of mirrors, left no doubt as to their success; Dufay⁹ repeated this experiment; Hartsoëker¹⁰ began a machine, but the honour of being the first, amongst the moderns, to demonstrate the extraordinary experiment of a fire lit two hundred feet away, an experiment that had not been seen before, except at Syracuse and Constantinople, fell to Buffon."

"Shortly afterwards, he proposed the idea of a rib lens which did not require the enormous amount of glass (which made it difficult to mould and work) and, because it needed a small thickness, absorbed a small quantity of light and in fact had the advantage of correcting a great deal of the spherical aberration. This lens, proposed in 1748 by Mr. Buffon, was not made successfully until more than 30 years later (1778), when Abbé Rochon¹¹ demonstrated that it merited preference over ordinary lentils. One could even compare several pieces of these rib lenses; the experience gained in making them would lead to a great decrease in their cost, [as well as] the advantage of being able to give them more breadth and of using, according to need, a greater or smaller number of circles and thus of obtaining different degrees of intensity from the same instrument."

Mr. Buffon died on 16 April 1788 and it was during that year, or at the latest in 1789, that Condorcet pronounced his eulogy to the Academy of Sciences. I do not think, therefore, that at this time Sir David Brewster had even thought of rib lenses or had described as fully as Condorcet the advantages that the new lenses presented. It is, moreover, perfectly well known that Mr. Buffon occupied himself with physics only in his youth and before dedicating himself to natural history; it is thus probable that, in fact, one should go back to 1748 to find the first notion of rib lentils. It should not just be in 1788 that this should be recognised; the physicists of today should, with the same modesty as Fresnel, acknowledge that they were not the first to have thought of this useful invention.

As for the lentil made in 1778 by Abbé Rochon, it was later in the cabinet of physics that belonged to Mr. Charles¹² and is now in the Conservatoire des Arts et Métiers in Paris; Abbé Moigno assures me that he had the opportunity of seeing it in the collection of this establishment. But for my part, I must confess that I have not been as fortunate in this regard¹³.

Concerning the experiments that are going to be done for Trinity House on the moulded lentils, I fear that I did not emphasise this essential point in my

previous letter: that it is possible that lentils with an equal focal length are not as effective as cut lentils, but that their near [focus] being much smaller, it is thus possible to achieve an equal effect producing them with a larger focus. Thus, at the present moment, Mr. Reynaud has just ordered eight instruments in moulded glass amongst which is found an apparatus with fixed light of the 3rd order. In cut glass this apparatus would have an interior diameter of 1m, whilst I am going to have it made in moulded glass with a diameter of 1.2m. In these conditions it will certainly have, according to experiments already carried out, a superior effect to that of an instrument of 1m in cut glass; its near focus will nevertheless remain about 40% inferior to that of the latter apparatus.

Mr. Reynaud was extremely touched by your kind remembrance and joins me, dear and honored Sir, in begging you to accept the expression of our high consideration and our most devoted sentiments,

Degrad

P.S. So far as I am concerned, I wish particularly to thank you a thousand and a thousand times for your indulgent and kind letter.

1. Not found, but see letter 3682.
2. Fresnel (1822), 125 refers to Buffon's work.
3. Marie-Jean-Antoine-Nicholas Caritat, Marquis de Condorcet (1743–1794, DSB). French mathematician. Condorcet (1788), 54.
4. Condorcet (1804), 4: 27–90, p. 34.
5. Archimedes (c.287–212 BCE, DSB). Mathematician who lived and worked in Syracuse.
6. Proclus (c.410–485, DSB). Greek philosopher.
7. Joannes Tzetzes (c.1110–c.1180, ODB). Byzantine scholar.
8. Athanasius Kircher (c.1602–1680, DSB). Natural philosopher.
9. Charles-François de Cisternai Dufay (1698–1739, DSB). French natural philosopher.
10. Nicolas Hartsoëker (1656–1725, Fontenelle (1725)). Dutch optician.
11. Alexis-Marie Rochon (1741–1817, NBU). French astronomer.
12. Jacques-Alexandre-César Charles (1746–1823, DSB). French physicist.
13. Brewster (1827), 35 claimed that Buffon had not built his lens and claimed the invention for himself. For a discussion of the issues involved see Middleton (1961).

Letter 3688

James Clerk Maxwell to Faraday

30 November 1859

From the original in IET MS SC 2

Marischal College, Aberdeen | 30 Nov 1859

Dear Sir,

I am a candidate for the Chair of Natural Philosophy in the University of Edinburgh, which will soon be vacant by the appointment of Professor



Plate 11. James Clerk Maxwell. From Thomson *et al.* (1931), opposite p. 12.

J.D. Forbes to St Andrews¹[.] If you should be able from your knowledge of the attention which I have paid to science, to recommend me to the notice of the Curators, it would be greatly in my favour and I should be much indebted to you for such a certificate.

I was sorry that I had so little time in September that I could not write out an explanation of the figures of lines of force which I sent you², but Professor W Thomson to whom I lent them, seems to have indicated all that was necessary, and most of them can be recognised from their resemblance to the curves made with Iron filings.

The only thing to be observed is, that these curves are due to the action either of long wires perpendicular to the paper or of elongated magnetic poles such as the edge of a long ribbon of steel magnetized transversely. By considering infinitely long currents or magnetic poles perpendicular to the paper, we obtain systems of curves far more easily traced than in any other case, while their general appearance is similar to those produced in the ordinary experiments.

All the diagrams have two sets of lines at right angles to each other and the width between the two sets of lines is the same so that the reticulation is nearly square. If one system belongs to poles, the other belongs to currents, so that if the meaning of one be known, that of the other may be deduced from it.

I remain | Yours truly | James Clerk Maxwell
Professor Faraday

1. Maxwell was unsuccessful. Peter Guthrie Tait (1831–1901, ODNB), Professor of Mathematics at Queen's College, Belfast, 1854–1860, was elected.

2. See letters 3664, 3665, 3667 and 3670.

Letter 3689

Faraday to John Murray

2 December 1859

From the original in NLS JMA

[Royal Institution embossed letterhead] | 2 Dec 1859

My dear Sir,

I am exceedingly obliged by your kindness in sending me Mr. Darwins¹ remarkable book¹. I have received it at Brighton where it arrived before me & shall read it with great attention[.]

Ever faithfully Yours | M. Faraday

John Murray Esqr | &c &c &c

1. Charles Robert Darwin (1809–1882, ODNB). Naturalist who lived at Down in Kent.
2. Darwin (1859) published by Murray on 24 November 1859.

Letter 3690

William Thomson to Faraday

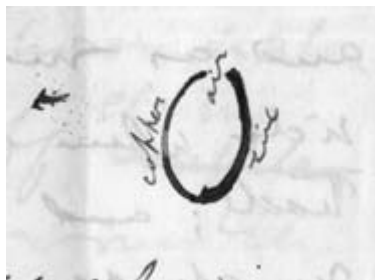
3 December 1859

From the original in IET MS SC 2

2 College Glasgow | 3 Dec 1859

My dear Faraday,

I have got myself into great trouble with my experiment on zinc and copper. I cannot understand the condition of the air between the ends of a compound zinc–copper arc with no liquid.



A positively electrified body placed mid way between them will certainly experience a force towards the copper: and a negatively elec'd. body towards the zinc. This is proved, beyond all doubt I now think, by the experiment which my assistant Mr. Macfarlane described to you a fortnight ago¹. I have repeated it today with great variation in the amount of electrification of the aluminium index, and have found the result confirmed. I have had the index going 10° or 15° on one side of its zero posn. when electrified positively and as many on the other side when electrified negatively; and I have had much smaller effects shown with feebler electrifications immediately after very strong electrifns. of either kind had been applied to the index in various positions, which convinced me that the results could not be explained away by disturbing influences owing to electrifns acquired by imperfectly conducting matter. I have not yet taken the most thorough plan to screen off electric influence from the glass lid of the case, but I mean to do it immediately tho' I cannot now doubt what the result will be.

We may infer that



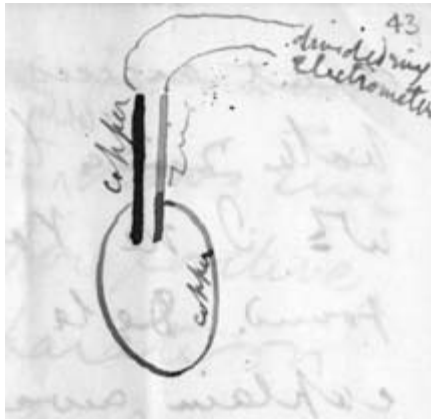
opposed areas of zinc & copper if there be metallic contact in any arc between them will be oppositely electrified, the zinc pos. the copper neg and that they attract one another. The simplest view to be taken of the subject is that the chemical affinity between the zinc and the copper acts at a distance. It would appear from this that if small pieces of zinc and copper, in thin sheets are put together by simple contact, a portion of their chemical affinity is satisfied: and less heat of combination will be evolved if certain quantities of zinc & copper thus previously approximated, are melted together, then if equal quantities in large masses are allowed to run together in melting. This difference would in all probability be quite undiscoverable by direct experiment.

My difficulty is to understand the condition of the air between surfaces of zinc & copper



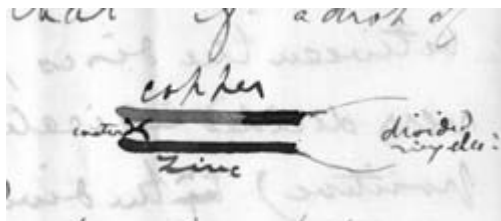
gradually approaching one another. When they are very near, at $\frac{1}{1000}$ or $\frac{1}{1,000,000}$ of an inch, the force in the air between them considered mainly in reference to the air might be supposed to tend to produce a disruptive effect. Yet no spark could pass, because as the surfaces come nearer & nearer one another they acquire in a perfectly gradual manner the relative condition in which they are satisfied to remain after contact. We have therefore *force* in the air as shown by the motions which it produces in electrified bodies, without any disruptive tension; which is contrary to what we might from previous knowledge expect. It seems as if the true explanation must be that the disruptive tendency proceeds from the roots of the lines of force on the opposed surfaces, and that in the present instance the electric relation between the metals and intervening air is such that we have electrification of each surface without any disruptive action.

That there is electrifn. of the opposed surfaces



is merely Volta's experiment, which I have repeated in a considerable variety of ways, finding consistent results. For instance holding the discs at distances from $\frac{1}{10}$ to $\frac{1}{4}$ or $\frac{1}{2}$ an inch, one or both insulated: touching the copper by a copper wire attached to the zinc: separating it copper from copper: and then increasing the distance between the discs gives a very decided indication (zinc positive) on the divided zinc electrometer. The same experiment succeeds perfectly with both zinc & copper thickly varnished, wh. I see Volta himself found. De la Rive tries to explain away this by moisture in cracks in the varnish. If there were any such, sufficient to produce every visible effect, they

would diminish or reverse Volta's result. Thus I find that if a drop of water be put on one of the plates and

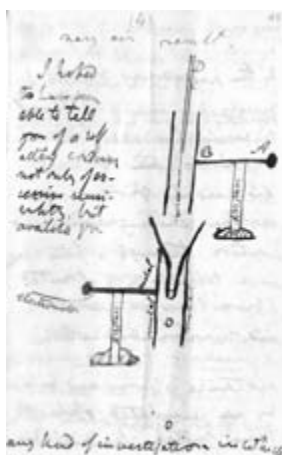


if the last separation between them be water from water, the electrometer shows zinc negative, copper positive, after the separation. This is an extraordinary result at least it was to me unexpected. I expected that the electrometer would show nothing, believing that the electrolytic conductive polarisation of the water would keep the copper & zinc in the state in which their direct Volta-electrifications would be exactly annulled. It seems that the electrolytic action preponderates over the pure copper-air-zinc effect.

If the whole surfaces of the two discs are wet, or lined with wet paper, the effect on separation is insensible: as it is also if a zinc disc be lifted out of water contained in a copper vessel whether it be lifted out very horizontally & slowly till a drop breaks away from one point, or drawn away in an inclined position

I have not yet tried the Volta expt. under naphtha², but I shall do so immediately and I have no doubt it will give the ordinary air result.

I hoped to have been able to tell you of a self acting condenser



not only of excessive sensitivity, but available for any kind of investigation in which very small quantities of electricity are looked for. It is (represented by the preceding sketch) merely an insulated metal funnel discharging in large drops, water which it collects as spray falling from a stream discharging from a cistern in any convenient position, through a fine aperture in a tapering tube of metal (brass I have at present): the jet of water being surrounded where it breaks & for a considerable distance above and below by an insulated tube of the same metal as the nozzle. My trial apparatus has certainly great sensitivity & shows beautiful condensing action. Thus if I lay a little piece of rubbed sealing wax or glass on the insulated area AB which carries the brass tube, the electrometer shows very strong positive: so much so that a succession of sparks pass at very regular intervals (a second of time for instance, or more) and will continue to pass for hours between the insulated and the uninsulated half rings, which are about $\frac{1}{20}$ inch apart. But the apparatus is so liable to disturbance from what I suppose is to be called (after Ritter³) "polarisation", that after a positive charge has been left for a little time on AB the electrometer will show negative for a long time after[.] AB is connected with CD by a wire; and the reverse for a long time after AB has been negatively charged. I have found no material difference in this respect when the inside of the tube round the jet is lined with paper, which is sometimes kept washed and stopping by strong spray and some times is not.

I am going to try tin instead of brass as I find the brass of the nozzle & if the tubes not to a [word illegible] electrically. An iron nozzle I find was neutral to the brass tube I have. Pardon me for this long communication of difficulties.

Yours always truly W. Thomson

1. Letter 3681.

2. See letter 3679.

3. Johann Wilhelm Ritter (1776–1810, DSB). German natural philosopher.

Letter 3691

Peter Henry Berthon to Faraday

6 December 1859

From the original in GL MS 30108/3/101

Trinity-House, London, EC | 6 Dec 1859

Sir,

I am directed to transmit to you the enclosed Copy of a Letter addressed by this Board to Messrs. Chance, Brothers, and Co., of Birmingham, relative to the modification proposed by them suitable to a First Order Fixed Light,

and their reply thereto, together with the accompanying Drawings,– and to request that you will favor the Elder Brethren with your opinion thereon.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq.– F.R.S | &c &c &c

Letter 3692

Faraday to Peter Henry Berthon

8 December 1859

From the original copy in GL MS 30108/3/101

Brighton | 8 Dec 1859

Sir,

I have received from you Mr. Chances letter of the 5th Decr. 1859 and the drawings referred to in it¹. I agree with the principles stated in the letter I had occasion not long since to consider the chief matter; for which purpose I wrote to Messrs Chance and received from them a drawing & explanation which I sent to you². You will perceive that the object he has in view in increasing the number of the pieces of glass is precisely that proposed to be obtained in the pressed glass of M Degrand by increasing the number of stops i.e. diminishing the thickness of the glass traversed by the light & so diminishing the loss by absorption. I return the drawings & letter as desired³.

I have the honour to be | Sir | Your Very Obedient humble Servant |
M. Faraday
P.H. Berthon Esqr | &c &c &c

1. Letter 3691.

2. See letter 3656.

3. This letter was read to Trinity House Wardens Committee, 13 December 1859, GL MS 30025/27, p. 236. It was agreed to adopt Chance's proposals.

Letter 3693

Faraday to William Thomson

8 December 1859

From the original in BL add MS 48983, f. 8

[Royal Institution embossed letterhead], Brighton |
8 Dec 1859

My dear Thomson,

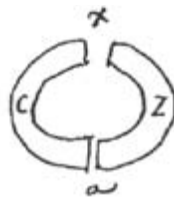
I have been away from home, so am able *only now* to read & answer your letter¹. I did not write in reply to Mr. MacFarlane², for his account raised so many ideas & doubts, that I thought I would wait for more matter. You puzzle me greatly; & I am in great doubt, because I never can *judge* an experiment or make up my mind about it *without seeing it*. No description suffices to answer

all the mental inquiries that arise about the conditions. As you say, you have just got hold of Voltas experiment; only you refer to the air place, where the zinc & copper oppose each other for that cause of the final effect, which he finds in the place of metallic contact.



You seem to have made out that the place of excitement is really at *x* and not at *a*. I suppose if Voltas contact were effectual your charged index which placed at *x* goes either towards the *Z* or *C* according it is neg or positive, ought to do the same thing if placed over *a*; for I do not see why the metallic contact there should undo or hide the electric state which it is supposed to bring on. How is this in experiment? Is your charged index indifferent at *a* & other places except at *x*, where there is separation?

You seem to refer the electric state at *x* to the mutual chemical relations of the zinc & copper;— as if the chemical relations of the air there went for nothing. How would it be, however, if the zinc & copper ring were in such different atmospheres as oxygen carbonic acid – hydrogen, &c the oxygen being ozonified if you like? Would they be indifferent as you seem to expect naphtha will be. You might perhaps select atmospheres which would act more on copper than on zinc, as Sul Hydrogen; or at all events as much or more on silver than on zinc.



If the action be in & through the air place *x*., and that varies with distance, as I think you shew by one experiment, then, should not two half rings of zinc approximated in different degrees at *x* and *a* shew a difference of action at the two places, & therefore a difference of state? Or if metallic contact at *a* is required in the first instance, suppose that made by a metallic arc & the arc then removed, what state will the two opposed surfaces at *a* have? and also those at *x*?

Suppose the contact at *a* made also by *water* & by *air* what are the results in comparison with those of contact by metal.

Does a piece of *Zinc alone* become positive in air i.e. without any metallic contact with copper or other metal – I think I remember an experiment by De la Rive in support of the chemical origin of the electricity of the voltaic pile, in which, passing chlorine through a tube of platinum, he found either the issuing chlorine or the tube electric.

Supposing your charged aluminium index was over *x*, in figure page 2., it will go either towards the zinc or the copper, according as it has been rendered Pos or Neg. If the place *x* was closed the metals being there brought into contact, would the signs of electricity in the ring cease? and then on opening the place would they appear there again?

Is there *no* transfer of Electricity from copper to zinc at the place of metallic contact?

Is it possible that the metal of the index may act a part in the play at the place *x*? It is evidently within the reach of the sources of action if they exist there;– or else it would not be affected: Would indexes of zinc, Gold, Copper &c, behave differently one to another?

I dare say you have by this time answers to many of these questions & many others, that would, with me, arise in turn. But I shall wait to hear your news.

I understand your self acting condenser; but all the influencing circumstances can only be learned, as you are finding, by trial & experiment.

Kindest remembrances to Mrs. Thomson and heartiest wishes for your quick success[.] Remember that the more embarrassing the effects may be with you just now the more novel & important may be the principles involved in them. Any thing regarding the first motions of Electricity among metals & surrounding exciting bodies must be of the utmost consequence to the progress of the science.

Ever Yours | M. Faraday

1. Letter 3690.

2. Letter 3681.

Letter 3694

Arthur-Auguste De La Rive to Faraday

9 December 1859

From the original in IET MS SC 2

Genève | le 9 Xbre 1859

Monsieur & très cher ami,

Qu'il y a long-temps que je n'ai eu de vos nouvelles. J'ai su par Tyndall qui nous a fait une visite cet été, que vous étiez bien & toujours aussi actif. Quant à moi j'ai passé aussi un bon été & je me prépare à donner cet hiver

quelques *lectures* (pour employer le mot anglais) sur les phénomènes naturels. Je suis fatigué, je vous l'avoue, de l'invasion de la Science industrielle & je trouve que les oeuvres de Dieu dans la nature méritent bien aussi, & peut-être plus encore que celles de l'homme dans les arts, d'attirer l'attention & d'exciter l'admiration. Je me propose donc dans ces lectures d'examiner successivement les phénomènes naturels qui sont dus à l'attraction, à la chaleur, à la lumière & à l'électricité. Si je vous dis tout cela, c'est que j'ai un service à vous demander. Edmond Becquerel m'a écrit que vous aviez donné une leçon dans laquelle vous aviez réussi à montrer les phénomènes de phosphorescence à un auditoire nombreux en les projetant sur un écran.— Avez-vous publié cette leçon?¹ & dans ce cas auriez-vous la bonté de m'en envoyer un exemplaire? en tout cas serait-il indiscret de vous demander quelques détails sur la manière dont vous avez opéré.? Si vous m'envoyez un article imprimé, vous n'avez qu'à le mettre sous bande, mais plus il faut l'affranchir; je crois que c'est une affaire de quelques pences. L'autre jour on m'a demandé pour un numéro du Quarterly qu'on n'avait pas affranchi frs. 43.50c. de poste, tandis qu'avec 10 pence on l'aurait affranchi.— Quant aux lettres il est inutile de les affranchir.

J'ai lu avec beaucoup d'intérêt le dernier mémoire de Mr. Gassiot²; je suis très occupé dans ce moment du même sujet & j'espère ne pas tarder à faire connaître les résultats assez intéressants que j'ai obtenus. J'ai aussi quelques autres travaux d'un autre genre sur le métier que j'espère mener à bien avant le printemps. Si Dieu me prête vie, j'ai toujours le projet d'aller faire avec ma femme & mes deux filles cadettes³, une visite à nos amis de Londres le printemps prochain. Ce sera probablement la dernière, car je me fais vieux & passé un certain âge il convient aussi bien pour le *moral* que pour le *physique* de rester chez soi, d'adopter une vie calme & sérieuse & de savoir mettre un intervalle entre la vie & la mort afin de pouvoir se recueillir avant de paraître devant Celui qui doit nous juger.

Ne trouvez-vous pas que plus on avance dans la vie, plus on sent le besoin de penser au ciel plus qu'à la terre; mais au milieu de tant d'intérêts & de préoccupations, cela m'est bien difficile & j'ai des moments de découragement qui ne doivent pas vous étonner vous qui mieux que personne comprenez ce que je dois éprouver. Il m'est doux de pouvoir vous parler ainsi; vous êtes le seul de mes amis Scientifiques auquel je puisse me confier & m'entretenir de ces sujets. C'est que vous êtes mieux qu'un ami Scientifique, vous êtes pour moi un ami Chrétien & ce titre que vous me permettez de vous donner est, vous le savez, bien doux pour moi.

Ma femme me charge de ses compliments les plus affectueux pour vous & pour Madame Faraday; je vous prie de me rappeler aussi à son bon souvenir

Your faithfull | A.de la Rive

TRANSLATION

Geneva | 9 Dec 1859

Sir and very dear friend,

What a long time it has been since I heard from you. I knew through Tyndall, who visited us this summer, that you were well and just as active as always. As for me, I also had a good summer and I am getting ready to give, this winter, a few *lectures* (to use the English word) on natural phenomena. I am tired, I must admit, of the invasion of industrial Science and I think that God's works in nature also deserve to attract attention and to excite admiration, perhaps more so than those of man in the arts. I intend, therefore, in these lectures, to examine successively the natural phenomena that are due to attraction, to heat, to light and to electricity. I tell you all this because I have a request to make of you. Edmond Becquerel wrote that you had given a lecture in which you had succeeded in displaying the phenomena of phosphorescence to a large audience by projecting them on a screen. Have you published this lecture¹ and if so, would you have the kindness to send me a copy? In any case, would it be indiscreet to ask you for some details on the manner in which you operated? If you send me a printed article, you have only to put it under cover, but you must also stamp it; I believe it is a case of just a few pence. The other day I had to pay 43.50 francs postage for an issue of the Quarterly that had not been stamped, whereas it could have been stamped for 10 pence. As for letters, there is no point in stamping them.

I read with considerable interest Mr. Gassiot's last paper². I am very occupied at the moment with the same topic and I hope not to delay in making known the quite interesting results that I have obtained. I am also working on something else which I hope to complete successfully before the spring. If God lends me life, I still hope, with my wife and two younger daughters³, to visit to our friends in London next spring. It will probably be my last visit, because I am getting old and past a certain age, it behoves us, both for *moral* and *physical* reasons, to remain at home, to adopt a quiet and serious life and to know how to put an interval between life and death in order to gather one's thoughts prior to appearing before Him who will judge us.

Do you not find that the more one advances in life, the more one feels the need to think of heaven more than earth; but in the middle of so many interests and preoccupations, it is very difficult and I have moments of discouragement that will not surprise you, you who better than anyone, can understand what I feel. It is a great comfort to me to be able to speak to you in this way; you are the only one of my Scientific friends to whom I can confide and talk of these topics. It is because you are better than a Scientific friend, you are, for me, a Christian friend and this title which you allow me to use, is, as you know, very dear to me.

My wife charges me with her most affectionate compliments to you and Mrs. Faraday; I also ask to be remembered to her.
Your faithful | A. de la Rive

1. Faraday (1859c), Friday Evening Discourse of 17 June 1859.

2. Probably Gassiot (1859).

3. Adélaïde-Eugénie-Augusta De La Rive (1838–1924, Choisy (1947), 51) and Françoise-Amélie-Alice De La Rive (1844–1914, Choisy (1947), 51).

Letter 3695

Faraday to Arthur-Auguste De La Rive

16 December 1859

From the original in BPUG MS 2361, f. 91–2

Royal Institution | 16 Dec 1859

My dear De La Rive,

Your letter¹ was a very agreeable surprize I cannot tell when I wrote to you last² but know I often think of you and that with very great pleasure since it is in the belief that you can understand more of the power of God than what can be gained by the study only of his material works_[.] Yet how wonderful they are I think yours is just the mind to revel amongst them as the evidences in natural things of his eternal power & Godhead³ – and though I do not like when speaking of them in a common lecture to deal irreverently with religion by drawing it in at second hand I think it is impossible to forget who hath ordered them.

I have no doubt you have Becquerels papers from the *Annales de Chimie*⁴ whether he will be able (or Ruhmkorf for him) to send you some of his phosphori I do not know. I had some from him very beautiful. They are preparations of the Sulphurets of Strontia, lime and baryta – He pulverizes them & then having gummed a paper surface (a circle 8 or 9 inches in diameter) he sifts the powder over it & when all is dry knock off the loose powder & a pure phosphorescent surface is left_[.] Having an Electric lamp with a lens & a divergent beam – placing the expanded hand on the surface & then throwing the beam on to it for a moment the result is beautiful for on showing it to the audience after the hand is removed there is its black form impressed on a sheet of light. These phosphori on paper or exposed to air will not keep_[.] Those I had are all insensible now as I found the other day: or I would have sent some by post – but kept in tubes hermetically sealed up they remain good for years_[.]

When Becquerel speaks in his letter to you of my experiments on a screen I suppose he refers to fluorescence as well as phosphorescence_[.] The fluorescence is most powerfully produced by the rays at the violet end of the spectrum_[.] So by throwing the spectrum obtained by passing a divergent

beam from the Electric lamp through a prism placed near the lens of the lamp; cutting off the red orange yellow rays &c by a screen placed near the prism – a beam of feeble light is obtained on the large white screen well fitted to bring out the fluorescent results of Uranium glass – Quinine, [word illegible] &c[.]

I have sent you a printed report of my evening⁵ – by the regulations here it seems to require only a penny stamp but I have been so often disappointed by the uncertainty of the Post office proceeding that I have not usually sent these unimportant papers to you. I only hope you will not have more to pay[.] The report is not worth more[.] However in it you will see described a phosphoscope which I had made for the evening & which answered exceedingly well especially with the nitrate of uranium. I think you will have no difficulty in comprehending it. The cylinder described was in a vertical position and no part of it more than 10 or 12 inches distant from the Electric focus[.] It was closely made & blackened so that no light escaped outwardly but such as was brought out by the phosphorescent or fluorescent substance[.]

I am glad to hear that you think of paying England a visit[.] I look upon you as a young man – and then again as a strong young man. I know you can go on working in a manner which astonishes me I look on & admire – I rejoice with you & Madame De la Rive and though I wish for the power of imitating I do not envy you[.] It is surely my time to rest. A new year is coming and a new period & life may they be happy to us and ours is the earnest & I desire it to be the only wish of your old friend[.]

My wife desires to join me in the sincerest thoughts of good to you and Madame de la Rive.

Ever My dear friend | Affectionately Yours | M. Faraday

1. Letter 3694.

2. Letter 3519.

3. Romans 1: 20.

4. Becquerel (1859).

5. Faraday (1859c), Friday Evening Discourse of 17 June 1859.

Letter 3696

Faraday to James Walker

20 December 1859

From the original in SI D MS 554 A

R Institution | 20 Dec 1859

My dear Mr. Walker,

I intended to call on you and asked for a little confidential talk – but I have been seriously ill of sore throat & not able to leave the house – Now I am getting better & hope to lecture next Tuesday¹ as announced but this illness has drawn off my preparations for the course (of 6 lectures) so much that I fear I cannot ask you to let me call on you before they are over. I should be

glad however if you would give me general leave & let me know your usually convenient hour

Ever Truly Yours | M. Faraday

1. That is 27 December 1859. However, the first Christmas lecture of Faraday (1860b) was not delivered until 31 December 1859, RI MS Le1/1, p. 115. Faraday (1860b), 175 noted that the opening lecture was postponed twice.

Letter 3697

James Walker to Faraday

23 December 1859

From the original in RI MS Conybeare Album, f. 41

23, Gt. George Street, Westminster, SW | 23 Dec 1859

My dear Mr. Faraday,

I came from Dover last night.

As I have *quadrupeds*, it is easy for me to drive up to you at any time, if you will name a time¹.

I am | My dear Mr. Faraday | Your's truly | Ja Walker

1. See letter 3696.

Letter 3698

Peter Henry Berthon to Faraday

23 December 1859

From the original in GL MS 30108/3/102

Trinity House | 23 Dec 1859

My dear Sir,

I hope we shall not be putting you to any serious inconvenience on requesting you to examine one of Messrs Chances' apparatus (dioptric) at this House on *Tuesday next* the 27. Inst.

It is for a Colonial Light and Messrs Chance have taken some weeks beyond their stipulated time for it's construction, and the Board of Trade, under the impression that it would be ready in due course, have, I understand, engaged Freight for it on board a vessel sailing early in January, hence has arisen the necessity for our summoning you thus suddenly.

Pray accept my best wishes for the health & happiness of yourself & all in whom you are [word illegible] at this Season, and Believe me

My dear Sir | Yours very truly | P.H. Berthon

M. Faraday Esq | &c &c &c

Letter 3699**John Tyndall to Sarah Faraday****24 December 1859****From the typescript in RI MS JT TS Volume 12, pp. 4093–5**

Geneva | Christmas eve, 1859

My dear Mrs. Faraday,

I have just dined, feel exceedingly well, and cannot I think adopt a better plan to convert the whole dinner into nutriment than the plan of writing to you. I take the opinion of the celebrated Hufeland¹ on this point, and he affirms that there is nothing better than the company of pleasant friends for promoting health and digestion. I am here at Geneva – I succeeded in crossing the channel on Thursday night² – the commotion had almost entirely subsided and nobody that I saw was ill. From Calais half way to Paris we had a good carriage, we then changed, and I found myself with five others, and three footwarmers in the same small compartment. The smell was like the cabin of a badly ventilated ship: but the travellers like so many toads seemed to revel in the noisome atmosphere. They would not open a chink nor let a breath of fresh air into the carriage, and so we were compelled to partake of each others carbonic acid again and again. Having arrived at Paris I drove straight to the Lyons station, and being determined to get rid of the diabolical footwarmers at least I took a second class to Macon. Further than this it was not possible to go. So I had to content myself with the space from London to Macon unbroken by a night's delay. At Macon I slept – and so soundly! The boots almost shook my door off its hinges before he could wake me this morning. The streets of Paris were in a dreadful state of puddle from the melted snow – for 50 miles beyond Paris there was but little snow upon the ground – here and there along the fringes of the river the ice was piled in long barriers, but the country generally was free from it. At Macon however the whole land lay in the stern clasp of winter; between Macon and Geneva for some short distance the covering of snow again lightened but near Geneva and around the town all is thickly covered. The chain of the Jura shining white is quite majestic. I have paid my fare in the diligence to Sallanches, but I was informed to my comfort that there was no communication with Chamouni, and that it was very doubtful whether the snow would permit my reaching it. My informant did not know what a good pair of legs can do when all other modes of conveyance fail. I journeyed part of the way with a Frenchman, or a Swiss whose language was French. He had a theory of England which did not please me – England was sinking must sink in accordance with the law of things – she had developed all that she could develop, and must now go down hill. France was now to rise, but when I pushed him as to the powers of development which France possessed and we did not possess he was a little put out. These old Continental shavers imagine that England is like a gouty old man, rich and comfortable, but with a vitality which is fast dying out.

I should very much like to have my antagonist with me for half a day upon the glaciers, just to give him a taste of the vanishing vitality of England.

I have seen nobody at Geneva, but to night I purpose calling at the house of Mr. DelaRive. I hope Mr. Faraday is better; previous to leaving I saw Dr. Bence Jones and his opinion was that Mr. Faraday would soon be well – I sincerely trust that nature may do her work of renovation even quicker than Dr. Jones anticipates.

Tomorrow morning (Xmas morning) I start for Sallenches; and shall try and get a sledge from there to Chamouni – There my trouble really commences: but if I can only see across the Mer de Glace, and if the guides at Chamouni be neither cowardly or lazy I hope to get my work done – At all events I shall try and having done my part should I even fail. I shall return content.

A merry Christmas to you and very many of them – I shall make myself as merry as it is possible to be in the interior of a diligence. – good bye

Ever Yours sincerely | John Tyndall

1. Christoph Wilhelm Hufeland (1762–1836, NDB). German physician and medical writer.
2. That is 22 December 1859.

Letter 3700

John Tyndall to Faraday

26 December 1859

From the typescript in RI MS JT TS Volume 12, pp. 4096–9

My dear Mr. Faraday,

I wrote to Mrs. Faraday from Geneva¹, and will now report to *you* a portion of my subsequent proceedings. I started from Geneva at 8 A.M. on Christmas morning and reached Sallenches a little after 4 P.M. The time at my disposal being so extremely short my aim was to reach Chamouni on the night of Christmas day, in order to commence, weather permitting, my observations on Monday morning². I therefore hired a vehicle at Sallenches and started thence at 5 P.M. Not liking to run a risk, the exact nature of which I did not see, I soon forsook the carriage and placed myself beside the postillion. He was young, strong and resolute, had good eyes, and needed them extremely at times. We had no moon, no lamps, and a dense gray cloud canopy over head cut away from us the feeble light of the stars. As far as Servoz our journey was pleasant enough but afterwards the road was difficult and our progress slow. Deep snow flanked us right and left, which, in the middle of the road, was more or less beaten down, filling the space however with knobs and hummocks. Trotting was here quite out of the question. Having passed the pont Pelissier, a little above Servoz, we alighted to lessen the strain upon the horses, and I went on in advance. Bare, brown, and motionless the trees stood right and left, while the cliffs and precipices, mottled with

the snow which clung to their ledges took any form which the imagination choosed to give them. To my left the Arne rushed along a deep ravine, and sent upwards through darkness a sad and broken murmur. Sometimes on coming behind an eminence the sound was suddenly and utterly cut away, and the consequent stillness was solemn in the extreme. It was the silence of a churchyard; and the huge black pines which threw their gloom upon the road seemed like the hearseplumes of a dead world. I walked on till I reached a wooden hut where batons crystals and *eaudevie* are offered to the passing travellers in summer; leaning against the door I enjoyed for a time the intense sternness of the scene. The sound of the river was here audible, but dim, distant, and melancholy, yet still the voice of life. I was far in advance of my conveyance, and the intermittent tinkle of the horses bells informed me of the progress and the pauses of the vehicle. At the summit of the incline we again mounted and proceeded slowly towards *lesouches*. We passed several houses, all dark and dismal, loaded with snow and without light or sound of merriment to denote that it was the pleasant Christmas time. The horses seemed affected by a kind of torpor, and leaned listlessly against each other as they crawled along. Vainly the postillion tried with tongue and whip to kindle their enthusiasm: once or twice he succeeded in urging them to a trot down the steeper slopes, but they quickly subsided into the previous monotonous pull. As we approached Chamouni the drowsiness of nature began to operate upon me, and I quitted the carriage within what I supposed to be a quarter of an hour's walk to the village. Previous to this however the wind from the mountains had met us in strong and hostile gusts, as if Mont Blanc wished to warn us from his terretory. At the top of the last inclination of the road the surface became more exposed to the action of the wind, and on passing the summit the road disappeared. The wall to the left was quite covered, and a few isolated stones which rose above the snow at intervals to the right were the sole land marks of the right hand wall. The snow, drifted by the storm, had ranged itself in oblique ridges across the road; they could not be seen; there was no light to shew them, but I staggered over four or five of them in succession, sinking kneedeep, until finally on stepping promptly forward to regain a lost equilibrium, I found myself immersed to the waist. The idea occurred to me that I had missed the road, and I vainly looked round in search of some object by which I might check my position. I retraced my steps: the carriage, when I reached it, had got among the ridges, and one of the horses was down; his haunches were immersed in the snow, one of his legs sunk to the shoulder, and the other thrown forward upon the surface. My companion, as I have said, was strong and resolute, and he needed both qualities. In extreme cases I have known guides to give way to sullenness and ill humour, but this man shewed nothing of the kind. He succeeded in getting his horse upon his legs; but being, like myself, doubtful as to whether we had not quitted the road, I went back exploring, and thus satisfied myself that we were right. Afterwards I walked on in front, choosing the harder portions of

the snow, while he slowly followed, holding his horses by their heads. After half an hour's struggling we found ourselves in the streets of Chamouni. All seemed dead here also – no sound, no light; the thawing snow splashed as it fell from the loaded eaves, the fountain made a melancholy gurgle; here and there a loosened window shutter, swung creaking in the wind, and banged against the object which limited its oscillations. All was desolation. The Hotel de l'Union so gay and full of life in summer, was nailed up and deserted. We rang the bell at the Royal Hotel, but the deep bark of a watch dog, the resonance of which proclaimed the hollowness of the house was long our only answer. Ringing seemed powerless, so I tried the energy of my bootheel against the door, and by perseverance roused the sleepers. In ten minutes I was seated beside a stove in a comfortable room, listening to the wild harmonies of the storm as it roared through the chimney funnel.

This morning, Monday, the snow falls heavily: tomorrow I shall make an attempt on the Mer de Glace. It is absolutely necessary that I should be able to see *across* the glacier. If this be not possible I return defeated, but still with the consolation that I have done

"The best my circumstances allows"

Believe me | Ever Yours | John Tyndall

1. Letter 3699.

2. That is 26 December 1859.

Letter 3701

John Tyndall to Faraday

27 December 1859

From the typescript in RI MS JT TS Volume 12, pp. 4100–3

Montanvert, Chamouni | 27 Dec 1859

My dear Mr. Faraday,

At 3 o'clock to-day just as you were commencing your lecture in Albemarle Street, I drank your health at the Montanvert, and wished you vigour to accomplish your work to your own satisfaction and to the delight of "the juveniles"¹. The wine in which I drank it had to be dug out of the earth where it had been buried to keep it from the hands of an adventurous robber, and also from the sterner hand of the frost. I left Chamouni this morning under the most unpromising auspices it had snowed heavily during the night, and the clouds when I arose gathered all around the mountains obscuring them from top to base. It was also spitting snow. My men came to me at 7 o'clock and we agreed to wait till 8, and then decide as to our future movements. The idea of coming all the way to Chamouni and quitting it without at least making an attempt to accomplish my work was extremely repugnant to me, so that when my men came a second time, and appeared rather in favour of the expedition than otherwise, I at once resolved to start. We did so at a quarter before 9 A.M. Two of my men had tried to force their way

from the source of the Arverron straight up the mountain to the Montanvert on Thursday last², but found the effort so painful that today they decided on following as close as possible the usual route. For the sake of security I have engaged four men who are to be tied together while ranging out my lines; the crevasses are covered with snow, but some of them only lightly, so the man who fixes my stations must have three others to take care of him. Two additional men were hired to carry our provisions to the Montanvert. Our work, for some time, was comparatively easy: the men however halted after the first three quarters of an hour, attached pattens to their feet. These were pieces of wood, about sixteen inches long and ten wide, which presented a large surface to the snow and prevented the wearers from sinking too deeply. I tried the pattens, but not being accustomed to them rejected them after a brief essay. I had reason to repent of this afterwards. The pine trees were quite majestic: the snow of the preceeding night had fallen through a perfectly still atmosphere and loaded the branches, forming in some cases, a layer at least ten inches deep. The branches drooped under the weight and presented the appearances of arms terminated by vast claws turned downwards; some of the smaller pines with closer branches were quite covered, and presented most fantastic appearances; one in particular was exactly like a huge white parrot with folded wings and drooping head. The average depth of the snow was between 3 and 4 feet; but in some places it exceeded five feet. The men took the lead in turns; the snow was like flour, and the first man laboured through it up to his knees and often to his hips he and the last man viz myself suffered most: he, because he had to break fresh ground, and I because the steps which my precursors made were not always sufficiently rigid to sustain my weight, and I sometimes found myself in a shaft waist deep, from which it required a considerable effort to extricate me. Two or three times one of the men in advance of me sank suddenly, leaving absolutely nothing but his head above the surface of the snow. On the steeper slopes the work was very difficult, for here a certain component of the slope added itself to the true vertical height of the snow. The couloirs were encumbered with the *debris* of avalanches, and on one open space we worked upwards for three quarters of an hour over the ruins of an avalanche that had fallen the night before. We choose it, because the snow of the avalanche was more compact than the fresh snow which it had swept away in its downward rush. At several different places my attention was excited by a dull booming sound like the report of a distant gun: it was caused by the breaking asunder of the layer of snow, which had been for some time previous in a state of strain, the forward portion being held back by its attachment to the portion behind. We actually in several instances saw the crevasse formed at the moment that the sound was heard. The weather improved as we ascended, and some of the cloud effects were wonderful: language is a mere lumbering vehicle to describe the magnificence of their arrangement. They shifted incessantly: sometimes leaving the grandest summits drawn against the deep blue heaven, sometimes obscuring all above and casting an angry indigo-blue light upon the snow and pines. The Aiguille du

Dru was noble in the extreme – half its mighty cone strongly illuminated, and half in shade; while the light snow powder scattered upon its ledges was caught by the wind and unfurled itself as a banner which streamed far southward from the mountain head. We reached the Montanvert at 2 P.M. after five hours and a quarter of extremely hard work; I in a ferocious state of hunger as I had not taken any food during the journey. The Mer de Glace was quite glorious, not as in summer wasted and dirty, but pure and white with its frozen billows steep, high, and sharply crested. It was all in shadow save the portion opposite to the tributary which descends from the Col de geant; this tributary being 14 degrees from the meridian, at 2 o'clock the sun beams gushed straight down the corridor of le geant, ruled the Mer de Glace with a transverse beam and flooded the Aiguille de Morne with radiance from base to summit. Having swept the snow from the floor of our domicile a large pine fire was made beside which I now sit. A thermometer at 8 feet distance from the fire still shews a temperature of half a degree centigrade under zero. I have just placed a minimum thermometer on a chair which I have fastened in the snow, and tomorrow morning I shall be able to state the lowest temperature of the night. The stars are shining, and the north wind an hour ago shook all the windows and made some of the panes perfectly musical. It has now subsided and I have strong hopes of being able to accomplish what I intended. I have given you but a feeble idea of the interest of this day's experience. The frozen figures on the window panes of the Montanvert Auberge are the most beautiful things imaginable, and when I place my fingers against the glass and melt the frozen layer the recrystallization of the liquid water, looked at through a pocket lens, is perfectly exquisite. It gladdens one's very heart to see those beauties: Some artist proudly remarked on viewing a fine picture "I too am a painter" – Is it presumption if when looking at those wondrous productions, and feeling a certain affinity with them one should bless the gods for being a natural philosopher?

Good bye | Ever Yours | John Tyndall

1. Faraday (1860b). See note 1, letter 3696.

2. That is 22 December 1859.

Letter 3702

Faraday to Edward Frankland

29 December 1859

From the original in the possession of Mrs. Raven Frankland

[Royal Institution embossed letterhead] | 29 Dec 1859

My dear Frankland,

I hope to begin my Juveniles on Saturday¹ and wanting to use Colladion balloons apply to you to know if you can tell me where I can get them for

I really do not know. Can you help me[.] If I get them by Tuesday² that will do[.]

Mrs. Faraday had a letter from Tyndall dated *Christmas Eve Geneva*³[.] The next day he was to start for Salenche [sic] & Chamouni[.] Plenty of snow – Give our kindest remembrances to Mrs. Frankland⁴ and with the heartiest wishes of all here with me for your health & happiness at this cheerful season & at all times[.]

Believe me to be | My dear Frankland | Ever Truly Yours |
M. Faraday

1. That is 31 December 1859 when he delivered the first of Faraday (1860b). RI MS Le1/1, p. 115.

2. That is 3 January 1860.

3. Letter 3699.

4. Sophie Frankland, née Fick (1821–1874, ODNB under E. Frankland). Married Frankland in 1851.

Letter 3703

Faraday to Peter Henry Berthon

29 December 1859

From the original copy in GL MS 30108/3/102

Royal Institution | 29 Dec 1859

Sir,

I have examined the two dioptric apparatus to which your letter of the 23¹ referred and which are I believe intended for Vancouver island. I find them excellent. The glass is perfect as to colour and relatively very free from striae especially in the central & more important parts. It is well wrought as to finish and shape so that the various parts (no less than 580 in number in the larger one) are perfectly accordant in their optical action so as to send a fine ray forward in the right direction[.] The larger apparatus is constructed to give 20 flashes in one revolution. This involves the use of 20 pannels with their metal frames & the consequence is that one ninth of the light falls on these frames & is cut off[.] Nevertheless as far as I am able to judge not more metal has been employed than is sufficient to give the required stability & strength to the whole structure[.]

I have the honour to be Sir | Your faithful humble Servant | M. Faraday
P.H. Berthon Esqr. | &c &c &c

1. Letter 3698.

2. This letter was read to Trinity House Wardens Committee, 3 January 1860, GL MS 30025/27, p. 257.

Letter 3704**Thomas Mayo¹ to Faraday****January 1860²****From the original in RI MS Conybeare Album, f. 46**

[Athenaeum letterhead], 56 Wimpole Street | Jan

Dear Faraday,

I have in my desk the rudiments of a lecture on 'the Relations of the Public to the science & practise of Medicine'. I know not whether I am competent to give such a Lecture; but I know that it is *wanted* at the present time. It is at the Service of the Royal institution, if they are disposed to give me a Friday Evg in the course of the Season³.

Yours truly | Thos Mayo

1. Thomas Mayo (1790–1871, ODNB). Physician.

2. Dated on the basis of the reference to Mayo's discourse.

3. Mayo, T. (1860), Friday Evening Discourse of 11 May 1860.

Letter 3705**Faraday to Edward Frankland****2 January 1860****From the original in the possession of Mrs. Raven Frankland**

[Royal Institution embossed letterhead] | 2 Jan 1860

My dear Frankland,

Let me acknowledge your kindness in the warmest manner I can. Your deed & your manner of doing it are very acceptable, & pleasant[.] I hope to use the balloons¹ in the course of a day or two in the mean time I fear I am robbing you when you may want them – but after this week is over I will try to see you[.] My kindest remembrances to Mrs. Frankland² & to all yours with the best wishes of the season[.]

Yours Most faithfully | M. Faraday

Tyndall has come back & looks well.

1. For his Christmas lectures, Faraday (1860b). See letter 3702.

2. Sophie Frankland, née Fick (1821–1874, ODNB under E. Frankland). Married Frankland in 1851.

Letter 3706**Faraday to Juliet Pollock****4 January 1860****From the original in SI D MS 554 A**

[Royal Institution embossed letterhead] | 4 Jan 1860

My dear Mrs. Pollock,

You will think me sadly remiss in thanking you for your kind letter & thoughts but coarse things & the concerns of the lectures¹ have so taken up my time that I have been really unable fitly to express myself[.] I write a line

or two of a note & then forget what is written & how the construction of the sentence runs & get all wrong & give it up – and this I have done more than once[.]

But I value your favours greatly – and your boys² visit & their kindness in thinking of me & now I remember that they have only just returned home and might like to hear an odd lecture or two. If they do pray let them run in as they pass using my name at the door[.]

I am sorry you were dull at Christmas but hope better things of you[.] We are getting on pretty well here[.] My wife & niece having much cold in the body but warm remembrances for you[.] Jeannie has lost her voice so that I am really too quiet[.]

Again & Again Ever Yours | Most Truly | M. Faraday

Mrs. Pollock,

Love to all whom I dare send it to[.] I would not be in debt for such a precious thing as that & yet I do not want it to be by measure
MF

1. That is his Christmas lectures, Faraday (1860b).

2. Frederick Pollock (1845–1937, ODNB), later a legal scholar and writer. Walter Herries Pollock (1850–1926, AC), later a lawyer and writer. Maurice Emilius Pollock (1857–1932, *The Times*, 26 April 1932, p. 1, col. b), later a sculptor.

Letter 3707

James Copcutt¹ to Faraday

4 January 1860

From the original in GL MS 30108/3/122.1

New Road, Aylesbury | 4 Jan 1860

Professor Faraday,

My Dear Sir,

Having now successfully arranged the various departments of our new System of lighting – the “Fitzmaurice Light” – I am desired to convey (with his Comps.) the wishes of the Hon W.E. Fitzmaurice, (who is abroad) that you should at any time convenient to yourself, appoint a time privately to see the new arrangements, and make any enquiries you may wish respecting them, at his residence, “*Conway Lodge, 12 Hyde Park Gate, W*”.

I have the pleasure to send to you (with my highest regards) a copy of my Pamphlet² – a feeble effort – to explain to the Public as well as I am able. May I add that, with 48 hours notice I shall have more pleasure to meet you at “Conway Lodge” as above, at which place any communication will be most cheerfully attended to by

Yours sincerely | J. Copcutt

M. Faraday Esq

1. Unidentified.

2. Copcutt (1859). There is a copy of the pamphlet in GL MS 30108/3/122.

Letter 3708**Faraday to James Copcutt¹****9 January 1860****From the original copy in GL MS 30108/3/122.1**

Albemarle St. | 9 Jan 1860

Sir,

I have received your note² and read your pamphlet³. There is nothing in either which makes it at all desirable to me to see your apparatus;— for I have not time to spare to look at a matter two or three times over. In referring to Major Fitzmaurice I suppose you refer also to his application to the Trinity House. In that case I shall hear from him *through the Trinity House* – he has however certain enquiries (which I have no doubt have gone to him long ago through the Trinity House) to answer before I shall think it necessary to take any further steps in the matter⁴. With these however I suppose you have nothing to do.

Are you aware that many years ago our Institution was lighted up for months if not years together by oil gas, (or as you call it Olefiant gas) compressed into cylinders to the extent of 30 atmospheres and brought to us from a distance. I have no idea that the Patent referred to at the bottom of page 9 could stand for an hour in a court of law⁵. I think too, you are wrong in misapplying the word *olefiant*_[.] It already belongs to a particular gas and cannot without confusion be used as you use it_[.]

I am Sir | Your obedient Servant | M. Faraday
J. Copcutt Esqr.

1. Unidentified.

2. Letter 3707.

3. Copcutt (1859).

4. See letter 3484.

5. Copcutt's Patent 1859-413 for 'Improvements in Obtaining Light from Gases'.

Letter 3709**Faraday to Edward Frankland****10 January 1860¹****From the original in the possession of Mrs. Raven Frankland**

[Royal Institution embossed letterhead] | 11 Jan 1860 [sic]

My dear Frankland,

I return the Collodion balloons I hope all safe & ready for use²_[.] I hope I have not had them away at an inconvenient moment.

Curious fatality with us just now Owen lectured³ to day with the smallest possible amount of voice & I was very glad he gave in 15' or 20' before the hour was over⁴_[.]

Ever Your Obliged | M. Faraday

1. Dated on the basis of the reference to Owen's lecture.
2. He used these for his Christmas lectures, Faraday (1860b). See letters 3702 and 3705.
3. The first of Owen's Fullerian course 'Fossil Birds and Reptiles', RI MS Le4/227.
4. Owen (1894), 2: 97 noted that he had a cold and gave 'only three-quarters of lecture'.

Letter 3710**Edward Frankland to Faraday****12 January 1860****From the original in RI MS Conybeare Album, f. 3**

St. Bartholemew's Hospital, EC | 12 Jan 1860

My dear Professor Faraday,

I am sorry you had the trouble of sending back the balloons¹, as I did not intend them to be returned; however in some respects I am glad, since such clumsy specimens of collodion art do me no credit. When I make another lot I will take the liberty of sending you a few better made ones so that you may have them at hand whenever you want them.

I am heartily glad to hear from the servant who brought your note that you have got rid of your cold & that you have not suffered from the fatigue of your lectures² which came at such an inopportune moment.

With kind regards to Mrs. Faraday and Miss Barnard

Believe me | ever yours sincerely | E. Frankland

1. See letter 3709.

2. That is his Christmas lectures, Faraday (1860b).

Letter 3711**Faraday to Angela Georgina Burdett Coutts****16 January 1860****From the original in BL Burdett-Coutts papers**

[Royal Institution embossed letterhead],

Albemarle St. W | 16 Jan 1860

My dear Miss Coutts,

I received your very kind message and remembrances by Mr. Barlow and the accounts of your health which gave me great pleasure: I am always made happy by the consciousness that I am remembered by my friends. Your message has roused me up to think of a Pantomime night and I am about to say that if next Tuesday tomorrow week¹ you could give me your order I should like much to take our own party & a couple of friends; but we should then be five – is not that too much. If Tuesday should be a wrong day with you then let me say Monday i.e. to day week but if the days are indifferent to you Tuesday will be the best[.]

Believe me to be Ever | Your faithful Servant | M. Faraday

1. That is 24 January 1860. See letter 3718.

Letter 3712

Faraday to William Scrope Ayrton

16 January 1860

From a photocopy in BL RP 1880

[Royal Institution embossed letterhead], London |
16 Jan 1860

My dear Sir,

In reference to the rules of the Royal Society regarding papers I can tell you how they were when I took part somewhat actively in the proceedings of years now long past^[1]. Papers may either be presented through the Secretary or by a Fellow, but when a Fellow presents a paper for his friend he is considered (or was considered) as answerable for its character as new and sound philosophy. I used to have a great many papers sent to me and as I could not approve of more than one in ten or thereabouts I gave great offence to the other nine to whom I had no other answer than the principle of action I have told you of. After a time therefore I resolved to present no more but to ask all who applied to me to send through the Secretary and I am obliged to adopt the same law of conduct still or else break down in consistency & also open a door which I had much labour in shutting^[1]. I am sure you will accept my explanation & not wish me now to alter the rule¹^[1].

Ever My dear Sir | Very Truly Yours | M. Faraday

W.S. Ayrton | &c &c &c

1. There is no paper by Ayrton in the archives of the Royal Society and none seems to have been submitted.

Letter 3713

Faraday report to Trinity House

16 January 1860

From the original copy in GL MS 30108/3/103

On Moulded Glass

Royal Institution | 16 Jan 1860

The specimens of moulded glass received from France are intended for optical use in lighthouses, and are like those that are already applied in the third order light established on the Point du Walde near Calais. Instead of being *wrought* into shape by machinery, they are *pressed* into shape in moulds whilst hot. Instead of each rib being separate, many ribs are contained in one piece of glass. As the ribs may be numerous without increased expence, the

whole thickness of the glass traversed by the light may be reduced much less than in the case of worked ribs; so that less absorption of light is caused, yet with a glass surface considered sufficiently perfect and at a greatly diminished expence in the manufacture.

Hitherto the tendency has been to raise the optical character of the lighthouse apparatus on every point: to render the glass more clear & colourless; – to diminish the presence of striae and bubbles; & to perfect the form of the glass, and the character of its surface, so as to refract the rays of light properly and send them with as little deviation as possible on their way. I think this endeavour has been in the right direction. High perfection has a moral influence on the keepers and all concerned and helps to sustain the standard of lighthouse duty; – it is highly advantageous with the use of the ordinary oil lamp – and if hereafter, the employment of intense compressed sources of light, as the Electric lamp or the oxy lime light, should be successfully introduced, it will be of the utmost consequence to have the power of applying optical apparatus of diminished size, which can only be done by the use of perfect means & workmanship.

The following is an account of the pieces of glass sent to the Trinity House.

Pieces A/1 and A/2 being put together edge ways form a plano convex lens 20.2 inches in height and 10 inches in width having a focal distance of 11.8 inches. Besides the central lens portion, there are 16 rings either whole or in part round this center, making up the rest of the lens. As to the light it received from a lamp when in place it is equal to 80° in the vertical direction & 45° in the horizontal direction[.]

Pieces B/1 and B/2 being put together edgeways, form a pannel for a fixed light: – its height is $19\frac{1}{2}$ inches – its width $5\frac{1}{2}$ and its focal distance 8.87 inches. The ribs are horizontal and altogether are 32 in number. 95° vertical of light are received by this pannel[.]

Pieces D/1, D/2, D/3 and D/4, placed edge to edge, form a pannel for a fixed light having 19 inches height and a width of $8\frac{1}{2}$ inches. The focal distance is 6 inches. The ribs are of course horizontal. The whole number is 48, there being 8 in each of the middle pieces and 16 in each of the extreme pieces. The vertical angle of light received is 116° [.]

Pieces E/1 and E/2, placed edge to edge, make a pannel for a fixed light, 12 inches high and $6\frac{1}{2}$ wide. The focal distance is 5.25 inches. The ribs are of course horizontal and many of them very fine; there being 70 in the 12 inches of height[.] The vertical angle of light received is 96° .

All these pieces have been cast with pressure in moulds. In many parts the fire surfaces have been left upon them but as these have a certain slight irregularity of form (& perhaps degree of dullness) which tends to disperse light; wherever the surfaces are large enough to permit of it they have been brought under the tool & polished: not for the sake of giving form to the

piece as a whole, but of removing these minute irregularities of the surface, & rendering it clear & uniform.

The glass used is flint glass, it being softer when hot than plate glass, & so taking a sharper impression in the moulds. It is excellent in colour i.e. it is nearly colourless. Some pieces contain striae rather large in size, but the greater number are very good in that respect. Striae are more likely to occur in flint than in plate glass.

The chief point of comparison (speaking optically) between the pressed glass and the wrought or cut glass is in their power of *accurate refraction* so as to send the light ray in the required direction; which, excluding colour transparency striae &c, will depend upon the general angular form of the glass, and upon the degree of perfection & truthfulness of its surfaces[.] The refractive power of the moulded glass pieces was very good; and *remarkable*, considering that they are the result of very early attempts; but they will not compare except at a considerable distance, with well wrought cut glass. The difference is evident when a lamp is placed in the focus of the pieces, & the course of the outgoing rays observed. It is more evident when parallel rays, as those of the sun, are received upon the exterior of the pieces & their collection & concentration on a screen placed in the focus observed;— or when the rays from a distant electric lamp, with its intense but compressed light are employed in like manner, and the image in the focus examined. The pieces have been examined in all these ways but it will be sufficient to state the result with the Electric lamp only.

In order to have a point of comparison a lens of wrought glass was procured consisting of a central plano-convex part, & two concentric rings: it was 12 inches in diameter & had a focal distance of 9.75 inches. The electric lamp placed at the distance of 41 feet gave a very bright & concentrated focal image, not more than $\frac{1}{8}$ of an inch in diameter surrounded by a small circle of blue & another of red light due to the difference in refraction of the different rays; these effects showed that the lens was excellent in workmanship. When the same experiment was made with the pieces A/1 and A/2, the image in the focus was much more diffused & much less bright in the centre: — and though it is difficult to define the limit of an illuminated spot which gradually fades from the centre outwards, yet a space of from 2 to 3 inches broad was more or less considerably illuminated at that place & distance when the focal light was most compressed together and what may be considered as the brightest part was half an inch or more broad. The focal distance is given at 12 inches but I think the most compressed usage was probably at 13 inches; but the greatest compression is difficult to judge of through 2 inches or more A/1 A/2 The effects produced when an ordinary lamp was placed in the focus of each of these lenses & the outgoing rays were observed at a distance corresponded with the indication thus obtained[.]

It will be seen that as the focal distance of the wrought lens was 9.75 inches and that of the moulded lens 12 inches the former is in that respect under a greater disadvantage than the latter: – that as the angle of rays received by the former was 63° that number squared and multiplied by 0.7854 shews that 3120 may represent the light falling on it from a central lamp whilst as the vertical angle of the pressed lens was 80° and its horizontal angle 45° , 3600 may represent the light falling on it; so that as regarded external illumination the disadvantage was still, in that respect, against the wrought lens. Again the greatest thickness of the wrought glass was $1\frac{1}{2}$ inches whilst that of the pressed glass was only $\frac{6}{8}$ or only half as much. Notwithstanding these points the wrought glass did its work better and gave a much better & brighter image either inwards or outwards than the pressed glass.

The pieces B/1 B/2 are intended to give a fixed light: and with the external electric light the compression of the focal line is to be estimated at that spot of most compression within the pannel in the axial lens of the ray[.] As the pannel is $19\frac{1}{2}$ inches high and has a focal distance of only 9 inches, it might be expected, if of cut glass, to give a wider image in the focus than the cut lens above referred to did. Experimentally it gave a very diffused image being an inch wide vertically & the brightest part $\frac{3}{8}$ of an inch wide so far corresponding with the action of pressed lens above referred to, but I think the general effect of this pannel was more correct than that of the lens A/1.2.

The pannel D/1.2.3.4. gave the most compressed focal band at the distance of $5\frac{1}{2}$ inches; the given focus being 6 inches off. The band was pretty well defined at the edges, but broad, being $\frac{9}{16}$ of an inch wide. The upper & lower pieces of glass did not appear to be very good they added little to the general effect & when acting without the middle pieces gave a band of diffuse light more than 2 inches wide & having a different final distance to the two middle pieces. The latter were very efficient[.]

The pannel E/1.2. is like the former in character and gives its best focal image at $5\frac{1}{2}$ inches; its given radius being 5.25 inches. The whole band was $\frac{1}{2}$ wide, of which $\frac{2}{8}$ in the middle were brightest & the upper & lower portions less bright[.]

When a lamp is placed in the foci of these pannels they give corresponding results in the external illumination at a distance. When this distance was made in succession 41 & 98 feet B gave more light than D and D was better than E.

When the three catadioptric reflecting pieces C/1.2.3. where [sic] examined, they gave results, more irregular than, & inferior to those of the lens & pannel pieces reported above[.]

These results with the electric lamp shew generally what is the amount of divergence due to the condition & form of the surface of the glass, and they shew how much greater it is with the pressed than with the cut glass. The amount of divergence in a perfect lighthouse apparatus may be estimated approximately by the size of the flame & its distance from the refractors. To this divergence, which is generally & roughly estimated at about 6° , is to be added the divergence due to the glass which may make it 8° , 10° , 12° or more. All the glass divergence, even to the widest & fullest parts of the result, as with the upper & lower D pieces take away light from the chief place to be illuminated which we may consider as that within 6° ; but speaking generally, for because of the irregularity of action it is only so I can speak, if we assume that the divergence with a lens is increased from 6° to 8° then the intensity of the light seen at a distance is diminished from 64 to 36 or by nearly one half;— if the divergence is increased to 10° the intensity is diminished from 100 to 36 or nearly by $\frac{2}{3}$ rds; and if increased to 12° , only one fourth of the intensity of the light remains, the rest being dispersed by divergence[.]

Defective action of the kind described increases as the focal distance is lessened, and decreases as that distance is greater; and as I understand that the cost of the pressed glass will be very much less than for the cut glass, it allows of the use of a larger size for a smaller one, by which the errors belonging to the smaller size may be reduced and also the size of the flame increased. Therefore, in harbour lights and those corresponding to the third & fourth order, the proposition, if it ultimately succeeds is more likely to be carried first into effect. Increasing the flame with increase of apparatus diminishes the proportionate effect of the glass disturbance; but with the accepted magnitude of a first class light, the magnitude of the flame cannot be well increased; because of the then necessarily increased size of the lenses & other circumstances. On the other hand attempts to exalt the force of the light by increasing its intensity & diminishing its size as in the Electric lamp & accompanying that by a diminished glass apparatus would increase the proportion of glass error.

These specimens of cast glass are the earliest produce of the attempt to simplify and economize the manufacture of the optical part of a lighthouse. They have not yet been extended to the service of high & leading lights, and much experience & progress will be required before a judgement can be formed of the probable final result. In the mean time they are very good & important as steps and deserve every encouragement; and there is no doubt they will have that in France where their application for trial & proof has already been commenced¹.

M. Faraday

1. This letter was read to Trinity House By Board, 24 January 1860, GL MS 30010/42, p. 506. It was ordered that it be entered on the records as usual.

Letter 3714**Faraday to George Buchanan****19 January 1860****From the original in RI MS F1 N/1/33**

[Royal Institution embossed letterhead] | 19 Jan 1860

My dear George,

I forgot yesterday to ask you whether you would find an increase of the loan convenient; but suppose as you said nothing about it you did not desire it. Now I send you a cheque for £5.– as a remembrance gift from us on sta[r]ting¹_[.] You will easily give it shape in any way which will remind you of our affectionate love & earnest wishes for your welfare & happiness: I speak jointly for your Aunt & myself_[.]

Ever My dear George | Your Affectionate Uncle | M. Faraday

Address: Geo. Buchanan Esq

1. Presumably for India.

Letter 3715**John James Waterston¹ to Faraday****20 January 1860****From the original in IET MS SC 2**

26 Royal Crescent Edinburgh | 20 Jan 1860

Sir,

In a paper that I am drawing up for the Philosophical Magazine (entitled – “*On the Gradient of Density in saturated vapours and its development as a physical relation between bodies of definite chemical constitution* [”]²) – I have made use of your observations on the condensable gasses that appeared many years ago in the Phil: Trans:³ To do so with effect I have to reduce the temperatures to the standard scale of the air thermometer_[.]

The following is the extract from your memoir describing the means employed to measure the low temperatures obtained “*In order to obtain some idea of this temperature (i.e. the temperature of the liquid carbonic acid and ether bath) I had an alcohol thermometer made, of which the graduation was carried below 32°F by degrees equal in capacity to those between 32° & 212°*”⁴_[.]

I have assumed that the length on the scale of this thermometer corresponding to 10° was uniform and that it was found by comparing the reading with a thermometer of mercury between 0° & 32°_[.] On this assumption we can reduce the temperatures very precisely to the air thermometer by means of M. Pierres⁵ observations on the expansion of absolute alcohol⁶ – The results exhibit your observations at these extremely low temperatures to be in conformity with the general law of density of saturated vapours_[.]

I shall be highly obliged if you can inform me if I am right in assuming that the scale of your thermometer was thus determined or was it formed as usual with mercurial Thermometers viz: by plunging into melting ice and boiling water & dividing the interval into 180 equal parts for degrees[.]

It is perhaps too much to expect you to remember such particulars so far back, but it is of so much importance in establishing a general principle in science that I have ventured to intrude[.]

I am | Sir | Your obedient humble servant | J.J. Waterston

Address: Dr. Faraday | Royal Institution | Albemarle Street | London

1. John James Waterston (1811–1883, ODNB). Scottish man of science, who developed an early kinetic theory of gases.
2. This paper does not appear to have been published.
3. Faraday (1845).
4. *Ibid.*, 158. The section in parentheses was inserted by Waterston.
5. Joachim Isidore Pierre (1813–1881, P2, 3). Professor of Chemistry at Caen.
6. Pierre (1848).

Letter 3716

Faraday to Aspasia Paola A. Lega Fletcher¹

23 January 1860

From the original in University of Leeds Library, Brotherton Collection, miscellaneous letters (Lega–Fletcher autographs)

23 Jan 1860

Mr. Faraday presents his compliments to Mrs. Fletcher and returns the MS. If Dr. Guiseppe² expects to realise a large sum by the papers &c he is probably very much mistaken. Italy would probably offer as large a price as any other country[.]

1. Aspasia Paola A. Lega Fletcher (d.1890, age 73, GRO). Language teacher. See 1861 Census, TNA RG9/106, f. 13, p. 22.
2. Unidentified.

Letter 3717

Faraday to Austen Henry Layard¹

25 January 1860

From the original in Kurt Wolfgang Vincentz

25 Jan 1860

My dear Sir,

If you are at home the bearer will bring away the diagrams – or he will call at any time you like for them²[.]

Ever Truly Yours | M. Faraday

A.H. Layard Esqr | &c &c &c

1. Austen Henry Layard (1817–1894, ODNB). Archaeologist and politician.
2. See letter 3735.

Letter 3718**Faraday to Angela Georgina Burdett Coutts****25 January 1860¹****From the original in BL Burdett-Coutts papers**

Dear Miss Coutts,

We last night enjoyed ourselves under your kindness and I thank you heartily for the enjoyment & your kind note – My wife was not able to go for she is not strong – and she thanks you very much for your remaining offer. We do not often go to the theatre and I have no doubt that your concurrent kindness makes it more tempting than any attraction the theatre would have of itself[.]

Mr. Barlow has told me of your munificence at Oxford & the thought that you & Mr. Pengelly² have of speaking (by him) of it & the fossils here³. I shall rejoice if we can profit intellectually by the matter & equally rejoice if we can in any way be useful in so forwarding instruction in however minor a degree. We know with whom we trust in trusting you & Mr. Pengelly and are sure that any thing resulting cannot be wrong to Oxford minds. However I know so little of University feeling that I have no right to say a word upon such a point[.]

Ever Your Very faithful Servant | M. Faraday

I think of Mrs. Brown whenever I write to you but my memory is so treacherous that it renders me afraid & I hesitate.– If with you may I send my kindest remembrances

MF.

1. Dated on the basis, first, that Faraday had attended the pantomime referred to in letter 3711 and, second, that Coutts would donate Pengelly's geological collection to the new University Museum in Oxford in February 1860. See *The Times*, 9 February 1860, p. 12, col. c.

2. William Pengelly (1812–1894, ODNB). Devon geologist.

3. Pengelly, W. (1860), Friday Evening Discourse of 25 May 1860.

Letter 3719**Peter Henry Berthon to Faraday****31 January 1860****From the original in GL MS 30108/3/103**

Trinity-House, London, EC | 31 Jan 1860

Sir,

Mr. Alexander Gordon having applied for leave to peruse your Report on the Moulded Lenses received from France¹, I am directed by the Elder Brethren to request you will acquaint me whether you have any objection to his being allowed to do so².–

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday | &c &c &c

1. Letter 3713.

2. On this see Trinity House By Board, 31 January 1860, GL MS 30010/42, p. 518.

Letter 3720**Peter Henry Berthon to Faraday****31 January 1860****From the original in GL MS 30108/3/104**

Trinity House, London, EC | 31 Jan 1860

Sir,

I am directed by the Elder Brethren to transmit to you the accompanying plan, Specification and form of Tender for the optical part of a First Order fixed Dioptric Apparatus, and shall be glad to receive for their information, any observations you may have to offer thereon,— and whether in your opinion it is advisable for the Elder Brethren to adopt the proposed arrangement of the Armature.

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday | &c &c &c

Letter 3721**Faraday to Peter Henry Berthon****1 February 1860****From the original copy in GL MS 30108/3/103**

Royal Institution | 1 Feb 1860

Sir,

I have no objection to Mr. Gordon's (or any other person's) perusal of my report on moulded glass¹:— at the same time I think he ought not to take any copy of it, or communicate it to any other person, without the express leave of the Trinity house, *whose property it is*:— and I should object to its being used in any way for mere commercial purposes.

I am Sir | Your obedient humble Servant | M. Faraday
P.H. Berthon Esq

1. Letter 3713. See letter 3719.

Letter 3722**Peter Henry Berthon to Faraday****1 February 1860****From the original in GL MS 30108/3/106.2**

Trinity-House, London, EC | 1 Feb 1860

Sir,

I am directed by the elder Brethren to forward to you the accompanying Copy of a Letter from the "Lime Light Company"¹ proposing to try the effect of that system of producing Light in one of this Corporation's Light Houses,— and I am to request that you will favor the Elder Brethren with any

observations or suggestions on the subject, which may occur to you as useful in the consideration of this Proposal.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq. – F.R.S. | &c &c &c

1. Baxter to Berthon, 30 January 1860, GL MS 30108/3/111.

Letter 3723

Faraday to Peter Henry Berthon

7 February 1860

From the original copy in GL MS 30108/3/104

Royal Institution | 7 Feb 1860

Sir,

In reference to the plan & specification accompanying your letter of the 31 January¹. I have seen Messrs Walker & Co thereon and in conjunction with them have agreed on certain changes to avoid shadow as much as possible – I have also seen the amended plan & drawing which I approve of, but have returned it to Messrs Walker & Co for the emendation of two clerical errors and have asked them to send it when so amended to you²[.]

I have the honour to be | Sir | Your faithful humble Servant |
M. Faraday
P.H. Berthon Esqr | &c &c

1. Letter 3720.

2. This letter, which related to the Smalls and Dungeness lights, was read to Trinity House Wardens Committee, 14 February 1860, GL MS 30025/27, p. 294–5. It was agreed to invite tenders on the basis of the modifications.

Letter 3724

Faraday to Peter Henry Berthon

7 February 1860

From the original copy in GL MS 30108/3/105

Royal Institution | 7 Feb 1860

Sir,

Several years ago I took a wind guard to the Needles point lighthouse for experiment No.49. – p16 – June 1849¹ also letter date 8 Decr.1849², with the view of replacing the ordinary cowl by it: I was so satisfied with the results then, & have been so much confirmed in that view by observation since, that I beg to ask for any opportunity that may arise to apply the wind guard on the *full scale*; with a view to the more perfect & certain ventilation of the lanthorn and the replacement of all the cowls by wind guards (if the result justifies my expectation) as occasion may arise³[.]

I am Sir | Your Very faithful humble Servant | M. Faraday
 P.H. Berthon Esqr | &c &c &c

1. See Faraday to Herbert, 30 June 1849, letter 2204, volume 4.
2. Faraday to Herbert, 8 December 1849, letter 2238, volume 4.
3. This letter was read to Trinity House By Board, 14 February 1860, GL MS 30010/42, p. 529. It was referred to the Lights Committee who agreed that this would be used on the Portland light. See Trinity House Wardens Committee, 13 March 1860, GL MS 30025/27, p. 323.

Letter 3725

Faraday to Peter Henry Berthon

8 February 1860

From the original in RI MS F1 N/5/2

Royal Institution | 8 Feb 1860

Sir,

In reply to your letter of the 1st instant¹, I have drawn up a set of enquiries which, in my opinion, it would be necessary to make, and have answered, *before* any proposal to introduce the lime light into a lighthouse could be considered. These I beg to submit to the consideration of the Deputy Master² and Elder brethren. As far as I am concerned, such preliminary enquiries and answers would be required in every like case.

I am Sir | Your faithful humble Servant | M. Faraday
 P.H. Berthon Esq | &c &c &c
 Enquiries &c

1 The Trinity House cannot undertake to consider a mere proposition, or an imperfect arrangement; but only such subjects as are presented to them in a practical state.

2 The T.H. cannot undertake to work out, or perfect, the application of a proposition, though it may feel justified in undertaking to test a perfected arrangement, upon good reasons and data being submitted to it

3 In order to enable the T.H. to judge whether a proposition made to them by parties, not practically experienced in the service of lighthouses, is well founded and has been thoroughly considered, it is desirable that certain questions should be answered; not from expectation only, but upon principle and from experience:— These, in the present case, may be founded upon the requisites for a *first order fixed light*; and may be as follows.

[1]³ 4 What is the quantity of light proposed to be supplied; expressed, either in relation to a central Fresnel lamp of four wicks, or to an ordinary Argand burner such as is used in a reflector?

[2] 5. The number of jets required to ignite the lime?

[3] 6. The quantity of oxygen required for 12 hours?

[4] 7. The current price of the oxygen? i.e. the cost of material, wages, repairs, and any other current expence.

[5] 8. The manner of storing the oxygen?

[6] 9. The nature of the gas fuel? – the quantity required in 12 hours?

[7] 10. How is the gas fuel to be obtained? How is it to be stored?

[8] 11. What is its current price for 12 hours; including materials, wages, repairs, and other current expences?

[9] 12. What is the shape and size of the lime or focal light? How often will it require renewal? – How, or where, is it to be obtained? – and what will be its current cost?

[10] 13. What degree of steadiness will the light possess? Is it *now* as steady as a well burning lamp, or is it unsteady, like a lamp in draughts? – or does it sink and rise at intervals?

[11] 14. Will the attention of the keeper be, of necessity, perpetual? – If not, for what intervals has the light been left, as yet, without falling off in character?

[12] 15. What is the vertical height, and horizontal width of the *most* intense part of the luminous object? – and what the height of the part which may be called, generally, intensely luminous?

[13] 16. What number of persons would it be necessary to employ *upon the spot*, in relation to the lighthouse? – and what would be their occupations?

[14] 17. What buildings or outhouses for retorts, gazometers, &c, and what habitations besides, those now belonging to a first order lighthouse, would be required?

[15] 18. Are the means of obtaining the light considered as applicable, only in favourable situations, or in all ordinary situations? In the latter case, let the reply have relation to such a lighthouse as that at Dungeness or Flamboro' Head.

[16] 19. Are there any exceptional cases where the light could not be applied with advantage; – as the Needles – Eddystone – Bishops – Longships – Plymouth breakwater – Casquets – Longstone – Bell rock – Skerryvore – Stack – Smalls – Tuskar – and others? If so, are they supposed to be few or numerous?

[17] 20. Will the service of the lime light make it dependent upon the neighbourhood of a town? – or if not so dependent, what kind of annual supplies, – or supplies at considerable intervals, will probably be required?

[18] 21. What will be the probable *outfit* of the apparatus with the building necessary for it and for the accommodation of the extra staff required?

[19] 22. What will be the nature of the necessary repairs?

[20] 23. What will be the *whole current expence* of the application of the lime light? (Including Royalty &c)

The T.H. cannot authorize any chance of interference with the *certainty of lighthouse action* by the introduction of any uncertain or unproved arrangements tending to disturb the actual service of the light. It therefore requires full proof of the fitness of any proposed arrangement, *before* considering its introduction into a lighthouse[.]

Any failure in such preliminary proof, or any serious departure in the results, from the answers given to the questions 4. 5. 6. 7. 9. 10. 11. 12. 13. 14. 16. 17. 18. 20. 23, which can be as well obtained out of a lighthouse as in it, will be considered as shewing that the proposed application has not been sufficiently matured;— and if not removed by further investigation and proof, will lead to the conclusion, that the method is not applicable to the service of lighthouses⁴.

M. Faraday

Endorsement: No. 661 | Universal Lime Lt. | Professor Faraday submittd set of preliminary enquiries which will be also suitable to similar cases.— | 8. Feb: 1860. | 15/2/60. | (Enter in Scientific Reports) Folio 346 | Board 14 Feb 1860 communicated the points of Enquiry as suggested by Mr. Faraday to Mr. Baxter⁵.

1. Letter 3722.

2. Robert Gordon.

3. The numbers in square brackets for the remainder of this letter were added in pencil in another hand.

4. This letter was read to Trinity House By Board, 14 February 1860, GL MS 30010/42, pp. 528–9. The heads of enquiry were approved.

5. Stafford Squire Baxter (d.1880, age 55, GRO). Secretary of the Universal Limelight Company, Baxter to Berthon, 30 January 1860, GL MS 30108/3/111.

Letter 3726

Joseph Toynbee¹ to Faraday

10 February 1860

From the original in RI MS Conybeare Album, f. 21

18 Savile Row. Burlington Gardens. W | 10 Feb 1860

Dear Mr. Faraday,

Having been unsuccessful in my attempts to obtain a ticket for Mr. Huxley's lecture to night I shall esteem it a favour if you can give me one².

Believe me yours sincerely | & obliged | Joseph Toynbee

1. Joseph Toynbee (1815–1866, ODNB). Ear surgeon.

2. Huxley, T.H. (1860), Friday Evening Discourse of 10 February 1860.

Letter 3727**Peter Henry Berthon to Faraday****14 February 1860****From the original in GL MS 30108/3/108**

Trinity House, London, E.C | 14 Feb 1860

Sir,

I am directed to transmit to you the accompanying Six Samples of Red Lead, and to request you will be so good as to analyse the same, and favour me with the result for the Board's information.—

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday | &c &c &c

Letter 3728**Faraday report to Trinity House****20 February 1860****From *Parliamentary Papers*, 1862 (489), LIV, p. 9**

Royal Institution | 20 Feb 1860

I went to Dover last Monday (the 13th instant); was caught in a snow storm between Ashford and Dover and nearly blocked up in the train; could not go to the lighthouse that night; and finding, next day, that the roads on the downs were snowed up, returned to London. On Friday I again went to Dover and proceeded by a fly that night, hoping to find the roads clear of snow; they were still blocked up towards the lighthouse, but by climbing over hedges, walls, and fields, I succeeded in getting there and making the necessary inquiries and observations.

The light was of the same fine character as when I last saw it; shining over the country and the sea. It was generally very steady, but with slight interruptions now and then from iron in the carbons. I found a new keeper in the lantern (Griffiths¹); he had learned how to attend the lamp readily and effectually. The light has a tendency to sudden and spontaneous extinction. This arises from the breaking off of the end of the carbon, or from circumstances interfering with the delicacy of the fine mechanical work of the lamp. It happens two, three, or four times every night; and being once extinguished, the lamp does not re-light itself. The slightest touch of the keeper's hand is enough to restore the light; but the liability causes an anxious watchfulness on the part of the attendant, who dares not descend to the guard-room, but is constrained to stop in the lantern continually. The light has never been stopped, as yet, by any deficiency of action in the machine room.

I found the two engines in the machine-room, and also both the steam-engines, and the magneto-electric machines, properly at work; the latter had not become deranged or given way at any point, and the magnets appeared by the results to be as powerful as ever. One of the steam-engines had met

with an accident; the guide of the piston-rod broke, and the engine ceased to work; but fortunately this took place in the morning, and the engineer was able during the day to establish a temporary guide, which enabled the engine to go on working at nights until the repair was fully made. The water condensers and other parts acted properly, and as they were required.

Though I was not at the lighthouse on the Monday night, yet I could see from Dover that the light was at that time in its proper and regular condition².

(signed) M. Faraday

1. John Griffiths (d.1906, age 75, GRO).

2. This letter was read to Trinity House By Board, 21 February 1860, GL MS 30010/42, p. 541. It was agreed to defer discussion until the following meeting. At Trinity House By Board, 28 February 1860, GL MS 30010/42, pp. 552–3 Faraday was present and it was agreed that he would report about continuing the use of the magneto-electric light (letter 3733) and that it should be tested in a revolving light.

Letter 3729

Lyon Playfair to Faraday

20 February 1860

From the original in IET MS SC 2

Private

Royal Society of Edinburgh, Edinburgh | 20 Feb 1860

My dear Dr. Faraday,

Lord Brougham¹ & Mr. Gladstone² are coming here in Easter Week to be installed as Chancellor & The Rector of Edinburgh University & the Senate are desirous to Confer the Title of LLD on a few distinguished savans on the occasion, on the condition that they will come to the Installation & receive their Diploma in person³[.]

There is a strong feeling among my Colleagues to confer the Doctorate on You. Could you make it convenient to come down about the 15th or 16 April to attend the Ceremony. If so then no doubt the Senate on Saturday⁴ next will have great pleasure in nominating You with perfect unanimity.

Of course I shall Expect the pleasure of having You as an Inmate in My House during the time You may remain in Edinburgh –

Yours Sincerely | Lyon Playfair

1. Henry Peter, Lord Brougham (1778–1868, ODNB). Whig politician and scientific writer.

2. William Ewart Gladstone (1809–1898, ODNB). Liberal MP for Oxford University and Chancellor of the Exchequer, 1859–1865.

3. Faraday did not receive this degree.

4. That is 25 February 1860.

Letter 3730**Peter Henry Berthon to Faraday****22 February 1860****From the original in GL MS 30108/3/108**

Trinity House, London, E.C | 22 Feb 1860

Sir,

I am directed to transmit to you the accompanying three samples of White Lead, and to request you will be so good as to analyse the same, and favor me with the result for the Board's information¹.–

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday | &c &c &c

1. Faraday's notes of the analysis are on the back page of this letter where he noted that he replied on 5 March 1860. It was noted at Trinity House Wardens Committee, 6 March 1860, GL MS 30025/27, pp. 315–16 that the analyses were satisfactory and it was agreed that the lowest tender should be accepted.

Letter 3731**Faraday to Peter Henry Berthon****23 February 1860****From the original copy in GL MS 30108/3/108**

Royal Institution | 23 Feb 1860

Sir,

Having analyzed the six samples of Red lead labeled (as above¹) I do not find in any of them any adulteration or substance added to make weight. They are in that respect unobjectionable & appear to me to be alike in preparation and well manufactured articles – but there may be reasons for preference in relation to their use dependant upon the manner in which they are prepared of which I am not a judge.

I am Sir | Your Very faithful Servant | M. Faraday
P.H. Berthon | &c &c &c

1. This refers to Faraday's notes on the analysis. See letter 3727.

Letter 3732**Faraday to Royal Commission on Lighthouses****25 February 1860****From the original copy in GL MS 30108/5, f. 6–7**

Royal Institution | 25 Feb 1860

Reply to Printed enquiries on the Royal Commission of Lights (23 Feb 1860.) – Buoys & beacons 7, Millbank Street S.W.¹

See the printed form & its numbers.

1. Michael Faraday. Royal Institution. In 1836 I was appointed "Scientific adviser to the Corporation of the Trinity House in experiments on lights[.]" Since then a large part of my attention has been given to the lighthouses in respect of their ventilation their electric arrangements & conductors the impurity & cure of waters – the provision of domestic water the examination of optical apparatus &c the results of which may be seen in various reports to the Trinity house:– but a very large part of my consideration has been given to the numerous propositions of all kind[s] which have been & are presented continually to the Corporation[.]. Few of these present any reasonable prospect of practical & useful application and I have been obliged to use my judgment chiefly in checking imperfect & unsafe propositions rather than in forwarding any which could be advanced to a practical result. Hence I cannot give simple answers to the queries beneath and therefore think it better to refer when there is occasion for it to my carefully considered communications to the Trinity house. The Royal Commission may not think it necessary to refer to any or at all events not to many of these papers but as an illustration of my position & duty I will refer in the first instance to a letter of enquiries &c. of the date 8 Feby 1860, relating to Prosser's² lime light³[.]

2. Not as yet.

Gurney oxyoil lamp – Reports 15 Feby 1837. – 15 Jany 1838 – 14 June 1838 – 29 Octr 1838 – 12 Aug 1839⁴. great exertions were made to perfect this lamp but its application failed

FitzMaurice lime light. Letters to Mr. Berthon of the 21 July 1858⁵ – 20 August 1858⁶

Prosser's lime light – Letter & inquiries 8 Feby 1860⁷

*Watson's*⁸ *Voltaic light* – report 15 Aug 1854⁹

Way's mercurial Electric lamp 27 June 1859¹⁰.

Holmes Magneto-Electric light His letter 28 April 1857 – My comments 1 May 1857¹¹ My report, 29 April 1859¹² – also 20 Feby 1860¹³

Pyrotechnic mixtures & the *association of steam with the lamp* have been proposed – to the reports on which I do not refer

3. No.

4. The best *at present* are the refracting & reflecting apparatus now in use

5. The best *at present* are the arrangements now in use

6. Blank.

7a Blank

7b 7c Moulded glass. Letter to Trinity house 16 Jany 1860¹⁴

8 No

9 blank

9b. Propositions as to reddening the light, 12 [sic] July 1837¹⁵, 17 March 1843¹⁶, 26 Decr 1845¹⁷. – 14 Octr 1859¹⁸.

10a red from white.

10b. White light is more serviceable & penetrating whilst white than if reduced by the intervention of coloured media

11a 11b. Uncertain approximations because of the darkness, the unknown haziness of the atmosphere & the refractions

12a blank

12b. blank

13. Such improvements in Ventilation, or in other points as have occurred to me have been already introduced

14 blank

15a 15b blank

16a 16b blank

16.c. Messrs Brown¹⁹ ignited Platinum wire 17 Decr 1847²⁰.

17. blank

18. 19 20

Parabolic reflector

16 Aug 1848²¹

Whistle — —

24 March 1853²²

22 Sept 1853²³.

Electro magnetic arrangement

6 Octr & 16 Octr 1857²⁴.

Boulogne fog bell & reflector

24 Octr 1859²⁵.

Start point reflector —

21 November 1859²⁶.

21. Depend[s] upon the locality

22. I think each locality requires special consideration

23. blank

24 blank

25a. blank

25b. blank

1. This is Faraday in response to the numbered questions of Circular IX issued by the Light-house Commission for scientific men and manufacturers. At the end of this letter is printed Faraday's answers to Circular X. This is not given here as the text is in letter 3750. The texts of the questionnaires are in *Parliamentary Papers*, 1861 [2793], XXV, at the beginning of volume 2.

2. Unidentified.

3. Letter 3725.

4. These reports of Faraday are respectively in GL MS 30108/A1, pp. 27–42, 43–51, 52–6, 57–75 and 76–92 and will be published in an appendix to volume 6.

5. Letter 3484.
6. Letter 3505.
7. Letter 3725.
8. Joseph John William Watson (d.1886, age 55, GRO). Worked on electrical light; described as a civil enginner in 1871 census, TNA RG10/207, f. 8, p. 8. See also James (1997), 294.
9. Faraday report to Trinity House, 15 August 1854, letter 2878, volume 4.
10. Letter 3609.
11. Letter 3279.
12. Letter 3590.
13. Letter 3728.
14. Letter 3713.
15. This report by Faraday is in GL MS 30108/4/128 and will be published in an appendix to volume 6.
16. Faraday to Herbert, 17 March 1843, letter 1479, volume 3.
17. Faraday to Herbert, 26 December 1845, letter 1810, volume 3.
18. Not found.
19. Edward Brown and James Brown. Buoy makers in Preston.
20. Faraday to Brown and Brown, 17 December 1847, and Faraday to Herbert, 17 December 1847, letters 2038 and 2039, volume 3.
21. Not found.
22. Faraday to Herbert, 24 March 1853, letter 2660, volume 4.
23. Faraday to Herbert, 22 September 1853, letter 2733, volume 4.
24. Letters 3340 and 3345.
25. Letter 3661.
26. Letter 3685.

Letter 3733

Faraday report to Trinity House

28 February 1860

From *Parliamentary Papers*, 1862 (489), LIV, pp. 9–10

Royal Institution | 28 Feb 1860

The appointed time, during which the Magneto-electric Light was to be placed under practical trial at the South Foreland, having come to an end, I may be allowed to say that it has, so far, justified itself in its results, as to make me hope that the Trinity House will see fit to authorise its application, either there or somewhere else, for a further and a longer period. The light has proved to be practical and manageable; and has supplied the means of putting into a lighthouse lantern, for six months or more, a source of illumination far surpassing in intensity and effect any other previously so employed. I do not know at what cost this result has been obtained, but unless that is very great indeed, I am of opinion that a large increase upon the expense of the old method (estimated upon the quantity of light obtained) ought to be permitted in the first establishment of a mode of illumination, which apparently promises many improvements during its further development¹.

(signed) M. Faraday

1. This letter was read to Trinity House Court, 6 March 1860, GL MS 30004/28, pp. 61–2. It was referred to a joint committee of Wardens and lights.

Letter 3734**Faraday to William Whewell****2 March 1860****From the original in TCC MS O.15.49, f. 42**[Royal Institution embossed letterhead], Albemarle St. W |
2 Mar 1860

My dear Dr. Whewell,

Let me thank you very truly & earnestly for your kindness in sending me a remembrance under the form of your last volume on the Philosophy of Discovery¹ – I wish I had something new & good to send you in return[.]

Ever faithfully Yours | M. Faraday

1. Whewell (1860).

Letter 3735**Austen Henry Layard¹ to Faraday****3 March 1860²****From the original in RI MS Conybeare Album, f. 41**

130 Piccadilly | 3 Apr 1860 [sic]

Dear Professor Faraday,

I must apologise for not having sent you long ere this the diagrams³ used in my lectures of last year⁴ and for which the Institution paid. I have been absent from London nearly the whole spring. I am fortunate, however, in being in Town on Friday night and hearing your most interesting lecture on lighthouses⁵. I hope you have not been victim to influenza like so many.

Yours very sincerely | A.H. Layard

1. Austen Henry Layard (1817–1894, ODNB). Archaeologist and politician.
2. Dated on the basis of the reference to Faraday's Friday Evening Discourse.
3. See letter 3717.
4. Layard had delivered, after Easter 1859, a course of seven lectures on 'The Seven Periods of Art', RI MS Le4/223.
5. Faraday (1860a), Friday Evening Discourse of 9 March 1860.

Letter 3736**Faraday to Josiah Latimer Clark****5 March 1860****From a photocopy in RI MS**

[Royal Institution embossed letterhead] | 5 Mar 1860

My dear Sir,

My subject next Friday is the lighthouse¹[.] I am sorry that I have not power to offer you a ticket either to use or burn but I am asking Members for a ticket if they have one to spare.

The experiment you describe is a very interesting one & I thank you heartily for you kind offer which however I cannot avail myself of this time as my subject is far away from it. I think I heard Mr. Barlow speak as if he hoped that you will or would tell us some of your stores of knowledge² & that gives me some hopes that *you* will describe & shew the experiment here.

Ever My dear Sir | Truly Yours | M. Faraday
Latimer Clarke Esq | &c &c &c

1. Faraday (1860a), Friday Evening Discourse of 9 March 1860.
2. Clark, J.L. (1861), Friday Evening Discourse of 15 March 1861.

Letter 3737

Alfred Austin to Faraday

6 March 1860

From *Parliamentary Papers*, 1860 (309), XL, pp. 1–2

Office of Works, &c | 6 Mar 1860

Sir,

I am directed by the First Commissioner of Her Majesty's Works¹, &c., to request that you will have the goodness to examine, in concert with Sir R. Murchison, the stonework of two bays of the river front of the New Palace at Westminster which has been indurated by two different processes, respectively applied by Mr. Ransome and Mr. Szerelmey, and to report to him which of those processes you consider best adapted to the purpose, and whether you can recommend either of them for general use in the building².

Mr. Phipps³, assistant surveyor of works, of this department, will point out to you the two bays above referred to.

I am, &c. | (signed) Alfred Austin | Secretary
Professor Faraday

1. William Francis Cowper.
2. See letters 3610, 3613, 3614 and notes.
3. John Phipps (c.1794–1868, *Builder* 1868, 26: 117). Assistant Surveyor of Works and Public Building in London.

Letter 3738

Faraday to Roderick Impey Murchison

7 March 1860

From the original in GS MS M/F4/6

[Royal Institution embossed letterhead] | 7 Mar 1860

My dear Sir Roderick,

It seems by a letter I have had from the Office of Works¹ that you and I are to examine in concert certain stone work preparation at the New Palace

of Westminster. I shall be out of town *to day* and entirely occupied on *Friday* with my F.E. subject² – but I could go tomorrow morning being back here *by two o'clk* or I could go on Saturday Morning. Will either of these suit you? If so at what hour and where shall we meet? at your Museum or the Office of Works or at Westminster. Mr. Phipps³ the assistant Surveyor of Works is to show us two particular Bays.

Ever My dear Sir Roderick | faithfully Yours | M. Faraday

Endorsement: Faraday & self to examine exterior of Houses of Parliament 1860

1. Letter 3737.

2. Faraday (1860a), Friday Evening Discourse of 9 March 1860.

Letter 3739

George Biddell Airy to Faraday

9 March 1860

From the original in IET MS SC 2

Royal Observatory, Greenwich, London, S.E | 9 March 1860

My dear Sir,

I wish to consult you about the location of magnetism in a steel bar – viz. a dipping needle¹.

We have one needle of our dipping-needle-apparatus which without any visible reason gives us a great deal of trouble. Its results are very discordant, but the means of results (in masses) come out very fairly: *but*, they differ very steadily from the mean results of other needles by about $\frac{1}{2}$ a degree.

The use of a dipping needle rests entirely on the assumption that, when the magnetism is reversed by the ordinary double-touch process, the new poles lie in the same line in which the old poles lay.

Now do you think it likely that there may be such perverse heterogeneity in the steel of the magnet that, when it is infected with one kind of magnetism, the magnetic axis may tend (steadily on the whole, though with some irregularity) to lie in one line: and that, when it is filled with the opposite magnetism, the magnetic axis may (steadily on the whole) be disposed to lie in a different line, inclined to the former at an angle of 1° ?



As thus:– B and A the boreal and austral poles at first, B' and A' the boreal and austral poles after reversion?

I am, my dear Sir | Yours very truly | G.B. Airy
Professor Faraday | &c – &c – &c

1. On this see Airy, W. (1896), 239.

Letter 3740

William Thomson to Faraday

10 March 1860

From the original in IET MS SC 2

2 College, Glasgow | 10 Mar 1860

My dear Faraday,

After long waiting I have been able to try some of the experiments we spoke of with the aid of an electrometer which makes them easy. It is one I have had made, to serve for the self recording atmospheric apparatus at Kew¹, and in the course of a week's trials I have had of it, I have used a glass fibre much lighter than that which will give it a metallic sensibility for the work for which it is designed. In the condition in which I have used it, it has given as much as 110 divisions on each side of zero (each divn. $\frac{1}{40}$ i., on a scale 22i from the mirror) is the effect of direct & reverse applications of a single cell of Daniell's battery. This effect it shows perfectly through my whole lecture room (holding about 100) with only a paraffine lamp. With a good mirror & the electric light, there would I am sure be no difficulty in removing the scale far enough to double the sensibility & yet good in effect visible through the whole of your theatre in the Royal institution. Thus the electroscopic effect of a single thermoelectric bism[uth]-antimony element could easily be shown.

In showing it to my class I preferred a rather less sensibility – giving about 60 divns (or an inch & a half) as the effect of a single cell of Daniell's.

If I lay a disc of copper on the table beside the instrumen[t] & make a good metallic communication between it (the copper) & the metal case of the electrometer, and then place a zinc disc resting on it, but separated from it by three thin pieces of glass, and connect the zinc by a wire with the insulated halfring, I find a deflection shown by the spot of light gradually supervenes. If a metallic arc is applied to make a communication betw. the disc & copper the spot comes back to zero, & again gradually moves when the zinc is left supported on the pieces of glass. If pieces of paper are substituted for the

glass the same thing take place but more rapidly. If the paper is wet, or if a drop of water is inserted between the zinc & copper discs so as to touch each while the glass separating pieces are retained, the something takes place instantaneously (altho after a metallic commn. is established, it takes a little time before the full deflection is again reached.) I believe when properly tried the final amount of the deflection will be the same in each case, although I have never got it quite so much with the glass or the paper, as with the drop of water. In fact the glass if perfectly dry would not in years conduct, by proper conductivity through its own substance, as much as passes in a few minutes or even when in its ordinary moist condn. in the atmosphere: and the pieces between the discs only differ from the glass supports of the insulated halfring in being in this moist state instead of in an artificially dry atmosphere. I think I can now answer with certainty your question as to the condition of perfectly unelectrified zinc & copper approaching before contact². They will exercise no force whatever on an electrified body in their neighbourhood; but the moment a metallic arc is placed connecting them, such a body, if free will move from the zinc or from the copper, according as it is positively or negatively electrified itself. I hope to show a single experimental demonstration of this; but in the mean time I infer it from a combination of what I have seen.

The two half rings of my electrometer are of one metal (brass.) If large discs of copper and zinc are connected respectively with the two half rings (one of which I always chief in metallic communication with the case,) and of these discs of zinc & copper are first well discharged at a distance & then (one of them being next well insulated) if they are gradually approximated to one another, the spot of light gradually moves so as to show zinc negative. When they are within a short distance, say $\frac{1}{10}$ of an inch, the deflection does not increase sensibly on their being brought closer & closer; but when they are brought into contact, it comes to its zero position suddenly. The maximum reflection here is I believe exactly the same as the true zinc copper single fused element. Again, if I substitute for the two half rings of the electrometer a compound ring of zinc & copper in the places of the two halves respectively soldered together, and if I give necessarily equal measured positive & negative charges to the index, the spot of the light moves to one side & the other alternately so as always to show the zinc pos. to the copper neg. The amount of the deflection I believe will be found to be the same as that of the single zinc copper water element, tested with the electrometer [word illegible] ordinary iron & needle always charged to the same degree.] Hence I infer that if the zinc half ring was connected with the zinc disc in the previous expt. & if a true discharge of the half ring were made before beginning, the spot of light would not stir, on the two discs being approximated. At the instant of the two discs touching

one another, it would move so as to show the deflection demonstrated by the second expt.

I am quite ashamed when I look through what I have written, and finding the appearance irremediably bad, I have only endeavoured as far as possible to make it legible. I feel reluctant to trouble you with it, but I send it as you kindly expressed an interest in some of what I was doing before on the subject.

Mrs. Thomson is feeling much better than she has done for a long time. She sends her kindest remembrances. When I saw Mr. Crawford last a few days ago I told him that I was going to write to you and he asked me to say that his family are all well & that he hopes you are so also.

Believe me Yours most truly | William Thomson

1. See Thomson (1859, 1860a) and letters 3664, 3665 and 3667.

2. In letter 3693.

Letter 3741

Faraday to William Francis Cowper

12 March 1860

From *Parliamentary Papers*, 1860 (309), XL, p. 2

Royal Institution | 12 Mar 1860

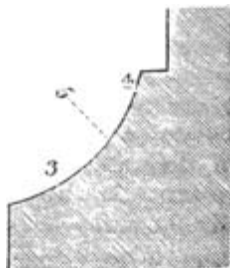
Sir,

In relation to the preservation of the stonework of the New Palace of Westminster, I hope you will do me the favour to refer to a letter on the subject of Mr. Szerelmey's process, which I wrote to the late Chief Commissioner¹ on 1st July 1859²; it will make it unnecessary at this time to refer to anything more than the two bays, to which the attention of Sir Roderick Murchison and myself have been directed³. Mr. Ransome prepared the south bay, which I will call No. I.; Mr. Szerelmey prepared the north bay, No. II.; both were cleared of the loose surface by hard brushing, &c., and then dealt with. The time which has elapsed is about six months; and if the processes are of any value, is hardly sufficient to show a difference due to weather.

Compared together, I. has more of the natural colour and appearance of the stone than II.; it is softer, wets immediately, and by degrees the water is absorbed. No. II. is darker, an effect due to the varnish-like character of the last coat applied; is harder, and does not absorb water; the fluid runs from

off it. The repellent coat appears to have much permanency, for alcohol and turpentine do not disturb it.

Though the time of trial is short, there is a place where, I think, evidence of difference appears. At a part of the wall having this form



rain droppings have fallen on the part below 5, whilst that above has escaped; consequently the stone 3 has suffered more than 4; and this is very evident in many of the untouched bays. In the prepared bays (where the loose surface had been removed before preparation) I looked for a six months difference at these parts. In the bay II. I could find no sensible difference between the parts 3 and 4, both were equally hard and good in character; in the bay I., 3 was softer than 4.

I do not find in other parts of the building, where Mr. Szerelmey's application is more than two years old, any signs of exfoliation, or separation of the prepared face of the stone; or any ground for fears in that respect. On the whole, therefore, and though the time of comparison is short, my belief is that his process will prove to be the best.

Though ever ready to aid the Government when it lies within my power, I am not professional. May I therefore be permitted to ask as a favour, that my opinion should not be communicated to either of the parties concerned to serve trade purposes.

I have, &c. | (signed) M. Faraday
The Right Hon. W. Cowper, M.P.

1. Henry Fitzroy.
2. Letter 3610. See also letters 3613 and 3614.
3. Letter 3737.

Letter 3742**Faraday report to Trinity House****12 March 1860****From *Parliamentary Papers*, 1862 (489), LIV, p. 10**

Royal Institution | 12 Mar 1860

I have been engaged in some observations upon the divergence of the electric beam, when emerging from a perfect lens, of the first order light apparatus, the electric spark being in the true focus. The lens was 39 inches high and 28 inches broad. I have no doubt the divergence in a vertical direction will be sufficient; indeed, it was proved to be so at the South Foreland. That in a horizontal direction will be less; both because the width of the lens is less than its height, and the width of the electric light is also smaller than its height, the two carbons used being one above and the other below. I fear, therefore, that the flash in the distance may, *with the present apparatus*, be of very short duration, though intense in power.

In a letter, dated 3d April 1857¹, is noticed the appearance, at Woolwich, of the Electric Light placed in the focus of the "Bishop's" apparatus at Blackwall; and there the beauty and briefness of the light is referred to. If the illumination seaward is only over a limited number of degrees, then the light on the land side may, by small reflecting surfaces, be thrown seaward, so as to prolong in some degree the duration of the flash².

(signed) M. Faraday

1. Letter 3265.

2. This letter was read to Trinity House By Board, 13 March 1860, GL MS 30010/42, pp. 559–60. It was agreed that Faraday's report did not support the continuation of testing the magneto-electric light in revolving lens, but it was agreed that the light should be tried at Dungeness.

Letter 3743**Faraday to an unidentified correspondent****12 March 1860****From the original in RI MS**

[Royal Institution embossed letterhead] | 12 Mar 1860

My dear friend,

Come & I will go[.]

Yours affectionately | M. Faraday

Letter 3744**Faraday to George Biddell Airy****12 March 1860****From the original in RGO6/678, f. 156–7**

[Royal Institution embossed letterhead] | 12 Mar 1860

My dear Sir,

Hard steel, which has been strongly magnetized at the first, in a certain direction, tends to keep up a condition in that direction when the bar is

demagnetized or reversed; and I think it very possible that the condition you refer to¹, may occur, & perhaps even not infrequently. There are circumstances under which a magnetic bar may be considered as having two sets of particles within it magnetized in opposite directions. I had some bars which were harder along one side than the other, as from



A to B. When these were very highly charged by the electro magnet, they soon fell somewhat in power, and I had reason to believe by the assumption on the softer side of a magnetism in the contrary direction to that in the hard part[.]

I believe this case could easily be made to occur. Let N.S. be a hard



magnet, well magnetised in the first instance in that direction;— and let N.S. be a like hard steel magnet, equally magnetised in the direction indicated;— & let these be put together as figured: the system will shew little or no power. But if the system be magnetized, with the intention of making the Sn' end north, the axis will be in the direction n's'; whereas, if it be changed and magnetised in the opposite direction, the axis will be in the direction N.S. It is not difficult to perceive how the parts of a magnet may have more or less of this relation and how its heterogeneity, either in composition, or hardness, or application of the dominant magnet at the first on one side, or along an oblique line, may produce a certain amount of effect such as you are investigating:

Ever My dear Sir | Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. In letter 3739.

Letter 3745

George Biddell Airy to Faraday

13 March 1860

From the original press copy in RGO6/678, f. 158

13 Mar 1860

My dear Sir,

I am much obliged by your answer to my query touching Magnetized Bars¹, which seems to bear in a very important degree on the possible explanation of the idiosyncrasies of dipping needles.

I had not the good fortune to hear you concerning lighthouses²; if other things had permitted, my tie to the Astronomical Society (where I had to make a statement about the Eclipse³) would have prevented it. But I want to hear you through the press, as Admiral Hamilton tells me that my assistance may be wanted by the Lighthouse Commission⁴. Will you let me beg an early copy of the printed Resumé.

I am, my dear Sir | Very truly yours | G.B. Airy
Professor Faraday

1. Letter 3744.

2. Faraday (1860a), Friday Evening Discourse of 9 March 1860.

3. See *Month. Not. Roy. Ast. Soc.*, 1860, 20: 181–9 for Airy's statement about the total eclipse of the sun on 18 July 1860 visible from north-east Spain.

4. See Airy, W. (1896), 240–1.

Letter 3746

Peter Henry Berthon to Faraday

14 March 1860

From the original in GL MS 30108/3/105

Trinity House | 14 Mar 1860

Dear Sir,

Referring to your Letter of the 7th Ult¹:— I am directed to acquaint you, that, in compliance with your Suggestion, it is proposed to cause the Ventilation Head, or Windguard, to be applied to the two New Lanterns intended to be erected at the Portland Light Houses².— I am also to forward, for your information, the enclosed Copy of a Report from the Agent for the Whitby Light Houses³; at which Station the Windguard was fitted in Novr. 1858.

I am | Dear Sir | Your's faithfully | P.H. Berthon
M. Faraday Esq. | &c &c &c

1. Letter 3724.

2. See note 3, letter 3724.

3. Gatenby to Berthon, 9 March 1860, GL MS 30108/3/105.

Letter 3747

Faraday to A. & C. Black¹

15 March 1860

From the original in WIHM MS FALF

[Royal Institution embossed letterhead] | 15 Mar 1860

Gentlemen,

I beg to thank you very sincerely for your kindness in sending me a copy of Owen's recent work on Paleontology²[.]

Ever Your faithful Servant | M. Faraday
Messrs. A & C Black | &c &c &c

1. A. & C. Black. Scottish publishing firm. See ODNB under Adam Black (1784–1874).
2. Owen (1860).

Letter 3748

Faraday report to Trinity House

21 March 1860

From *Parliamentary Papers*, 1862 (489), LIV, p. 10

Royal Institution | 21 Mar 1860

In reference to your question respecting the introduction of the Magneto-electric Light at Dungeness¹, I see no reason to expect that there would be greater difficulty respecting the illumination of the sea than at the South Foreland. The diverging beam would strike the sea at about 750 yards' distance from the lighthouse at high water. With respect to the general effect to be looked for, there is not so much back light to be reflected seaward at Dungeness as at the South Foreland, 280° requiring illumination at the former; and therefore the amount of light in any one direction must not be expected to equal that obtained where only 180° or 200° were illuminated².

(signed) M. Faraday

1. See Trinity House By Board, 13 March 1860, GL MS 30010/42, p. 560 for this request.
2. This letter was read to Trinity House By Board, 27 March 1860, GL MS 30010/42, pp. 575–6.

Letter 3749

Board of Trade to Faraday

23 March 1860

From the original press copy in TNA MT4/65, p. 1722

23 Mar 1860

acquaint you that H.M. Pay-Master General has been requested to pay to you the sum of one hundred pounds £100 less income tax, being payment for services in connection with Lighthouse abroad during the year ended 1st March 1860.

Enclosed is a form of receipt which upon being stamped and signed by you can be presented for payment through a Banker.

I am | Sir | Your obedient Servant | [not signed]
Professor Faraday F:R:S; | Royal Institution | 21 Albemarle Street

Letter 3750

Faraday to John Francis Campbell

26 March 1860

From the original copy in GL MS 30108/5, f. 8

To J.F. Campbell Esqr | &c &c &c | Secretary Royal Commission
*answers to second set of questions*¹

26 Mar 1860

I presume that in Gt Britain as abroad there is always a competent authority who considers & decides the circumstances of every particular lighthouse & gives instructions to the competing manufacturers[.] I think a manufacturer should not on his own judgment interfere with the judgment of such an authority

He may know the *height* of the lanthorn above the sea but ought not to vary his angles for any variation in that respect. The variation of the angles of the glass is rarely if ever the kind of change required. The authorities are conscious of the need of making the beam drop *if necessary* & always have the power of deciding it. In respect of the horizontal arc also the authorities always I believe give the number of degrees and the manufacturer should not use his judgment in the matter[.]

2. The manufacturer does not need this information to guide him in the grinding & fitting of his lenses prisms &c. He should receive a specification for an apparatus of well known construction to perform a certain well known optical duty and if when it is examined it does not perform that duty it is rejected.

3. There is not the slightest occasion for a table relating to the *horizontal arc*. The instruction is in any case for a certain number of degrees; with frame bars &c in certain plans and the work simply to be done. As to elevation the depression of the ray necessary on that account is in the power of the authorities being either ordered especially or governed by adjustment of the lamp and it should not be interfered with by the manufacturer[.]

4. I have no duty in this respect only that of examination nevertheless as far as I know by the results the specification & instruction given by the authorities are sufficient for the purpose. Perfect numerical exactness in the angles &c cannot properly be directed or required: for the maker has rightly a power of improvement over his glass & if his glass vary the angles must vary also. The specification should tell every thing that is missing for the full information of the makers. The makers have their work subjected to a very close optical scrutiny, and it is probable that any attempt to refine further in their department might lead to the removal of responsibility from the parties who ought to bear it.

26 Mar 1860 | M. Faraday

1. See note 1, letter 3732.

Letter 3751

Faraday to Christian Friedrich Schoenbein

27 March 1860

From the original in UB MS NS 452

Royal Institution | 27 Mar 1860

My dear Schoenbein,

It seems to me a long while since we have spoken together, & I know that the blame is mine, but I cannot help it, only regret it, though I can certainly try to bring the fault to an end. When I want to write to you it seems as if only nonsense would come to mind – & yet it is not nonsense to think of past friendship and dear communions. When I try to write of science it comes back to me in confusion. I do not remember the order of things – or even the facts themselves. I do not remember what you last told me¹ though I think I sent it to the Phil Mag & had it printed² – & if I try to remember up, it becomes too much the head gets giddy and the mental view only the more confused. I know you do not want me to labour in vain but I do not like to seem forgetful of what you tell me and the only relief I have at such times is to correct myself & believe that you will know the forgetfulness is *involuntary*. After all 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 and in thought shake hands tongues & hearts together.

We are all pretty well here. We get on well enough in a manner & are very happy – and I cannot wish you better things; though I have no intention when I say that to imagine you without your memory or your science:– long may you be privileged to use them for the good of human nature.

Our friend Miss Hornblower suffers very much from an affection of the knee of which I spoke before to you³. Lately she has seen Sir Benjamin Brodie who does not make himself responsible for advising an amputation,– he says it is a case so serious that the Profession ought not to be made responsible for the results of an operation. Whilst going there I have several times gone into a place of rest in that neighbourhood to look at a stone you know of & think of you all. Such places draw my thoughts much now & have for years had great interest for me. They are not to me mere places of the dead but full of the greatest hope that is set before man even in the very zenith of his physical power & mental force.

But perhaps I disturb you in calling your loss to mind – forgive me. Yet remember me very kindly to the mother & sisters.

Ever My dear Schoenbein | Yours Affectionately | M. Faraday

Address: Dr. Schoenbein | &c &c &c | University | Basle | Switzerland

1. Letter 3663.
2. Schoenbein (1859a).
3. See letter 3588.

Letter 3752

Faraday to Benjamin Silliman¹

27 March 1860

From the original in YUL Gen MS Misc 2599

Royal Institution | 27 Mar 1860

My dear Dr. Silliman,

I received your pleasant letter and immediately forwarded its companions. As to the one to our Secretary² I have not the least doubt that your wishes will be at once attended to. With respect to that for the Royal Society³ I will do all I can but I have no power or influence there. I know they have certain regulations but do not know what they are & I have not been on the Council for very many years. Work formerly made me decline & now a bad memory keeps me separate from every occupation I can avoid – but I will do all I can in the matter. You ought to have the Transactions_[.]

I happened to be in the North when that wonderful Aurora you speak of occurred⁴ & saw it there night after night. What a pity some mighty intellect could not see all that was happening electrically in both hemispheres & in all parts of the world at that time. I hope some powerful mind will at all events sum up the evidence & give us a combined result and such a view of the whole as the evidence will justify_[.] What a state the whole globe must have been in?

Give my kindest & sincerest respects to Your father⁵. It delights me to hear of him & especially to find that he is in the midst of pleasant thoughts & affections. I dare say he is not so old as I am though 10 or 11 years my Senior – but *I forget* data, faces, events, all things but I am very cheerful & very well. What does a man want more_[.]

By the bye I want something less than you give me_[.] I am not Sir Michael_[.] Many years ago I had to decide upon the unfitness of such a thing_[.]

Ever My dear Sir | Most truly Yours | M. Faraday

Dr. B. Silliman Junr | &c &c &c

1. Benjamin Silliman (1816–1885, DSB). Professor of Chemistry at Yale, 1854–1870 and an editor of *Am. J. Sci.*
2. John Barlow.
3. Silliman to Council of the Royal Society, 6 March 1860, RS MS MC6.61 which requested back volumes of the *Phil. Trans.* from 1850. The request was approved by the Council of the Royal Society. RS CM, 19 April 1860, 3: 57.
4. See letter 3686 and *Ann. Reg.*, 1859, **101**: 128–9 for this aurora which occurred on 28–29 August 1859.
5. Benjamin Silliman (1779–1864, DSB). Professor of Chemistry at Yale, 1813–1854. Founder and Editor of *Am. J. Sci.*

Letter 3753**Faraday to Julius Plücker****29 March 1860****From the original in NRCC ISTI**

Royal Institution | 29 Mar 1860

My dear Professor,

It is a long while since I have written to you¹ – too long but the fact is that the writing a letter or any action that calls upon memory is a burden to me – and not merely that but often makes my head ache and obliges me to cease – I am fit for nothing now but small gentle acts of thinking – I have just read over your last letter² and the description therein given of the light of the discharge in narrow tubes & the spectra which you obtain. They seem to me very wonderful but I cannot as I read retain the case of each vapour sufficiently to compare one with the other in my mind – or the phenomena altogether with – the luminous phenomena before known. In fact the luminous phenomena of the Electric discharge across very rare media are so numerous so varied *so indicative* and yet as it appears to me so little understood in respect of their *law* of fundamental principle that I *can not* retain them in my mind – for I have no memory & memory only can keep hold of them.

But though I cannot discuss these beautiful phenomena with you I can enjoy them & your success in the development of them & I doubt not that some day the whole beautiful encircling cloud of luminous results will open out into perfect order & intelligence & you will either produce that result or be a chief leader in obtaining it[.]

In the mean time I commend myself to you as an old worker in science one that loves to look on the present bands of worker[s] & as far as he can to keep up a relation with them if it be only be reminiscences & the memory of past times. And so it is that I write you now though I have no science to send you and am ashamed that I have not written before to thank you for yours & to say how heartily I am

My dear Professor | Your Obligated | M. Faraday
Professor Plucker | &c &c &c

1. See letter 3612 which refers to a letter Faraday wrote but which has not been found.

2. Letter 3612.

Letter 3754**Christian Friedrich Schoenbein to Faraday****29 March 1860****From the original in UB MS NS 453**

Bâle | 29 Mar 1860

My dear Faraday,

I have been expecting letters from you these many months, but up to this moment quite in vain. I trust, that nothing is wrong with you i.e. that not ill health or any other of the evils “which human flesh is heir to”¹ has been the

cause of your long silence. Now Professor Wiedemann and Dr. Hagenbach², intimate friends of mine going to London, I send you some lines through them, to give a sign, that, at least, your friend is still alive. Mr. Wiedemann will tell you, that I have strictly followed up your very wise advice³ and worked rather hard all the year round. The remedy has not been without its salutary effects, though I cannot say, that the deep wound inflicted upon us last year is healed. As to Mrs. Schoenbein, she certainly at intervals seems to be composed and resigned but the feelings of deep mourning and distress darken her mind again.

To give you a notion of my late scientific doings, I send you some papers on what I call "the chemical polarisation of the neutral oxygen"⁴, of which highly interesting subject Mr. Wiedemann, who has seen the experiments, will give all the details you may desire to know. I am inclined to believe, that it is now a matter of fact, that any oxidation, apparently effected by common or inactive oxygen, is always preceded by the polarisation of that element. I know now three or four dozens of cases of slow oxidation, in which that mysterious act takes place; for instance in that of Phosphorus, Zinc, Iron, Lead, Copper &c; Ether, and a number of other organic matters: pyrogalllic acid, desoxidized indigo &c.

In presence of such a numerous body of matter-of-fact evidence, I think, very little doubt can be entertained about the correctness of the new doctrine, that Oxygen is capable of being chemically polarized.

How is poor Miss Hornblower? Pray write in your next letter, how she is doing and remember me most friendly to your friend.

In entertaining the confident hope, that all is well with you and yours, I beg you to offer my kindest regards to Mrs. Faraday, your Niece and Brother's.

For ever | Your's | most faithfully | C.F. Schoenbein

Address: Doctor Michael Faraday | &c &c &c | Royal Institution | London

1. A slight misquotation of William Shakespeare, *Hamlet*, III, 1, 65.

2. Jacob Eduard Hagenbach (1833–1910, P3, 4, 5). Taught physics and chemistry at the Technical School in Basle, 1856–1862.

3. See letter 3572.

4. Schoenbein (1859b).

Letter 3755

Zerah Colburn¹ to Faraday

2 April 1860

From the original in RI MS Conybeare Album, f. 41

Office of "The Engineer." 163, Strand, London, W.C | 2 Apr 1860

Sir,

I took the liberty, a few days ago, to send you a pamphlet which I have issued on Boiler Explosions². The Explanation which I have advanced on

the 48th page of that pamphlet is, I believe, new in some respects³. Should you have had time to glance over it, I should feel myself much honoured in having your opinion, as to the soundness or otherwise of the views in question. Nothing but the great value which I should attach to your opinion could have induced me to obtrude this matter upon your attention, necessarily occupied as your time must be with other matters of greater importance.

I am Sir | Your Obedient Servant | Zerah Colburn
Professor Faraday

1. Zerah Colburn (1832–1870, B4). Engineer and journalist.

2. Colburn (1860).

3. *Ibid.*, 48 suggested that four distinct consecutive operations occurred in quick succession in the course of a boiler explosion.

Letter 3756

William Keddie¹ to Faraday

4 April 1860

From the printed original in RS MS 241, f. 157

Philosophical Society, Glasgow | 4 Apr 1860

Sir,

I have the honour to inform you that on the 4th January, the Philosophical Society of Glasgow enrolled you among the Honorary Members².

Herewith you will receive the Society's Diploma, inscribed with your name

I have the honour to be | Sir | Your most Obedient Servant |
W. Keddie. Secretary
To | Professor Faraday

1. William Keddie (1809–1877, B2). Naturalist and Secretary of the Philosophical Society of Glasgow.

2. This was proposed by William Thomson. See *Proc. Phil. Soc. Glasgow*, 1860, 4: 269.

Letter 3757

Faraday to Thomas John Fuller Deacon

6 April 1860

From the original in the possession of Elizabeth M. Milton

Hampton Court Green | 6 Apr 1860

My dear friend,

Whatever shape the reason may take that causes me to write to you, I am very glad of it;— for if there is much that we should say to each other by word of mouth, one may say a little of it by letter:— and though writing is but a poor way to think in, & therefore feeble for the expression of thought, we may supplement it a little in many points, by that which is expressed very

emphatically by the words, “went not mine heart with thee”¹ and how delightful it is to think that it is in happy matters that our hearts may go together;—and further that Mr. Leighton² comes back to us with happy accounts; as your letter to my dear wife says at least; for I have not seen him since his return, nor heard any thing of him except his arrival here. And whilst I am thinking loving thoughts to you, let me send *our* love to your dear wife & daughter. Jeannie would do so too were she here:— but this being Good Friday she & a party of nine or ten young ones have gone from here down the river in a boat, & after dining somewhere, intend to return here to tea. It is one of the annual excursions which the young ones have had for several years past; & a very fine day they have for it now[.]

As to my writing. My wife knows of no one who would suit the [word illegible] you offer for her consideration, & so wishes me to tell you that at once, that you may advertise — She however intends to send your letter to Miss Hornblower; but do not wait for that[.]

I am just on the point of asking you about Mr. Paradise Mr. Reid³ & also other persons & other things:— yet how vain & feeble that is when in a day I shall have Mr. Leighton to ask[.] No doubt he has told you all our news — Some to rejoice at, some to weep about;— but all when well considered quite fitted to cause much thankfulness. Like many of the dealing of God, much that has happened with us partakes of that beautiful character “though sorrow may endure for a night joy cometh in the morning”⁴. May it be so with all of us. May we know when it is that God dealeth with us as with children⁵. I used to think it an extreme expression “that thou mayest be ashamed & confounded & never open thy mouth any more when I am reconciled unto thee”⁶; but when we are sober it must be so: it is only when we are becoming vain & foolish that it can be otherwise[.]

Ever My dear Brother | Affectionately Yours | M. Faraday
Mr. T. Deacon

1. 2 Kings 5: 26.

2. Unidentified.

3. Unidentified.

4. Psalms 30: 5.

5. See Hebrews 12: 7.

6. See Ezekiel 16: 63.

Letter 3758

George Biddell Airy to Faraday

6 April 1860

From the original press copy in RGO6/326, f. 133–4

6 Apr 1860

My dear Sir,

I have to thank you for the copies of your lighthouse lecture¹. (There is a graphic account of your Lecture in Moigno’s *Cosmos*², I suppose from one

of the audience). I have little wherewith to make my return to you, and none to make an adequate one. But I send by Post a Notice about the approaching Total Eclipse which may interest you³. If you do not fear heat of climate, I would recommend you to go to see it.

This week, at the request of Admiral Hamilton, I have been to Birmingham, to look at the glass works in reference to the manufacture of lighthouse glass and to see one lighthouse which is now being packed up to go to Australia⁴. And I have been very much pleased with what I have seen, of the accuracy of the workmanship, which is quite sufficient for the use of a small light (as the galvanic spark) with the ordinary rules of direction of the beam of light, and to which they can give the modifications that may be required for any different sections of the beams of light.

But this struck me. We had the great lamp burning, not to its very full French height, but I believe as high as it is burnt in England. Upon trying the central (dioptric) division of the glasses by itself, and the upper and lower (catadioptric) divisions by themselves, the adjustment was evidently much in error. With a small galvanic spark, the error would have been much more conspicuous. This error was corrected by raising the lamp-stand $\frac{5}{16}$ of an inch.

Now if a galvanic spark is used, can you be certain of its plane to a very small quantity (as $\frac{1}{16}$ or $\frac{2}{16}$) under the following conditions? – The two charcoal points are burning away, is it certain that they burn equally? The self-acting adjustment brings them together, does it move the two points equally? If one is equal and the other unequal the vertical position of the spark will vary.

If the galvanic spark shall be introduced, for which I think you have shewn strong reason, it will be of great importance for economy of light to attend to the vertical of adjustments. The horizontal spread in the revolving lights will require special optical arrangements.

I am, my dear Sir, Yours very truly | G.B. Airy
Professor Faraday

1. Faraday (1860a), Friday Evening Discourse of 9 March 1860.

2. *Cosmos*, 1860, 16: 337–9.

3. A copy of Airy's statement about the total eclipse of the sun on 18 July 1860 visible from north-east Spain from *Month. Not. Roy. Ast. Soc.*, 1860, 20: 181–9.

4. See Airy, W. (1896), 240.

Letter 3759

Faraday to Robert Barclay¹

7 April 1860

From a typescript in RI MS

Hampton Court Green | 7 Apr 1860

Dear Sir,

I received your paper² & thank you for it. I had seen the communication to the Soc Arts of which I am a very old member. You are aware that I am

restrained from giving any opinion on commercial matters having sacrificed all my Professional prospects many years ago that I might be freed from such occupation_[.]

I am | Very Truly Yours | M. Faraday
R. Barclay Esq | &c &c &c

1. Robert Barclay (1833–1876, ODNB). Manufacturer of stationery in London.
2. Barclay (1860).

Letter 3760

Faraday to George Biddell Airy

9 April 1860

From the original in RGO6/326, f. 135–6

[Royal Institution embossed letterhead],
21 Albemarle St. W | 9 Apr 1860

My dear Sir,

Many thanks for your letter¹ & for the Eclipse account²_[.] I do not hope for the pleasure of seeing the eclipse but I hope you will be fortunate for you are taking great pains & care in the matter. I am glad you approve of Chances power of working but am somewhat surprized at the account you give me of an apparatus_[.] Did they give no account or reason for it, either of their own or of the parties who have ordered or examined it? There ought to be no room for a mistake

As to your enquiries about the Electric spark the lamp that Professor Holmes has had constructed, *practically* works well; for being adjusted, an edge or object near the light throws a shadow on a distant wall whose plane does *not vary*. The keeper watches this shadow from time to time (or such a shadow) and if there be a change, corrects for it:– but the compensation for change of plane in the carbons, either upper or lower has been very good.– I have no fear for the place of the Magneto electric spark provided we can secure the attention of the keepers – that will not be called for more than is required by *the written regulations now*, or than was given by the keeper at the South foreland;– but it must be a good deal more than what is absolutely *necessary for an oil lamp*_[.]

If the electric light should come into use it seems to me that there will be no difficulty in bringing both the vertical & the horizontal divergence easily under command.

Ever My dear Sir | Very Truly Yours | M. Faraday
G.B. Airy Esqr | &c &c &c

1. Letter 3758.
2. See note 3, letter 3758.

Letter 3761**Peter Henry Berthon to Faraday****17 April 1860****From the original in GL MS 30108/3/111.1**

Trinity House, London, E.C | 17 Apr 1860

Sir,

Referring to your Report of the 8th. February¹ on the proposition made by the Universal Lime Light Company, in respect of a Trial of that Light in one of this Corporation's Light Houses, – I am now directed to forward to you the accompanying Replies which the Company have given to the various enquiries suggested in your said Report², and I am to request that you will favor the Corporation with any observations which you may deem it necessary to offer thereon.–

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq | &c &c &c

1. Letter 3725.

2. A copy of this is in GL MS 30108/3/111.3.

Letter 3762**Faraday to Peter Henry Berthon****19 April 1860****From the original in RI MS F1 N/5/3**

Royal Institution | 19 Apr 1860

Sir,

Several of the answers from the Lime light company¹, are not direct to, or sufficient for, the questions; I will briefly point them out.

2.6. "The nature of the *gas* fuel?" The quantity required in 12 hours?" – The answer has no reference to it but is about *coke* and *oxygen*[.]

2.7. "How is the *gas* fuel to be obtained? – How is it to be stored?" – The answer has no reference to the questions.

2.8. "What is its (the gas fuel) current price for 12 hours including materials, wages, repairs, and other current expences?" The word *its* in my query is written *the* in the company's paper and may perhaps explain why the answer has no reference to the question.

2.12. "What is the vertical height and horizontal width of the most intense part of the luminous object? and what the height of the part which may be called generally intensely luminous?" By luminous object I mean the whole cylinder of lime at the ignited part:– and being three inches in diameter or about $9\frac{1}{2}$ inches in circumference I conclude, by the answer, that there are in that circumference, six spots, half an inch wide, of most intense ignition;– that these being surrounded by less ignited parts may be considered as extending to an inch in width;– and that there are six intervening dark portions, each between 0.6 & 0.7 of an inch wide.

2.16. Asked whether there are any exceptional cases such as the Needles Eddystone, Bishops &c &c These (by the answer) do not seem to be known. Reference is made to the convenience of storing, building, &c and the exceptions are said, if any, to be where these conveniences do not exist.

2.18. Relates to outfit. The answer seems to imply that the Fresnel apparatus would be dispensed with and gives no estimate on that point.

Supposing these answers had been to the purpose, or that direct answers may *now* be supplied, there comes the point, how far will they be justified and sustained in practice. At the close of the company's letter, it is not doubted, that, the directors will obtain permission to exhibit their light *in a lighthouse*, subject to the rules and regulations of the Trinity Corporation. My letter, containing the enquiries (8 Feb 1860²) did not go so far as that, as you will see at the end:— for it requires *full proof* of the fitness of any proposed arrangement, *before* considering its introduction into a lighthouse: and says that any failure in such preliminary proof, or any serious departure in the results from the answer given to the questions, will be considered as showing that the proposed application has not been sufficiently matured &c. I see no reason at present to alter that course as far as I am concerned in watching the matter³[.]

I have the honor to be | Sir | Your Very Obedient Humble Servant |
M. Faraday
P.H. Berthon Esqr | &c &c &c

Endorsement: No 1933 | Universal Lime Lt | Professor Faraday:— reporting upon answers to Questions submitted by him; returning them. (enclo:) | 19 Apr: 1860. | Ent in book on Scientific Reports (folio) 353 | Board 27 April 1860

1. In GL MS 30108/3/111.3; see letter 3761.

2. Letter 3725.

3. This letter was read to Trinity House By Board, 24 April 1860, GL MS 30010/43, p. 3. It was agreed to ask Faraday if he had any objection to this letter being copied to the Universal Lime Light Company.

Letter 3763

Alfred Austin to Faraday

19 April 1860

From *Parliamentary Papers*, 1860 (309), XL, p. 3–4

Office of Works, &c | 19 Apr 1860

Sir,

With reference to your letter of the 12th ultimo¹, reporting your opinion on the different processes of Mr. Szerelmey and Mr. Ransome, for preserving the outside stonework of the New Palace at Westminster, I am directed by the First Commissioner of Her Majesty's Works², &c., to send you herewith copy of a letter, dated the 2d instant, which he has received from Mr. Ransome on the subject³, and also copy of a letter, dated the 27th May 1859, from Mr. Warrington⁴ [sic] to Mr. Ransome⁵, containing the analysis referred to by the latter, and which, in a letter from him, dated the 28th May⁶, he states was

made upon a composition scraped by him from the surface of the buttress at the south-west end of the Terrace of the New Palace in the presence of the then First Commissioner'; and I am to request that you will have the goodness to inform the First Commissioner whether you are satisfied that Mr. Szerelmey's composition does not contain oleaginous or other organic matter, which by exposure to weather would cease to exercise its preservative power; and also, whether your attention, and that of Sir R. Murchison, was so exclusively directed to the latter applications of the two processes of Mr. Szerelmey and Mr. Ransome that you did not consider the results of the earlier applications of both those processes, and whether you are satisfied that Mr. Szerelmey's is the best process for the purpose, at present known.

I am, &c. | (signed) Alfred Austin | Secretary
Dr. Faraday

1. Letter 3741.
2. William Francis Cowper.
3. Ransome to Cowper, 2 April 1860, *Parliamentary Papers*, 1860 (309), XL, p. 3.
4. Robert Warington (1807–1867, ODNB). Chemist to the Society of Apothecaries, 1842–1867.
5. Warington to Ransome, 27 May 1859, *Parliamentary Papers*, 1860 (186), XL, p. 20.
6. Ransome to Manners, 28 May 1859, *ibid.*, p. 19.
7. Lord John James Robert Manners (1818–1906, ODNB). Conservative MP for North Leicestershire and First Commissioner of Works, 1858–1859.

Letter 3764

Faraday to William Francis Cowper

20 April 1860

From *Parliamentary Papers*, 1860 (309), XL, p. 4

Royal Institution | 20 Apr 1860

Sir,

I think it will be sufficient if I state that I was quite aware of the general matters contained in the copies of letters from Mr. Ransome, which have been sent to me¹. I knew that both Messrs. Ransome and Szerelmey used soluble silicates, and the latter in addition a final coat of bituminous or other such like matter. I have also not forgotten the Speaker's Court, and other parts of the Houses of Parliament. My former letters² have been very carefully expressed, and I think I have distinctly stated that I could not decide upon principle only, since either process, or both, might fail after the lapse of years through want of care or other circumstances, but being thrown upon the short and insufficient evidence now existing, and taking into consideration as far as I can the two processes, my belief is now, as it was when I last wrote², that Mr. Szerelmey's process will prove to be the best.

I have, &c. | (signed) M. Faraday
The Right. Hon. W. Cowper, M.P.

1. See letter 3763.
2. Letters 3610 and 3614.
3. Letter 3741.

Letter 3765**Faraday to Charles William Pasley****21 April 1860****From the original in BL add MS 41964, f. 305**

[Royal Institution embossed letterhead] | 21 Apr 1860

My dear Sir Charles,

Your invitation is very kind but unfortunately I cannot accept it. I thought you were aware that it is now very many years since I made the rule never to dine out_[.] At that time my pursuits forbade it & my health afterwards_[.] Now if I broke the rule I should give offence to a great many. So I hope you will excuse me_[.] Our kindest thoughts to Mrs. & Capt'n Tyler¹_[.]

Ever Truly Yours | M. Faraday

1. Margaret Tyler, née Pasley (c.1826–1912, *The Times*, 7 December 1912, p. 9, col. b). Daughter of C.W. Pasley who married, in 1852, Royal Engineer captain and Inspector of Railways Henry Whatley Tyler (1827–1908, *The Times*, 31 January 1908, p. 14, col. d).

Letter 3766**Faraday to James Timmins Chance****23 April 1860****From the original in RI MS F1 N/4/1**[Royal Institution embossed letterhead], London W. Albemarle St |
23 Apr 1860

My dear Sir,

Mr. Airy tells me he found one of your finished apparatus much out of adjustment and that he required to raise the lamp stand $\frac{5}{16}$ of an inch to correct it¹. I have perhaps no right to ask you if this is so – or is so for any reason. I have no doubt if it be so you have a reason for it – but think it may be some mistake of his_[.]

Ever Truly Yours | M. Faraday

James Chance Esq | &c &c &c

1. Letter 3758.

Letter 3767**James Timmins Chance to Faraday****24 April 1860****From the original in RI MS F1 N/4/2**

Hamstead, Birmingham | 24 Apr 1860

My dear Sir,

I am obliged by your letter of yesterday^{–1}. When Professor Airy first inspected the apparatus, the lamp was placed with the top of its burner 28 mm below the focal plane.

With the height of flame then attained, 20 mm was found to be about the best distance of the top of the burner from the focal plane.– This is the chief point to which I imagine Professor Airy to refer – viz. the best distance of the burner below the focus.–

I believe that 20 mm is not unusually adopted by the Trinity House – or about that distance.

At all events the position of the focal plane in the flame is still an undecided one – and a very important one.

On a subsequent examination of the apparatus which Professor Airy saw, I determined still to adhere to 28 mm:– but I made a slight alteration in the position of the lower prisms in their panels.

I have never seen a better light than the one alluded to:– I should like you greatly to have inspected it.

I am glad to hear of your success with the electric light – for fixed lights².

In a sixth order Light (150 mm say) a height of flame equal to 1 mm (say) = $\frac{1}{25}$ th of an inch, ought (even supposing parallel emergence) to spread over 20 miles from the horizon inwards (say) at an elevation of 400 ft:–

The remaining distance to the shore can be provided for by the bottom prisms.

There will be an end I suppose of large apparatus.

Yours truly | J.T. Chance

1. Letter 3766.

2. See letter 3742.

Letter 3768

Henry Enfield Roscoe to Faraday

26 April 1860

From the original in IET MS SC 2

Owens College, Manchester | 26 Apr 1860

My dear Mr. Faraday,

You will I know be interested to hear that Bunsen has discovered a new alkaline metal¹ – discovered it by a method which, it seems to me, is for our Science of Chemistry what Adams's² & Leverrier's³ discovery of the planet Neptune is in Astronomy⁴.

I believe I mentioned to you, when I saw you last, that Bunsen and Kirchhoff are engaged at present upon what they term "Spectral Analysis" i.e., the identification of the constituents of a body by means of the various colours which these constituents impart to the flame.

You will probably have read a short notice given by Stokes in the Phil. Mag: a month or two ago of Kirchhoff's⁵ most interesting and important discovery of the cause of the Fraunhofers⁶ lines in the Solar Spectrum⁷.

Making use of this important discovery, Bunsen finds that every (or almost every) elementary body or its compounds imparts to a colourless flame light of a definite degree of refrangibility – soda for example giving a spectrum consisting of two narrow bright bands corresponding exactly in refrangibility to the dark line *D* in the Sun's spectrum. In this way, if a mixture of the Salts of Ba. Sr. Ca. Mg. Li. K. Na be made – & if $\frac{1}{10}$ of a milligramme in weight of such a mixture be placed in a colorless flame (of Hydrogen – or Coal gas & air) and if the resulting coloured rays be allowed to pass through a prism, the spectrum thus formed will show bands of light in different positions, *each one* of which corresponds to, & is produced by *one* of the constituents of the mixture. Thus at one glance the presence of each of these substances – in a mere trace of such a mixture – may in one moment with certainty be detected.

Bunsen has found Lithium in all the potashes which he has examined, also in 20 grammes of Sea-water!

Now examining in this way the alkalies he has found a substance which in its spectral relations is different from any of the 3 known fixed alkalies (K.Na.Li) – but up to the present time he has not found it in quantities sufficient to enable him to isolate it or to obtain the Chemical analogies of its salts.–

I need not insist on the immense importance of these investigations & the new & vast areas they open out – I intend going to Heidelberg in the summer, & on my return I should be glad, if you think it adviseable, to give an account of these experiments on one of your Friday Evenings in the spring⁸.

Ever my dear Mr. Faraday truly yours | Henry E. Roscoe
Professor Faraday

1. This was caesium. On Bunsen's discovery see James (1983a), 43.
2. John Couch Adams (1819–1892, ODNB). Astronomer and Fellow of Pembroke College, Cambridge, 1853 to 1892.
3. Urbain Jean Joseph Leverrier (1811–1877, DSB). Director of the Paris Observatory, 1854–1870.
4. Neptune was discovered in 1846. On this see Grosser (1962).
5. Gustav Robert Kirchhoff (1824–1887, DSB). Professor of Physics at Heidelberg, 1854–1875.
6. Joseph Fraunhofer (1787–1826, DSB). Bavarian glass maker and discoverer of the eponymous lines in the solar spectrum.
7. Stokes (1860).
8. Roscoe (1861), Friday Evening Discourse of 1 March 1861.

Letter 3769

P.M. Henschikoff¹ to Faraday

1 May 1860

From the original in IET MS SC 2

To Professor Faraday,

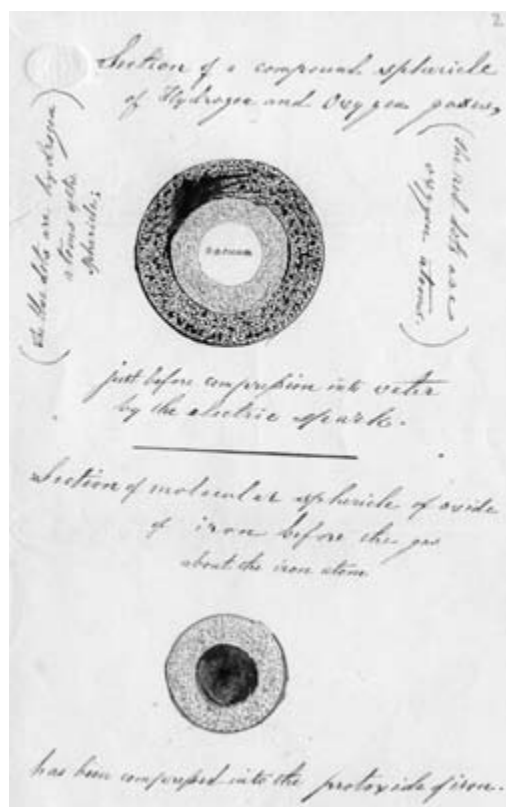
1 May 1860

Sir,

The equivalents that are gases are not atoms: all the equivalents which are gases are hollow sphericles; these sphericles are molecules consisting of

a number of very minute ultimate atoms. *vide* diagrams. Where an atom of iron is enclosed in the hollow of an oxygen sphericle, the oxygen becomes compressed into solidity about the isolated ultimate iron atom, which is thus oxidized. Heat by promoting the isolation favours the oxidation. When water freezes it expands, because when heat is abstracted the mass becomes rigid from the drawing closer of the molecular compound sphericles of hydrogen enclosing oxygen *vide* diagrams. But thus the pressure on the sphericles is diminished and consequently they expand, their liquid state, as molecular water sphericles, being elastic. By a diminution of this mysteriously subtle but infinite pressure, two volumes of ammonia expand into four volumes of gas, when the nitrogen sphericle is ejected from the hydrogen encasing spherical. And so are all similar chemical phenomena to be explained by a theory of galvanism which is most humbly submitted to Professor Faraday by his obedient servant,

P.M. Henschikoff



Address: Professor Faraday | Royal Institution | Albemarle Street | London
 Postmark: Limerick

1. Unidentified.

Letter 3770

Faraday to James Timmins Chance

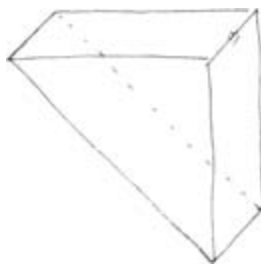
4 May 1860

From the original in RI MS F1 N/4/3

[Royal Institution embossed letterhead] | 4 May 1860

My dear Sir,

Can you & will you help me. I want a short right angled prism of glass of about this size



the thickness a being from $\frac{3}{4}$ to 1 inch_[.] It is for total reflexion so that the three right angled faces would require polishing but the two triangular faces not. The point however is that it should be moderately good glass and if I set our workers here about it I shall be delayed by the plea that perfect glass is not to be had. Does it so happen that you occasionally may have a spare piece not perfect but moderate in quality which you could let me have out of which I could get a prism cut approaching the size I have drawn. If I am imprudent in my enquiry tell me so at once.

Many thanks for your reply¹ to my Airy note².

With kindest remembrances to Mrs. Chance believe me Ever Truly Yours

| M. Faraday

Jas Chance Esq | &c &c &c

1. Letter 3767.

2. Letter 3766.

Letter 3771**Alfred Austin to Faraday****4 May 1860****From *Parliamentary Papers*, 1860 (309), XL, p. 4**

Office of Works, &c | 4 May 1860

Sir,

I am directed by the First Commissioner of Her Majesty's Works¹, &c., to acknowledge and thank you for your further communication of the 20th ultimo², respecting the stone-preserving processes of Mr. Szerelmey and Mr. Ransome, as applied to the stonework of the New Palace at Westminster.

I am, &c. | (signed) Alfred Austin | Secretary
Dr. Faraday

1. William Francis Cowper.

2. Letter 3764.

Letter 3772**Daniel Maclise¹ to Faraday²****8 May 1860****From the original in RI MS Conybeare Album, f. 28**

8 May 1860

Dear Sir,

How weak is my poor ordinary vision when compared to that inner and more intense sense which enabled to you to detect the adulteration – one has heard and seen Eau d'Or but a Gelatine of gold is a novelty.

Very faithfully your's | Danl Maclise

1. Daniel Maclise (1806–1870, ODNB). Painter.

2. Recipient identified on the basis of provenance.

Letter 3773**Faraday to Cecilia Anne Barlow****9 May 1860****From the original in RI MS F1 E21**

[Royal Institution embossed letterhead] | 9 May 1860

My dear Mrs. Barlow,

We dare not give ourselves the pleasure of being with you on Thursday¹ evening: the few times I have been out lately is producing a pressure that I must resist. I am sorry for my wife & Janes sake but believe it will be best[.]

Very faithfully & gratefully Yours | M. Faraday

Address: Mrs. Barlow

1. That is 10 May 1860.

Letter 3774**Alfred Austin to Faraday****9 May 1860****From *Parliamentary Papers*, 1860 (309), XL, p. 4**

Office of Works, &c | 9 May 1860

Sir,

I am directed by the First Commissioner of Her Majesty's Works¹, &c., to inform you that your attention having been only directed to the process which has been applied to the stonework of the New Palace at Westminster by Mr. Ransome since 1856, and by Mr. Szerelmey since 1858², the First Commissioner requests that you will also examine and report upon the stones which have been covered by Mr. Daine's process since 1855³, and Mr. Fincham⁴, the clerk of the works, has received directions to be in readiness at his office in the New Palace on Friday and Saturday next⁵, before two o'clock, in order to point out where the applications of Mr. Daine's process have been made.

I am, &c. | (signed) Alfred Austin | Secretary
Dr. Faraday

1. William Francis Cowper.

2. See letters 3610, 3613 and 3614 and notes.

3. Daines's method (see his Patent 1854-1785) had been used since mid-February 1854. See the report of on the decay of stone, *Parliamentary Papers*, 1861 (504), XXXV, question 702.

4. James Fincham (d.1874, age 59, GRO). Clerk of the works at Parliament *Royal Kalendar*, 1860, p. 166.

5. That is 11 and 12 May 1860.

Letter 3775**Peter Henry Berthon to Faraday****9 May 1860****From the original in GL MS 30108/3/111.8**

Trinity House, London, E.C | 9 May 1860

Sir,

Referring to your Report of the 19th Ultimo¹ on the subject of the Proposition made by the Universal Lime Light Company in respect of a Trial of that Light in one of this Corporation's Light Houses, – I am now directed to forward to you the accompanying Replies² which the Company have given to the further enquiries suggested in your said Report, and I am to request that you will favor the Corporation with any observations which you may deem it necessary to offer thereon[.]

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq – F.R.S., | &c &c &c

1. Letter 3762.

2. A copy of this is in GL MS 30108/3/111.12.

Letter 3776

Faraday to Peter Henry Berthon

11 May 1860

From the original in RI MS F1 N/5/4

Royal Institution | 11 May 1860

Sir,

The additional answers obtained from the Universal lime light company are to the point¹, except, that to question 18. The question is "What will be the probable *outfit* of the apparatus, with the buildings necessary for it and for the accom[m]odation of the extra staff required?" You will see that the answer makes no reference to the buildings, gazometers, retorts, and extra staff required.

The answers generally are such, as, in my opinion, to justify the Trinity House in permitting a trial on a full scale, of the applicability of the lime light to lighthouse purposes: provided it can be done so as to give full practical information on all matters, and without injury or interruption to the service. Such a trial should be sustained night after night for a given time, that the following, amongst other, points may be tested, namely;– the character and power of the light (that being at the time under general observation) – its constancy – its liability to accidents – its capability of replacement by the ordinary central lamp – its call on the keepers attention – the requisite number of persons for the service – the manufacture and storing of the gases – the quantity of gases consumed – the cost in full of the gases – the cost of wages – the cost of outfit and whole expence.

It is not for me to judge where such a trial could be best made: but wherever made, I think it should depend upon its own appliances as much as a lighthouse removed from the neighbourhood of a town and placed on a distant coast. I think too, it should be subject to the continual observation of external night watchers; and if they be mariners, or such as are concerned in lighthouse action, so much the better. The South Foreland upper light is an admirable station for observation; but perhaps cannot be permitted to be used on this occasion. The Purfleet lighthouse might serve, but would not be under good observation, or observation in all directions. I think a *fixed light* is the proper condition in which the light should be observed. If Purfleet, however, or any such situation, were chosen, it would not be necessary to have six jets on the lime, nor could they all be watched, by persons external to the lighthouse: perhaps three would do; only, this would not make the full call on the nightly supply of gas, and so far would leave the trial imperfect.]

Will you allow me to say that not having the answers to my questions, nor copies of them, I find myself frequently at a loss, when I desire to refer to them²[.]

I have the honor to be | Sir | Your Very humble Servant | M. Faraday
P.H. Berthon | &c &c &c

Endorsement: No. 2309 | Universal Lime Lt | Professor Faraday reporting upon additional answers. | 11 May 1860. Entd in book of Scientific Reports

(Folio 355) | Board 15 May 1860 | Wardens & Lt Comm to arrange for Trial at So. Foreland High Light | PHB

1. In GL MS 30108/3/111.12; see letter 3775.

2. This letter was read to Trinity House By Board, 15 May 1860, GL MS 30010/43, pp. 21–3. It was agreed to proceed with the trial.

Letter 3777

Faraday to Richard Griffin¹

11 May 1860

From the original in BL add MS 28510, f. 41

[Royal Institution embossed letterhead] | 11 May 1860

Dear Sir,

I am much obliged to you for the copies of the Christmas lectures². I am obliged to you also for the favourable opinion of them which is expressed in the Preface³ – and above all for the clear & plain manner in which it is shown that the act of publishing is not mine and so my relation to the volume made manifest to all into whose hands it may come⁴.

Ever my dear Sir | Very Truly Yours | M. Faraday
R. Griffin Esq

1. Scottish publishers founded by Richard Griffin (d.1832, age 43, B1 under Charles Griffin).

2. Faraday (1860b) which he delivered during the 1859–1860 season and was published by Richard Griffin and Company.

3. The preface (pp. iii–vi) was written by the Glasgow-based industrial chemist Charles Hanson Greville Williams (1829–1910, ODNB). See Crookes to Williams, 1 February 1861, in James (1981), letter 1.

4. Faraday (1860b), v–vi.

Letter 3778

Faraday to Alfred Austin

12 May 1860

From *Parliamentary Papers*, 1860 (309), XL, p. 5

Royal Institution | 12 May 1860

Sir,

In obedience to the wishes of the First Commissioner of Her Majesty's Works¹, &c., I yesterday examined the places at the Houses of Parliament where Mr. Daine's process (as I was informed by Mr. Fincham²) had been applied³; these consisted of three small surfaces on the terrace, done in 1854; the upper part of the walls in the Commons' inner court, done in 1854; the wall in the Peers' inner court, done in 1856; two turrets on the river front, done in 1856; and a window near the Victoria Tower, done in 1856.

I am told that Mr. Daine's application consists of oil and sulphur: as regards the oil, this agrees with what can now be observed; of the sulphur I

could find no trace. The composition can still be found in places, such as the walls of the Commons' inner court and the turrets. It occurs, occasionally, in patches, which have run down the walls, and also in tears resulting from such flow. These tears, though black on the outside, are soft, and when squeezed, the yellow thickened but unsolidified oil flows out. I find no sulphur in this oil. I send some specimens with this letter.

This composition is evidently still running down the wall, though slowly, in many places. Being yet soft it cannot be fixed to the surface of the stone, as a body becoming solid in a short time may be expected to be; and it is very probable that a large portion of that which was applied to the surface has sunk inwards (for, under the summer's sun its flow and absorption may be expected to be much quickened) into the mass of the stone. I removed some of the surface of the stone at the top or parapet part of these walls, but upon chemical examination could find no remains of the preparation in it. The stone seemed restored to the first or unprepared state; and at these places, and almost everywhere where the oil was not visible in runs upon the surface, water put upon it was absorbed as fast as upon the unprepared stone surfaces.

There is nothing in the appearance of the prepared stone which leads me to suppose that its surface has undergone any permanent protecting change; there is nothing by which I can perceive that any protecting or extraneous substance has been permanently fixed in the surface of the stone; nor is there anything in the description of the substance, or in the nature of the portions still found here and there, where, by reason of its fluidity it has accumulated, that leads me to expect the process can confer permanent protection upon the walls.

I have, &c. | (signed) M. Faraday

1. William Francis Cowper.

2. James Fincham (d.1874, age 59, GRO). Clerk of the works at Parliament *Royal Kalendar*, 1860, p. 166.

3. See note 3, letter 3774.

Letter 3779

Faraday to George Richmond

12 May 1860

From the original in SI D MS 554 A

[Royal Institution embossed letterhead] | 12 May 1860

My dear Richmond,

M de la Rive a very eminent natural philosopher of Geneva is in town on a diplomatic mission from the Government of Switzerland to our Government¹. He will have to stay here some weeks. I promised Madame de la Rive to ascertain from you whether you were so far at liberty as to draw his portrait, after the manner of Lyell², and if so what your terms were^[.] I believe his best hour is $\frac{1}{2}$ p 10 o clk – Are you inclined to do it & if so could

you send me such an answer as I could send to Madame or would you like to write to them direct³[.] They are living at 21 Dover Street Piccadilly – Ever My dear Richmond | Yours Most Truly | M. Faraday

1. This refers to the question to the annexation of Savoy by France which so threatened the position of Switzerland that they appointed De La Rive to be special ambassador in London. On this and De La Rive's role see Imlah (1966), 165–78.

2. Charles Lyell (1797–1875, ODNB). Geologist.

3. De La Rive is not included in the list of portraits by Richmond in Lister (1981), 150–75.

Letter 3780

William Thomson to Faraday

14 May 1860¹

From the original in IET MS SC 2

Laboratory R.I | Monday 4 pm

My dear Faraday,

Many thanks for your kind note². I hope by Wednesday³ to be ready with my instruments, and when you come I shall have to ask your advice on many points connected with the arrangements for my lecture⁴.

With Anderson's assistance I have been at work all day putting up one of my electrometers, and I shall probably have nearly as much work tomorrow with another. I find a great deal of trouble in making the glass fibre suspension having very little skill of hand so that what would be easy & short to others costs me a great deal of time & trouble.

Believe me | Yours very truly | William Thomson

Endorsed by Faraday: 6 Arlington Street | Piccadilly

1. Dated on the basis of the reference to Thomson's Friday Evening Discourse.

2. Not found.

3. That is 16 May 1860.

4. Thomson (1860b), Friday Evening Discourse of 18 May 1860.

Letter 3781

Faraday to John Tyndall

16 May 1860

From the typescript in RI MS JT TS Volume 12, p. 4151

Royal Institution | 16 May 1860

My dear Tyndall,

Just a line to say I am very sorry you grumble at your head. I was in hopes the weather and country had refreshed you as it has done me. Thomson is here at work for Friday¹ and inquiring earnestly after you. De la Rive is busy diplomatically². Barry is gone from us³; it strikes me the more because I have had occasion to be at the houses of parliament lately⁴ and it was only on

Saturday week⁵ we had a long conversation at the Royal Academy Dinner. Health and long life to you is the sincere wish of your friend⁶_[.]

1. Thomson (1860b), Friday Evening Discourse of 18 May 1860.
2. See note 1, letter 3779.
3. Barry died on 12 May 1860.
4. See letter 3778.
5. That is 5 May 1860. Faraday's presence at the dinner is noted in *The Times*, 7 May 1860, p. 5, col. c.
6. There is a note here saying 'M. Faraday' cut off'.

Letter 3782

William Francis Cowper to Faraday¹

19 May 1860

From the original copy in US MS 62 Broadlands Papers WFC/HH/1

Office of Works | 19 May 60

Dear Sir,

I am about to order the application of Zerelmy's [sic] process to the Palace of Westm[inster] but as you have taken so much trouble about the matter I am encouraged to ask whether you feel confident that no mischief will arise from the damp in the interior of the stone being prevented from exuding from the surface while some moisture is beneath the surface, may not ultimately lead to decay and will you also have the goodness to tell me whether you consider the effects of Szerelmey's process decidedly superior to those of paint – for after all might it not be as well to stick to paint².

Be | Your | signed WC

1. Recipient identified on the basis that letter 3785 is the reply and that both copies are on the same piece of paper.
2. See letters 3610, 3613, 3614 and notes.

Letter 3783

George Richmond to Faraday

20 May 1860

From the original in IET MS SC 2

10 York St, Port[man] Sq | 20 May 1860

My dear Mr. Faraday,

I am very sorry you have had the trouble of writing a second note¹ about the proposed sittings for M. de la Rive. I can quite understand how hard it must be for him to find time for such an occupation as sitting for a portrait and if he were not so distinguished a person, and a friend of yours, I should be only too pleased to have that time to give to others, which I should have felt it an honour to devote to him_[.]

Ever faithfully yours | Geo Richmond

1. See letter 3779.

Letter 3784**Faraday to Margaret Brodie Herschel¹****21 May 1860****From the original in WIHM MS FALF**

21 May 1860

Admit Lady Herschel and friends to the remaining 3 o'clk lectures of this season² | M. Faraday

1. Margaret Brodie Herschel, née Stewart (1810–1884, ODNB under J.F.W. Herschel). Married John Herschel on 3 March 1829, see his ODNB entry.

2. These would have been lectures by the Professor of Geology at King's College, David Thomas Ansted (1814–1880, ODNB), on geography, by the lecturer on botany at St Mary's Hospital, Thomas Spencer Cobbold (1828–1886, ODNB), on mammals and by the Professor of Chemistry at Royal Military Academy, Frederick Augustus Abel (1827–1902, ODNB), on explosives. Respectively RI MS Le4/233, 232, 234.

Letter 3785**Faraday to William Francis Cowper****21 May 1860****From the original copy in US MS 62 Broadlands Papers WFC/HH/1**

Royal Institution | 21 May 1860

Dear Sir,

Your letter of the 19th¹ contained two questions on which you do me the honor to ask my opinion viz whether the damp that may be included in the stone and covered by Mr. Szerelmy's process being prevented from escaping by the surface may not ultimately lead to decay? and whether his process is in effect superior to the application of a coat of paint?² Being told that Mr. Szerelmey's process included first the application of a silicaferous operation & then the application of a bituminous coat, I enquired and was informed that the latter was to be applied when the stone was perfectly dry. If their intention be properly carried out I do not think that any bad result dependent upon moisture left in the stone will occur.

Whether Mr. Szerelmey's process is at all superior to the careful application of a coat of paint is a question I must remit to those who have had experience of the value of the latter protection. I have not had that experience & as I have said before³ the experience we have had of Mr. Szerelmey's process or Mr. Ransome's process is by no means sufficient to establish with certainty the protecting power for many years of either one or the other or shews the superiority of one over the other. Nevertheless as you did me the honor to ask for the best opinion I could form under the cir[cumstan]ces I felt bound to respond to your wish informing you at the same time of the degree of reservation I was obliged to make & as betw[ee]n the 2 or 3 parties concerned, judging with the utmost degree of impartiality my mind was capable of. I only regret that I cannot make the evidence more forcible or my opinion stronger.

I ha | signed M. Faraday

1. Letter 3782.

2. See letters 3610, 3613, 3614 and notes.
3. Letter 3741.

Letter 3786**Faraday to Henry Stevens¹****29 May 1860****From the original in Huntington Library MS RH 633**

29 May 1860

Sir,

I have corrected the enclosed at page 26 in the manner desired^[.]

I have the honor to be | Sir | Your Very Obedient Servant | M. Faraday
 Henry Stevens Esqr | &c &c &c

1. Henry Stevens (1819–1886, B3, DAB). American-born bookdealer in London from 1845.

Letter 3787**Peter Henry Berthon to Faraday****2 June 1860****From the original in GL MS 30108/4/112**

Trinity House, London, E.C | 2 Jun 1860

Sir,

The Dioptric Apparatus for Foul Point Light House and also that for Elephant Harbour will be ready for inspection at this House on the 9th Instant, and the Elder Brethren will be glad if you will subject them to the customary Tests on their behalf, and I will arrange that everything shall be ready for you if you will kindly inform me of the day when it will be convenient to you to inspect them.

I enclose with reference to your Letter of 11th Ult¹. a Copy of the Questions sent to the Lime Light Company and also of their replies thereto; and am,

Sir | Your most humble Servant | P.H. Berthon
 M. Faraday Esq. – F.R.S. | &c &c &c

1. Letter 3776.

Letter 3788**George Gabriel Stokes to Faraday****8 June 1860****From the original in IET MS SC 2**

14 Bellvue Terrace, Southsea, Portsmouth | 8 Jun 1860

My dear Faraday,

I found your paper at the R.S. and took it here to read¹.

I am nearly sure you asked me to read it and give you my opinion about it. I will answer on that supposition.

I own my own opinion is against sending it in for the *Transactions*. It might have done as coming in incidentally in the body of a paper containing positive results but it seems to me it would scarcely do for an independent communication to the *Transactions*, a communication I mean made at one time though forming part of a train of experimental enquiry. If such negative results had the effect of correcting a commonly entertained expectation, or if the author's previous labours had led those who had followed them to regard a positive result as probable, or even not unlikely, the case might be different. But to my mind the antecedent probability of a positive result was too slender to justify the publication, in such a solemn manner as in the *Transactions*, of a negative result.

I should not myself expect a change in the temperature or electric state of a body even if one could transfer it to a place where gravity was only half what it is at the surface of the Earth; but even if a change were to be effected under these circumstances one could hardly expect to render it sensible in merely passing from the bottom to the top of a tower. To my mind the antecedent probability of a positive result is the product of two (to my mind small) fractions expressing the separate probabilities.

I write on the supposition that the change to be expected was one due to a change in the gravitating relations of the experimental mass – to a change for example from a place of strong to a place of weaker gravity – and not merely to a motion with or in opposition to the force of gravity: Such I take to be your view.

A sentence at the top of p. 2 will require modification. "The so called variation of gravitating force by change of distance, can only be taken into account in either astronomical or cosmical phenomena: neither of which can be made the subject of experiment." This statement is too absolute because the change is taken into account in Cavendish's² experiment³.

I don't think there would be any objection to the paper's appearing in the *Proceedings*. I should be glad if you would take the opinion of some one else.

I remain here till Tuesday⁴ when I go to Town.

Yours very truly | G.G. Stokes

or rather that you would think such a change, if it could be effected, more likely to yield a positive result.

1. This was Faraday's paper 'Note on the possible relation of Gravity with Electricity or Heat' dated 16 April 1860. There is a manuscript of this in RI MS F2, J286–90 where Faraday continued the paragraph numbering of the 'Experimental Researches in Electricity' series from 3300 to 3312. The opening part was published in Bence Jones (1870a), 2: 417–18. The paper was not formally noted as being received by the Royal Society in RS MS CMB 90d.

2. Henry Cavendish (1731–1810, ODNB). English natural philosopher.

3. To determine the density of the earth. See Cavendish (1798).

4. That is 12 June 1860.

Letter 3789**Faraday to Charles Babbage¹****9 June 1860****From the original in BL add MS 37198, f. 73**

[Royal Institution embossed letterhead] | 9 Jun 1860

My dear Babbage,

I send back the *Jacquarde* safely & am deeply obliged to you for the loan of it & grateful to you for thinking of us and doing what I never should have ventured to ask²[-]

Ever Truly Yours | M. Faraday

1. Charles Babbage (1791–1871, ODNB). Mainly worked on inventing mechanical calculating machines.
2. He discussed this in Faraday (1860c), Friday Evening Discourse of 8 June 1860.

Letter 3790**Faraday to George Gabriel Stokes****11 June 1860****From the original in ULC Add MS 7656, F25**

[Royal Institution embossed letterhead] | 11 Jun 1860

My dear Stokes,

I am very grateful for your kindness¹ though I had not ventured to presume on troubling you except by the general question whether the account was worth appearance in the proceedings or any where else at the R.S. – I quite go with you in all you say and think that the paper had better be withdrawn altogether if it can be. I want no other opinion than yours & my own.

I hope my acknowledgment will catch you at Southsea or at least follow you to London safely.

Ever Your Very Obligated | M. Faraday

G.G. Stokes Esqr | &c &c &c

1. Letter 3788.

Letter 3791**William Thomson to Faraday****12 June 1860****From the original in IET MS SC 2**

Thornliebank Glasgow | 12 Jun 1860

My dear Faraday,

The insurance trial regarding the Atlantic Telegraph, for which I expected to be summoned to London before this time, has been put off until the end of the month, and I expect to be required to attend between the 25th and the 1st of July¹. I shall call to see you if you are in London, and to ask if

you are disposed to come out to Kew along with me, where I shall have to go to look after the recording atmospheric electrometer².

I have made several attempts to discover, if possible, indications of electric force in the air over the surface of two liquids, such as sulphate of zinc and sulphate of copper, separated by a porous partition, but as yet with no result. I think there must be something to be found; and probably strong in such a case as caustic potash and nitric acid, since these two liquids when substituted for acidulated water next the zinc and platinum of a galvanic element increase its electro-motive force very largely.

I yesterday had an opportunity of observing something with my portable electrometer during thunder. No lightning was visible, but I could perceive the instants of the discharges that gave rise to audible sound by sudden motions of the needle. The thunder came about 20 seconds later than an impulse of this kind, several times, from which I judged that it was about 5 miles distant. The motion of the needle was more sudden than that which takes place when the conductor with the match burning is suddenly insulated. When this is done the needle gradually deflects without vibration, and shows nearly the full effect in 5 or 6 seconds. The changes yesterday were so sudden as to leave the needle vibrating, and were therefore *inductive* beginnings of the electric change in the conductor which the burning match completes. Besides the larger impulses which I was able to connect with the thunder, there was a constant flickering of the needle, which seemed to show that between flash and flash sufficient to make audible thunder, there were countless smaller discharges. On a small scale the same thing is produced, & is indicated by the needle in the same way, when shreds or fibre assist disruption of the air in any "field of electric force" in connection with experimental apparatus.

The ordinary atmospheric changes, although sometimes very rapid, for instance doubling the force in a minute or less, are not instantaneous, and show their effect by a gradual motion of the needle without vibrations. It seems certain that such changes are produced by motions of electrified air, while those I observed yesterday must have been due to discharges.

Believe me | My dear Faraday | Ever truly yours | William Thomson

1. For this and Thomson's evidence see Patterson v. Harris, *The Times*, 26 June 1860, p. 11, col. d and 28 June 1860, p. 10, col. d–e.

2. See Thomson (1859, 1860a) and letters 3664, 3665, 3667 and 3740.

Letter 3792

Faraday to Peter Henry Berthon

13 June 1860

From the original copy in GL MS 30108/4/112

Royal Institution | 13 Jun 1860

Sir,

On the 11th of June I examined the light apparatus to which your letter of the 2nd refers¹. On all points – as regards Colour of the glass – Striae –

bubbles – Workmanship of the pieces and association of the parts into two great wholes they were with one small exception equal and in respect of adjustment superior to former apparatus, so as to obtain my entire approval. The reserved point was this, a part of the glass at *one* of the centres of the eight holophotal lenses of the larger apparatus was a little deformed so that the focus of that part was not quite the same as that of all the rest of the glass. The space affected was not larger than a sixpence & the only result was to disturb a little the direction of the ray at the central part of the beam not however throwing it out of effect but simply diffusing it. The effect is so little (being not really injurious) that I did not think I ought to object to it, though it is not to be repeated_[.]

The lamp in the fourth order apparatus had one larger wick & a central deflector but a central wick in place of the deflector would have been a great advantage as regards those rays which *descend* from the flame to the lower part of the apparatus the deflector offers much obstruction for none of them can pass from the further side through it across the flame_[.] In my opinion the suppression of the central flame & the substitution of an opaque mass of metal in the shape of a deflector can never be an advantage².

I have the honor to be | Sir | Your faithful humble Servant |
M. Faraday
P.H. Berthon Esq | &c &c &c

1. Letter 3787.

2. This letter, which deals with the apparatus for the Trincomalee lighthouse, was read to Trinity House By Board, 19 June 1860, GL MS 30010/43, pp. 46–7.

Letter 3793

Faraday to Angela Georgina Burdett Coutts

26 June 1860

From the original in BL Burdett-Coutts papers

Royal Institution | 26 Jun 1860

Dear Miss Coutts,

I venture to submit the enclosed card as that of a case I know well & feel deeply for¹_[.]

Ever Your faithful Servant | M. Faraday

1. This was for a proxy for the Infant Orphan Asylum in Wanstead, which was similar to the London Orphan Asylum (see note 2, letter 3516) but which took children under the age of seven. On this asylum see Grist (1974), 1–42. The card enclosed with this letter showed that Faraday wanted the proxy for Minnie Leighton. Faraday is not listed as a subscriber in the records of the asylum in RLSA, but there was no prohibition on non-subscribers seeking to place an orphan in the asylum. Coutts had been a subscriber since 1838. See the 1861 *Report of the Infant Orphan Asylum* RLSA 90/21/10/18, p. 82.

Letter 3794**Faraday to Charles Brooke****26 June 1860****From the original in SI D MS 554 A**

26 Jun 1860

My dear Sir,

Help us if you can¹[.]

Ever Truly Yours | M. Faraday

Chas. Brooke Esqr | &c &c &c

1. This presumably referred to Faraday's request for a proxy vote for Minnie Leighton for a place in the Infant Orphan Asylum (see note 1, letter 3793). Brooke had been a subscriber since 1854. See the 1861 *Report of the Infant Orphan Asylum* RLSA 90/21/10/18, p. 46.

Letter 3795**Stroud¹ to Faraday****July 1860****From Bence Jones (1870a), 2: 441**

When combating the Old Testament narrative of the creation of man², he (Mr. Wild³) adverted to certain chemical experiments which he has alleged were made by you some years since, before audiences, both at Oxford and Cambridge, and also in London, when you demonstrated that life was but electricity, by producing through its agency animalcules, maggots, &c., accompanying those experiments by the remarks addressed to your audiences, as: "Gentlemen, there is life, and, for aught I can tell, man was so created."⁴ Mr. Wild has always held it (and has related the circumstances to show) that you inferred from your experiments that man could be created or generated, and in all probability was created, in the same *modus operandi* as by your experiments.

Mr. W., in relating the above, has always added that so unpalatable were your views, and contrary to what was received as orthodox, that the authorities used whose auspices the lectures were given (at which you experimented) had them discontinued.

1. Unidentified.

2. Genesis 2: 7.

3. An unidentified lecturer on Paddington Green. See Bence Jones (1870a), 2: 441.

4. On this see Stallybrass (1967) and Secord (1989).

Letter 3796**Faraday to George Frederick Chambers¹****5 July 1860****From the original in RI MS F1 N/1/50**

S.W Hampton Court | 5 Jul 1860

Sir,

One of the most powerful & ready things in dec[o]louring the solution of permanganate of potassa is a little solution of sulphurous acid. Being what

Schoenbein calls an ozone solution a trace almost of oxywater or per oxide of potassium or sodium or barium will do the same thing there being antiozone compounds. As you think a salt of soda was used so I have no doubt that a *sulphite* of soda would answer the purpose. A very little only of any of these things is necessary to decolour a deep coloured solution of the permanganate. Put a little water into a bottle burn a sulphur match in the bottle – shake the fumes & the water together – filter it & you will have a solution that will do[.]

I am Sir | Your Obedient Servant | M. Faraday
Geo. F. Chambers Esq | &c &c &c

1. George Frederick Chambers (1841–1915, WW1). Studied science at King's College, London. Later a barrister and amateur astronomer.

Letter 3797

Faraday to Stroud¹

6 July 1860

From Bence Jones (1870a), 2: 441–2

The Green, Hampton Court | 6 July 1860

Sir,

Your letter² has surprised me a good deal, for I did not know before that my name had been used as you describe, and cannot now imagine how it has been employed upon that side of the argument where your letter places it. I send herewith a part of your letter (*which, however, I will thank you to return to me again*). All that part which is between my initials on pp. 5 and 6 is utterly untrue. I never made animalcules or maggots by the agency of electricity, and when others said they had done anything of the kind, opposed their views, and all the conclusions derived from them³. I never lectured on science at Cambridge at all; no lectures of mine have been discontinued, and if I have given offence (which I can only imagine in the case of one person), it has been because I was supposed to pay too much respect to the Bible, which I believe to be the *Word of God*.

Some years ago I delivered a *lecture on education*⁴, which has since been reprinted at the end of a volume of *Juvenile Lectures on the forces of matter*⁵, just published by Griffin⁶, I believe⁷. Near the beginning of that lecture you will find a public answer to the inquiries which you make at the close of your note.

Your letter states that the object of the meetings on Paddington Green is the elucidation of truth. As far as your letter goes, they appear to me to have been effectual mainly in the generation and propagation of *error*.

You are at perfect liberty to use this letter in connexion with the subject in any way you may think fit.

I am, Sir, your very faithful servant | M. Faraday

1. Unidentified.

2. Letter 3795.

3. On this see Stallybrass (1967) and Secord (1989).

4. Faraday (1854b).

5. Faraday (1860b).

6. Scottish publishers founded by Richard Griffin (d.1832, age 43, B1 under Charles Griffin).

7. This is a mistake. Faraday (1854b) was not republished in Faraday (1860b), but his Friday Evening Discourse of 9 March 1860, 'On the Illumination of Lighthouses – The Electric Light', *Chem. News*, 17 March 1860, 1: 171–4 was republished in Faraday (1860b), 155–74.

Letter 3798

Faraday to Agnes Greig¹

6 July 1860

From the original in Bod MS Somerville dep b. 233 MS 3 F1

The Green Hampton Court | 6 Jul 1860

My dear Mrs. Greig,

I had heard of the death of Dr. Somerville² before I received your very kind note, and we were all deeply grieved in thinking of Mrs. Somerville, and the shake it would give to her feelings and immediate relations. No doubt the shock would be greater, because of the sudden coming on of danger & the end, after 30 hours of trust & reasonable expectation of recovery. Will you sympathize with Mr. Greig³ on our part. We feel very much with you both & with Mrs. Somerville, for our thoughts have been, & are now, much on death, because of the loss of several old & valued friends very recently⁴:- but I often feel when our attempts to say anything on the matter to those who are closely concerned, one runs a risk of speaking feebly or foolishly. Let us hope that in regarding the death of our dear friends, or in waiting for its approach to ourselves, we may be strengthened to look upon it with great hope.

Ever My dear Mrs. Greig | Most truly Yours | M. Faraday

Address: Mrs. Grieg | Surrey Lodge | Lambeth | S

1. Agnes Greig, née Graham (d.1874, age 67, GRO). Married Woronzow Greig, October 1837.

2. William Somerville (1771–1860, ODNB). Physician to the Royal Chelsea Hospital, 1819–1838. Died on 25 June 1860.

3. Woronzow Greig (1805–1865, B1). Son of Mary Somerville from her first marriage. Barrister.

4. These would have included Charles Barry on 12 May 1860 (see letter 3781) and the Dundee surgeon and member of the Glasite church John Crichton (1772–1860, B1) on 3 July 1860.

Letter 3799

Faraday to François Napoleon Marie Moigno

10 July 1860

From the original in AS MS Collection Bertrand

The Green Hampton Court | 10 Jul 1860

My dear Abbe,

My Tyndall has shewn me your note and I immediately packed up the last of my volumes which consists of papers Chemical and Physical¹ & have addressed it to you and left it in the Hall of the Royal Institution that M. Serrin²



Plate 12. Michael Faraday. Photograph taken by Charles Dodgson on 30 June 1860. From Collingwood (1898), 135.

may have it as he returns from Oxford³[.] I thought you had had the volume before but I suppose I was mistaken[.]

M. Serrin was at Oxford with his lamp which as far as I could judge seemed excellent in construction. He asked me for an opinion on it but as I never give an opinion for any invention brought to me I could not do so for him. Happily he was able to shew it at Oxford for though there was not a sufficient battery belonging to the establishments there a Gentleman of the name of Way who had a rival lamp there and a battery of his own was magnanimous enough to lend his battery to M. Serrin for his lamp whilst his own remained *unlighted & out of use*. I thought this exceedingly liberal of him[.]

I was so ill at Oxford that I could not remain there but left on the Saturday – so I do not know what happened afterward⁴. M. Serrin stopped I suppose to the end. M. Verdet⁵ was at Oxford and I was very glad to see him[.]

I have no news to tell you for I am very oblivious but I am Ever

Very Truly Yours | M. Faraday

a monsieur | M. l'Abbé Moigno | &c &c &c

1. Victor Louis Marie Serrin (b.1829, Stock (1990)). French inventor of an electric lamp.

2. Faraday (1859b).

3. From the meeting of the British Association.

4. This may be an oblique reference to the discussion on the afternoon of 30 June 1860 at the British Association over the theory of evolution by natural selection proposed by the naturalist Charles Darwin (1809–1882, ODNB). See James (2005) for an account of the discussion, the main protagonists of which were the Bishop of Oxford from 1845 to 1869, Samuel Wilberforce (1805–1873, ODNB), Thomas Huxley and Joseph Hooker. Faraday left Oxford after being photographed that morning by the mathematician, author, photographer and Fellow of Christ Church, Charles Lutwidge Dodgson (1832–1898, ODNB); see Taylor *et al.* (2002), 251. The image is reproduced as Plate 12.

5. Marcel Emile Verdet (1824–1866, P2, 3). Professor of Physics at the Ecole Normale, Paris.

Letter 3800

Peter Henry Berthon to Faraday

10 July 1860

From the original in GL MS 30108/5, f. 13

Trinity House London | 10 Jul 1860

Sir,

I beg to transmit to you the accompanying copy of a Letter from the Secretary to the Royal Commission for Lights¹ &c: in relation to the adjustment of the Dioptric Apparatus in Light Houses and am to express the Hope of the Elder Brethren that it may suit your arrangements to attend them on Thursday² next at $\frac{1}{2}$ past one 'o'clock in relation thereto.

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday F.R.S. | &c &c &c

Endorsed by Faraday: Went &c &c

1. John Francis Campbell.

2. That is 12 July 1860.

Letter 3801**Sarah Faraday to John Tyndall****11 July 1860****From the typescript in RI MS JT TS Volume 12, p. 4168**

Hampton Court | 11 Jul 1860

Dear Dr. Tyndall,

I was in hopes of thanking you personally last Saturday¹ for the flattering mark of your kind feeling which I received in the present of your book², but as I did not see you, I must send a few words to tell you how highly I esteem it; I shall not attempt to speak of its merits for which I am quite incompetent, but I may say that in the little I have read so far, I see there is a great deal to interest *even me*[.]

Your short visit was very tantalizing, for we hope it will be repeated before long when we shall have more time.

Believe me dear Dr. Tyndall | with warm interest in your welfare |
Most sincerely yours | S. Faraday

1. That is 7 July 1860.

2. Tyndall (1860).

Letter 3802**Peter Henry Berthon to Faraday****11 July 1860****From the original in GL MS 30108/4/113**

Trinity House | 11 Jul 1860

Dear Sir,

I am directed to forward to you the accompanying Bottle containing a Sample of Filtered Rain Water, used for Drinking, received from the Flambro' Head Light House stated to contain Lead in Solution & to request that you will favor the Elder Brethren by making an analysis of the same, at your Convenience, & furnishing me with a Report of the result.

I am | Dear Sir | Your's faithfully | P.H. Berthon
M. Faraday Esq. | &c &c &c

Letter 3803**Faraday to Cecilia Anne Barlow****13 July 1860****From the original in RI MS F1 E22**

The Green Hampton Court | 13 Jul 1860

My dear Mrs. Barlow,

I have written to Miss Coutts to say we shall be here next Monday and Tuesday¹. Tomorrow I must be in town. We shall be very happy to see her &

her friends for I think Miss Coutts is so sincere & valuable a person & knows so well our peculiar ways that there is no chance of her proposing any thing that not be satisfactory to herself or friends. I have full trust in her as I should have in you_[.]

Ever faithfully Yours | M. Faraday

Address: Mrs. Barlow | 5 Berkeley Street | Piccadilly | W.

1. That is 16 and 17 July 1860.

Letter 3804

Faraday to Peter Henry Berthon

16 July 1860

From the original copy in GL MS 30108/4/113

Royal Institution | 16 Jul 1860

Dear Sir,

The water you sent me from Flambro' head¹ contained a little lead in solution, there was also a deposit which was chiefly lead & which I have no doubt had been in solution also when the water was filtered & bottled. The water contained likewise that common salt which would give it the power of acting on lead & which had been derived from the spray of the sea.

As in the case of the water from Hurst the lead in the present water was entirely removed by a little powdered chalk or whitening_[.] Enough of this substance was added to make the water look like milk and stirred up with it for a minute after which it was allowed to clear either by filtering or subsidence. The dissolved lead was at once removed by this process².

Ever Dear Sir | Most truly Yours | M. Faraday

P.H. Berthon Esqr | &c &c &c

1. See letter 3802.

2. This letter was read to Trinity House By Board, 17 July 1860, GL MS 30010/43, pp. 72–3. It was referred to the Lights Committee.

Letter 3805

Peter Henry Berthon to Faraday

17 July 1860

From the original in GL MS 30108/5, f. 15

Trinity House, London. E.C | 17 Jul 1860

Sir,

Referring to the interview you had with a Committee of the Elder Brethren at this House on 12th instant¹, on the subject of Defects which the Commissioners on Lights &c stated, in a Letter of which you then had a Copy,

they had discovered in the Lighting Apparatus at the North Foreland and at the Whitby Light Houses², I am directed to intimate the request of the Board that you will take the earliest available opportunity before the 2nd Proximo, of visiting those Light Houses and thoroughly examining the Apparatus with a view of ascertaining if practicable the nature of the defects to which the Commissioners allude.

I am | Sir | Your most humble Servant | P.H. Berthon
Professor Faraday | &c &c &c

Endorsed by Faraday: Declined to go under the circumstances – without first writing to or hearing the Commissioners.

1. See letter 3800.

2. *Parliamentary Papers*, 1861 [2793], XXV, volume 1, pp. 80–1 and 79–80 respectively.

Letter 3806

Faraday to Thomas Newborn Robert Morson¹

24 July 1860

From the original in SI D MS 554 A

The Green Hampton Court | 24 Jul 1860

My dear Morson,

A friend of ours whom we both respect very highly (but who does not wish his name mentioned at present) has asked me who are the Manufacturers of Chemical, not of mere Soda or bleaching powder, but for scientific photographic & general use. His object may be to introduce his son into the trade and I (who know not of my own knowledge) promised to enquire. I do not know whom to enquire at so likely as you. Can you give me the names of some. I do not suppose there are many[.]

Ever Truly Yours | M. Faraday

1. Thomas Newborn Robert Morson (1799–1874, ODNB). Pharmaceutical chemist.

Letter 3807

Faraday to Thomas Newborn Robert Morson¹

26 July 1860

From the original in SI D MS 554 A

Hampton Court | 26 Jul 1860

My dear Morson,

I am very much obliged by your reply to my enquiry². I shall send it just as it is to “our friend” – whose name I expect you will shortly know[.]

Ever Truly Yours | M. Faraday

1. Thomas Newborn Robert Morson (1799–1874, ODNB). Pharmaceutical chemist.

2. Letter 3806.

Letter 3808**Faraday to Mary Ann Chantrey¹****26 July 1860****From the original in the possession of V.A. and J.R. Johnstone**

The Green Hampton Court | 26 Jul 1860

Dear Lady Chantrey,

You are far too kind for I did not expect an answer or wish to trouble you for one² – I am nevertheless Your very thankful & Grateful Servant | M. Faraday

1. Mary Ann Chantrey, née Wale (1787–1875, ODNB under F.L. Chantrey). Married Chantrey in 1809.

2. This presumably referred to Faraday's request for a proxy vote for Minnie Leighton for a place in the Infant Orphan Asylum (see note 1, letter 3793). Chantrey had been a subscriber since 1854. See the 1861 *Report of the Infant Orphan Asylum* RLSA 90/21/10/18, p. 64.

Letter 3809**Peter Henry Berthon to Faraday****26 July 1860****From the original in GL MS 30108/5, f. 16**

Trinity House | 26 Jul 1860

My dear Sir,

The Royal Commission have at last offered to enlighten us on the subject of the alleged defects in the lighting Apparatus¹, and an Arrangement has been made for a Committee of the Brethren to meet them at their Office 7 Milbank Street Westminster at 3 O'clock PM on Monday next², – at which time the Deputy Master³ earnestly hopes that it will suit you to meet the Brethren there; and see as the Boys say "what you shall see"⁴.

The Mountain is parturient I trust it may only bring forth a "ridiculous mess"⁵._[.]

Yours in great [word illegible] | P.H. Berthon

M. Faraday Esq | &c &c &c

1. See letter 3805.

2. That is 30 July 1860.

3. Robert Gordon.

4. A reference to Cary (1852), 264.

5. A deliberate reworking of Aesop fable 26.

Letter 3810
Faraday to Benjamin Vincent
27 July 1860
From the original in RI MS F1 E22a

H. Court | Friday, 27 Jul 1860

My dear friend,

Miss Hornblower has had her limb removed. I was to let you know when the operation was over – the enclosed is the account I have just received. How thankful we may be – tomorrow I shall hear how the case progresses & shall see you & report. Mr. Bowman was to perform the operation[.]

Yours Very Affectionately | M. Faraday
 Mr. Vincent

Letter 3811
Faraday to Waugh and Sons¹
28 July 1860
From the original in RI MS F1 A29

Royal Institution | 28 Jul 1860

Gentlemen,

I received your estimate for a carpet & have submitted it to our Authorities. At present they do not proceed to order a carpet. I do not know what their ultimate course will be, but keep your estimate to be used if the occasion occurs – and if there be occasion will let you know²[.]

Your Obedient Servant | M. Faraday
 Messrs. Waugh & Sons

1. Waugh and Sons. Carpet suppliers of 3 and 4 Goodge Street. POD.

2. The Royal Institution purchased a rug and carpet from Waugh and Sons a year later on 20 July 1861. RI MS Ledger, 1858–1866, p. 278.

Letter 3812
Faraday to William Francis Cowper
3 August 1860
From the original in SI D MS 554 A

Hampton Court Green | 3 Aug 1860

Dear Sir,

I found your letter¹ on returning from certain lighthouses². I do not see how I can help you in the Szerelmey–Ransome matter³. I do not know M Szerelmey, and I believe he wishes to keep his process secret. I answered your enquiries as well as I could with the knowledge I had; and have no

objection to the matter as it appears in the printed returns to the House of Commons⁴, though I would rather have had my letter⁵ (which you consider private) amongst the rest.

If you consider an analysis necessary for your object, I conclude that some of the Professional men attached to the Government, at the Jermyn Street Museum, – Woolwich; or elsewhere will be the proper persons to undertake it – For my own part I think *time* (as I said in that letter) is the *only* test of such a practical matters [sic].

I have lately had a visit and a threat of legal proceedings from Mr. Daine⁶ on account of my answers to your questions⁷. I will candidly confess that such results, cool, in some degree, my willingness to answer all enquiries made of me by the Governmental boards. If I thought that such a case were likely to occur again I would make *all* my letters *private* to prevent like results. Whenever you give me the pleasure of being any way useful to you again I hope you will help me to keep clear of the parties:– whose object is of course profit[.]

I am Sir | Very faithfully Yours | M. Faraday
The Right Honble | W. Cowper Esqr | &c &c &c

1. Not found.

2. At the North Foreland which Faraday visited on 2 August 1860. See letter 3828.

3. See letters 3610, 3613, 3614 and notes.

4. *Parliamentary Papers*, 1860 (309), XL.

5. Letter 3785.

6. See Bence Jones (1870a), 2: 435 for an account of the visit.

7. See letter 3778.

Letter 3813

Faraday to William Francis Cowper

4 August 1860

From the original in SI D MS 554 A

Private

[Royal Institution embossed letterhead] | 4 Aug 1860

Dear Sir,

When I was first called in to form some kind of judgment respecting the preservation of the stone work, I was to be guided by the appearance and state of the prepared specimens, and these alone. When I met M. Szerelmey at the Houses of Parliament¹, the engagement with him was already made; as Sir Chas. Barry, who was then present, told me:– but he was permitted to retain his secret. I tasted the liquid in one of his buckets, and have no doubt it was an alkaline silicate:– but I did not take any of it, nor ask him for it, as he would have then kept me to secrecy, and I did not chose to be in that position. I should not have thought it honest to take any of his preparation without his knowledge. I think you ought to have some security that he is

dealing honorably & fairly with you;– but the permission was given long ago that he might retain his secret,– and unless he freely yields up the knowledge I do not know what you can do; except to decline upon principle dealing with a person who has a secret, and over whom, consequently, you can have no hold[.]

I hope you understand that, in my opinion, a process, whether secret or open, whether guided by most promising principles or altogether by rule of thumb, can only be judged of by the proof of times action[.]

I have the honor to be | Dear Sir | Your Very faithful Servant |
M. Faraday
The Right Honorable | William Cowper M.P. | &c &c &c &c

1. On 30 June 1859. See letter 3610.

Letter 3814

Faraday to James Timmins Chance

4 August 1860

From the original in RI MS F1 N/4/4

[Royal Institution embossed letterhead] | 4 Aug 1860

My dear Sir,

Have you perchance a fragment or a bad piece of one of the upper reflectors which you could bring to Whitby next week and by which we could see practically what amount of change in position would suffice to make the image of the horizon travel over a certain small angle?

Ever Truly Yours | M. Faraday
Jas. Chance Esq | &c &c &c

Letter 3815

Julius Plücker to Faraday

4 August 1860

From the original in IET MS SC 2

Bonn | 4 Aug 1860

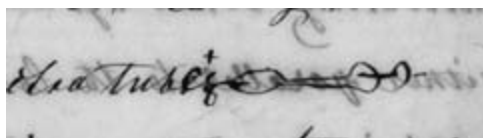
My dear Sir,

I thank you for the kind letter of March 29th¹ and deeply regret, to learn from it, that your state of health is not a quite satisfactory one. The same told me Professor Baumert² of Bonn, who met you at the Oxford Meeting³.

Chevallier Bunsen⁴ – as he was called when Prussian ambassador in London – who resides now among us, speaks of you with great enthusiasm, remembering with pleasure the happy hours, he passed with you. He charged myself to present to you his respects.

I sent an Abstract of my researches on the electric discharge through evacuated tubes to the Royal Society, October last, to be printed in the Proceedings. Professor Stokes informed me three months ago, that my paper was either already printed, or would be printed immediately⁵. But since I heard nothing about it. I wished I could show you the beautiful coloured lines of the spectra of the different gases, by which these gases are fully characterised and chemically analysed. Since Professor Bunsen examined in the same way the (generally less well defined) spectra of Kalium Sodium, Lithium, Calcium, Strontium and Barium, proving that these spectra solely depend upon the mere metal, like gases are determined by one of their brilliant spectra-lines. In this way he detected a new metal⁶, varying among the above mentioned, on examining the spectra of the residue from mineral waters, which he introduced into the flame of a gaz-lamp. A Chemist would certainly obtain curious results by a closer examination of gaz spectra. Some rather bold conceptions of mine, concerning for instance the beautiful spectra of chlorine, brome and iodine had hitherto no success.

Since my last communication I got only a few results concerning the gaz spectra⁷. The spectra remain the same, the gases may be rarefied, as they are in Geissler's tubes, or in the state of ordinary density. In this last case too, the discharge of a stronger apparatus of Ruhmkorff's easily passed through them, when contained in a capillar tube.— In order to prevent the influence of the Electrodes I recently substituted to my former spectra tubes



other ones into which no wire enters: the gazes included becoming luminous by *induced* currents



The spectra thus produced are equally beautiful.

I'll take liberty to send you by occasion my last paper on the magnetic condition of the different kinds of mica, compared with their optical properties⁸. The plane of the two magnetic axes is always perpendicular to

the plane of the two optic axes, whatever may be the angle of these axes. When the plane of the two magnetic axes passes through the shorter diagonal, the plane of the two optic axes passes through the larger one and vice versa. The middle lines of both systems of axes are the same. Mica if optically uniaxial is equally so magnetically. If the angle of the axes be small, there is no rule indicating what system of luminous vibrations (either parallel to the longer or to the shorter diagonal) is more absorbed. This last result seems favourable to the views of Mr. de Sénarmont⁹, who thinks that the different micas are composed of two normal species having their optic axes within two planes perpendicular to each other¹⁰.

I beg you, Sir, to present my respects to Mad. Faraday. With all my heart
Yours | Plücker

1. Letter 3753.
2. Friedrich Moritz Baumert (1818–1865, DBE). Taught chemistry at the University of Breslau.
3. At the meeting of the British Association.
4. Christian Karl Josias Bunsen (1791–1860, ODNB). Prussian ambassador in London, 1841–1854.
5. Plücker (1860a).
6. This was caesium. See James (1983a), 43.
7. On this see James (1983b), 153–6.
8. Plücker (1860b).
9. Henri Hureau de Sénarmont (1808–1862, DSB). French crystallographer.
10. Sénarmont (1852).

Letter 3816

Faraday to Lambert-Adolphe-Jacques Quetelet¹

6 August 1860

From the original in Bibliothèque royale Albert 1er, Académie royale de Belgique Archives No 17986/989

Royal Institution | 6 Aug 1860

My dear Mr. Quetelet,

Your letter² gave me great pleasure containing as it did so agreeable a mark of your remembrance. I heard you had been in London³ and at one moment hoped to meet you at Miss Coutts but could not get there – Just as I learned that you were at the house of M. Van der Weyer⁴ & was preparing at all event to leave a card for you, you were gone. I know how much your hours would be in request at the beginning of things, & I hope they were not shortened here by any cause of anxiety at home[.] May you be happy there – in that in that [sic] indeed which makes the true and real part of life.

I thank you very much for the many kind scientific remembrances which you send me – proving as they do your active & powerful exertions in the cause of science. I have little or nothing to send you in return – only a short note on regelation⁵ which this post will bring you – All things wear out and philosophers amongst the rest: and for my part I think it best that we should

have this lesson & be content & happy in our latter years – in possession of the many blessings that are granted to an humble & satisfied mind.

Ever my dear friend | Very Truly Yours | M. Faraday

1. Lambert-Adolphe-Jacques Quetelet (1796–1874, DSB). Astronomer at the Brussels Observatory from 1828 and Permanent Secretary of the Brussels Academy from 1834.

2. Not found.

3. For the meeting of the International Statistical Congress in July 1860 which Quetelet attended. *The Times*, 17 July 1860, p. 5, col. a.

4. Jean-Sylvain Van de Weyer (1802–1874, BNB). Belgian ambassador to London.

5. Faraday (1860d).

Letter 3817

Faraday to William Francis Cowper

7 August 1860

From the original in SI D MS 554 A

[Royal Institution embossed letterhead] | 7 Aug 1860

Dear Sir,

I am very much obliged by your kind consideration of me¹. I cannot analyse M. Szerelmeys preparations_[.] It is especially proper that I should not do so – for I was bound at first to give an opinion *without* knowing the composition and I would rather not alter my position now. There can be no possible objection to an analysis made at Jermyn Street_[.]

Ever Your faithful Servant | M. Faraday

The Right Honble | William Cowper M.P. | &c &c &c &c

1. See letter 3812.

Letter 3818

Faraday to Benjamin Vincent

7 August 1860¹

From the original in RI MS F1 G25

I go off tomorrow morning to Whitby & may not be home before Saturday²_[.]

[Royal Institution embossed letterhead], Tuesday | 5 o'clk

My dear friend,

We came home about an hour ago. I found your note – have been to Harley St – have received an *improved* account – Our friend³ has eaten some fish to day & is progressing. Mr. Bowman saw her two hours ago & is quite satisfied. I only saw the porter_[.]

Affectionately Yours | M. Faraday

Mr. Vincent

1. Dated on the basis of the reference to Faraday's journey to Whitby and Hornblower's operation.

2. That is 11 August 1860.
3. Jemima Hanbury Hornblower. See letter 3810.

Letter 3819**Peter Henry Berthon to Faraday****8 August 1860****From the original in GL MS 30108/3/90.27**

Trinity House, London, EC | 8 Aug 1860

Sir,

Referring to the Proposition made by Major the Hon. W.E. FitzMaurice in respect of a Trial of his Light in one of this Corporation's Light Houses, – I am directed to forward to you the accompanying Copy of the Replies which that Gentleman has given to the various enquiries suggested in your Report of the 8th February¹, – and I am to request you will favor the Corporation with any observations which you may deem it necessary to offer thereon.

I am | Sir | Your most humble Servant | P.H. Berthon
M. Faraday Esq.,– F.R.S.; | &c &c &c

1. Letter 3725.

Letter 3820**Faraday to James Timmins Chance****13 August 1860****From the original in RI MS F1 N/4/5**

[Royal Institution embossed letterhead] | 13 Aug 1860

My dear Sir,

You offered at one time to give me a drawing of your curves &c but I had in the mean time sent it back¹. That makes me ask your help in this way[.] Can you give me a drawing full size on a long sheet of cartridge paper of the position of your pieces of glass? I want to consider for myself in conjunction with experiment on a good lamp where I think the focal points should be taken. I do not want the angles of each piece with great accuracy but their position in relation to the lamp, so as to give their distance from it, & the angles which the rays proceeding from it to them make with the horizontal ray – also the distance between the burner & focal plane.

Ever My dear Sir | Truly Yours | M Faraday
Jas. Chance Esq | &c &c &c

1. See letter 3657.

Letter 3821**Robert Gordon to Faraday****13 August 1860****From the original in GL MS 30108/5, f. 27**

17 Pall Mall | 13 Aug

My Dear Sir,

I forgot to mention before we parted at Whitby, that I shall be glad if you will let us have a "Report" upon the investigations that have been made at the No Foreland & at Whitby¹.

Your's very truly | Robt. Gordon

1. Faraday had been at Whitby the previous week. See letter 3822.

Letter 3822**Faraday to James Timmins Chance****14 August 1860****From the original in RI MS F1 N/4/6**

Royal Institution | 14 Aug 1860

My dear Sir,

Had the lamp at the North lighthouse at Whitby been adjusted before our survey of matters on the 9th instant? or was it as you left it originally and as it was when the Astronomer Royal saw it?¹ Were you with him there? and what was the judgment regarding the upper reflectors formed from observation within the lanthorn at that time?

Ever Truly Yours | M. Faraday

James Chance Esq | &c &c &c

The bundle of photographs and the drawing have been found. They came to me but I return them to Mr. Wilkins for conveyance to M. Sautter | MF

Endorsement: *North Whitby* | Lamp 27 mm below focal plane of lenses. Same position as it had always been – not capable of adjustment – no change made.
| Lamp not in center.

1. Airy had visited Whitby in June 1860. Airy, W. (1896), 241.

Letter 3823**Faraday to Angela Georgina Burdett Coutts****August 1860¹****From the original in BL Burdett-Coutts papers**

[Royal Institution embossed letterhead]

My dear Miss Coutts,

Talking with Madame De la Rive this morning about Education I spoke to her of Your schools. I could not make up my mind to spare my copy of your book² or else it would have gone to her:– but I find that they have been

to your house & that you know the De la Rive's_[.] Is it a very wrong thing of me to think it possible you might like to send a copy of your practical work them. They are at 21 Dover St until Monday when they they return for a while to Geneva_[.] I have perhaps said too much.– I dare say no more – 'Tis your kindness that encourages me_[.]

Ever Your faithful Servant | M. Faraday

Address: Miss A.B. Coutts | Stratton Street | Piccadilly

1. Dated on the basis that letter 3824 is related to this letter.
2. Presumably Burdett Coutts (1860).

Letter 3824

Faraday to Angela Georgina Burdett Coutts

15 August 1860

From the original in BL Burdett-Coutts papers

[Royal Institution embossed letterhead] | 15 Aug 1860

Dear Miss Coutts,

If you like I will take charge of your book¹ for Madame de la Rive. He will be here again towards the end of the year & then he can have it either from you or me_[.]

Ever Your faithful Servant | M. Faraday

Address: Miss Coutts | 1 Stratton Street

1. Presumably Burdett Coutts (1860).

Letter 3825

James Emerson Tennent to Faraday

15 August 1860¹

From the original in RI MS Conybeare Album, f. 26

Board of Trade, SW | 15 Aug

My dear Sir,

I am so hopeful that Mr. Duncan Dunbar can be of service in the matter which interests you, that I am very desirous you shd. see him – and I enclose a note of Introduction to facilitate an interview_[.]

But to render it still more agreeable, I have written to Mr. D. Dunbar to acquaint him, that I have given you the letter of introduction;– and very briefly to set out the nature & object of your visit to him. This will prepare him to see you.

But in the mean time, as he lives far off; and as he leaves home early for the city; I think your prudent course will be for you to *write* a note to him, & to ask him to for a time & to name a place to receive you. “– at his own house

if he so prefers". – or perhaps it might consult his convenience, if in passing Albemarle St. he would *call upon you*?

You can give him the choice of either.

Heartily wishing you success in this matter, and greatly regretting that my own power is so disproportionate to my great desire to gratify you, by serving Cap Deacon_[.]

Believe me | Faithfully Yours | J. Emm Tennant

I have sent to Mr. D. Dunbar, the mem: you left with me of Capt Deacons Services afloat.

1. Dated on the basis that letter 3826 is the reply.

Letter 3826

Faraday to James Emerson Tennent

15 August 1860

From the original in the possession of Y. Watanabe

[Royal Institution embossed letterhead] | 15 Aug 1860

My dear Sir Emerson,

I am very deeply indebted for your prompt & large kindness¹ and know not how to thank you rightly in words_[.] If I knew Mr. Duncan Dunbar I should speak to him for I think he is just the man to appreciate Mr. Deacon['s] open sincerity & desire to work. I ought to have told you that last year he was upon your own ground and waters. Being in command of a merchant vessel there he was offered the command of the Rail Government vessel. Those who had been desired to look out for a Captain thought so well of him that they offered to wait 6 months for him but having been in India &c these 2 years he desired to come home & has not been many months here_[.]

Thanking you most sincerely & believing that if accidentally or otherwise any thing occurs to you in the matter you will tell me_[.]

I am | Very Gratefully Yours | M. Faraday

Sir J. Emerson Tennent Bart | &c &c &c

1. See letter 3825.

Letter 3827

James Timmins Chance to Faraday

15 August 1860

From the original in GL MS 30108/5, f. 28

Hamstead Birmingham | 15 Aug 1860

My dear Sir,

I have yr. two notes of Agt.13 &14th¹.

Tomorrow I will send you the plan of the 1st order Lights as at Whitby.

No change has been made in the lamps at Whitby: they are doubtless in the position which they had in 1858, and at the time of the Astronomer Royal seeing them².

I was *not* at Whitby with the Astronomer Royal.

I presume that he – or the Royal Commissioners – will have communicated to the Trinity Board the defects wh: he discovered in the two Whitby Lights respectively.

It was understood that I should write to the Trinity Board concerning the rectification of the Whitby Lights – beginning with the South one.

I should like very much to receive, before doing so, some statement of the degree of error in the lenses – and the lower prisms – especially, as also in the upper prisms.

My object is to show how much of the error in each portion is due to the dip (supposing the received calculations to have been accurately followed) – & how much to the fault of the manufacturer.

If, however, you think that I had better address the Board without having this information, I will do so with pleasure: for I shall be guided by yourself in the matter.

Most truly yours | J.T. Chance

1. Letters 3820 and 3822.

2. Airy had visited Whitby in June 1860. Airy, W. (1896), 241.

Letter 3828

Faraday report to Trinity House

16 August 1860

From the original copy in GL MS 30108/5, f. 29–37

Report, &c. To the Deputy-Master¹ & Brethren of the Trinity House

Royal Institution | 16 Aug 1860

The Royal Commission on Lights &c, having desired to meet the Brethren of the Trinity house at the North Foreland and Whitby Lighthouses, with certain other persons, such as the manufacturers of the apparatus, the representatives of the Irish and Scotch lighthouses, the Astronomer Royal, &c, for the purpose of making certain practical communications to them which could not be so well done by writing or elsewhere; such meetings have taken place. The one at the North Foreland occurred on the 2nd of August; when there were present Adml Hamilton, – Capt'n Ryder, Dr. Gladstone, the Astronomer Royal, and Mr. Campbell, of the Royal Commission;– Adml Gordon, Capt'n Close, Capt'n Baily², [blank in MS]³ of the Trinity House; Sir James Dumbrain [sic]⁴ of the Irish Board⁵;– Mr. Stevenson of the Scotch Board; Mr. Sautter of Paris, the maker of the apparatus with Mr. Wilkins;– and myself;– the other at Whitby occurred on the 9th of the same month; when

the same persons were present, with the exception of the Astronomer Royal, Capt'n Bailly, & Sir James Dumbrain; and the addition of Mr. Halpin⁶ of the Irish Ballast Board, and⁷ Mr. J. Chance, the manufacturer of the apparatus, with M. Masselin. I was there on the part of Trinity house, as their Scientific adviser in Experimental lights; and though I do not wish to assume the character or responsibility of Optical or Civil engineer, I feel it my duty, at the call of the Deputy Master⁸, to give my impression and conclusions. Having received no descriptive written document from the Royal Commission, I may perhaps pass by some points requiring attention, unawares.

The object of the R Commission was, I believe, to point out the necessity of a final examination of the optic apparatus in the lighthouse itself, after its erection, and the demonstration of a mode of making that examination in a practical manner, which I may call the method of the Commission. The object the optic apparatus in a lighthouse is to convey the rays from the source of light to the sea horizon, or to the parts of the sea between that horizon and the shore, in the most abundant degree possible & in the most favourable manner. This effect is obtained by a certain position & adjustment of the parts of the refracting & reflecting apparatus and whether these are right in this respect, may be ascertained by a process in some degree the reverse of the illuminating method. Thus; suppose the lamp is lighted, a given bright spot in its flame selected, and a given piece of the glass apparatus adjusted, so as to transmit the light passing through it from the chosen spot to the sea horizon:—then, if the adjustment be right, an observer placed on the other side of the flame, & looking through the spot and the glass, will see the horizon; & this is an observation easily made in the daytime, either with the flame actually existing, or with indicating gauges at the burner (as the edge of a card for instance) representing given or selected parts of the flame. If the horizon does not appear in the right place the adjustment is wrong. The same kind of test may be applied to every part of the apparatus in turn, whether refractive or reflective;— in respect of any part of the horizon, or the sea, or the sky; and in respect of any part of the flame or luminous object. A luminous point being selected, that place which the eye sees through it, will be the place to which the rays issuing from it in that direction, will proceed.

This appears to me to be an excellent practical application. In principle it is perfect. The application however cannot be equally perfect because of the inevitable imperfections of construction. Still the quality of the glass and its workmanship have arrived at such a degree of excellence, as to justify the application of this refined kind of inspection; which may be anticipated & worked up to, by processes applicable in the manufactory.

Assuming that the apparatus is as perfect in its execution as can be expected, then the causes that may interfere with due effect, (and chiefly by mal-adjustment) are several. First, those connected with the *lamp & flame*. The Fresnel lamp has a burner & cottons having a horizontal width of $3\frac{3}{4}$ of an inch

in diameter;— the flame as it rises from this base is obscure above & near to the cotton, then becomes luminous and powerful, and, contracting as it rises, usually ends in a series of forky tongues. When supplied by an overflowing lamp urged by the draught of a good chimney, the bright part of the flame may be from $2\frac{1}{2}$ to 4 inches from the bottom to the top of the chief tongues of flame; these latter having no smoke at their tops; and the horizontal section passing through the widest and brightest part of the flame will be from $\frac{6}{8}$ to $\frac{9}{8}$ of an inch above the burner. It is assumed that this section should coincide with the focal plane of the lenticular bands, or merely refracting part of the apparatus; for then the most powerful rays proceed in a horizontal direction, & will fall on the sea horizon, when the light has little or no elevation above the sea. All the light which emanates below that plane & passes through the lenticular bands, will be thrown up into the sky above the horizon; but all that, emanating from the great body of the flame above that plane, will be cast over the sea between the horizon & the shore, doing good service to the mariner.

The selection of this plane, or of the point in the centre of the flame coinciding with it, which is called the focal point in respect of the refractors, is of great importance. By numerous experiments & trials in France, it is considered as 28 mm or 1.05 of an inch above the burner. Now the brightest horizontal section of the flame, may be 1.12 inches above the burner with a well arranged overflowing lamp; or only 0.75 of an inch above it, with a low flame & non overflowing lamp;— & the adjustment in height of the lamp which would suit the first, & send a fine body of light from the upper part of the flame over the sea, would, with the latter, send little to the sea & the greater part of the light to the sky:— so that not merely has the adjustment to be attended to, but also the lamp suited to the adjustment.

As far as my observation goes, the lamp should overflow freely, so that only one fourth of the oil that passes over & through the wicks, should be burnt:— it should, in the case of a first order lamp, have four wicks;— and a chimney of glass & iron 6 feet high;— As much oil as possible should be burnt without smoking; for when in a good state, the light is as the oil burnt. The lights at Whitby had not overflow lamps; and only a certain amount of oil could be burnt & a certain height of flame (lower at the South than at the North lighthouse) be obtained. By making the oil overflow & raising the wick, the flame was raised at the base, but began to smoke;— by virtually lengthening the chimney, through the application of paper valves, the whole flame was raised, both at the base & at the summit, and the combustion very greatly improved, & that continuously. But the adjustment of the optical part, fit for the one state on flame would not be fit for the other. Being examined in the manner proposed by the Commission, if the combustion were low, the ray proceeding from the eye to the horizon would be much too high in the flame; whereas with a full & proper flame, it might pass in the best direction; for

the difference in level of the brightest sections of two such flames may be as much as $\frac{3}{8}$ of an inch;— and every diminution in the good condition of the flame, whether from the construction of the lamp, or inattention, tends to rob the flame at the upper of sea supplying part.

In respect of the reflectors circumstances are different. The whole of the flame radiates light towards each of the *upper reflectors*. If the eye be so placed (as in the R Commission process) as to see the horizon through the flame, in the middle of one of the reflecting prisms, then all the flame above that line, will throw its light into the sky, and only that part below the line will throw its light on to the waters. Hence, the line through the flame, or rather through its projection at the reflector, should go through a bright & abundant part of it, and should also leave as much as possible of the flame below that line; since that is the part which radiates light to the sea i.e. the observers ray should be taken as far back towards him, & as far up, as is consistent with a good line of flame for the horizon; and then the reflector ought to be adjusted, so as to throw this light which has reached it onward in the right direction[.] As regards the upper reflectors, generally, a point in the centre of the flame, 1.55 inches above the burner, is, from careful experiment considered as that which gives the best result; and is called the focal point for the upper reflectors, being common to all.

In respect of the lower reflectors, matters are very different: The burner & cottons cut off much of the light of the flame from them. With the best flame one half of the light is thus lost; & with a low flame, only a fourth or a fifth may pass to them to be utilized. The line of sight should, as regards the observer, be taken as far forward & up, as is consistent with its passing through a bright part of the flame; for here again, it is the part of the flame below this, which sends light to the sea, whilst the part above it casts its rays into the sky. In the French experience and practice, not one focal point, but several foci are taken for the different reflecting prisms. These are points in a vertical line, in the centre of the flame; the lowest focus is for the upper reflector & the highest for the lower reflector; & they are respectively at the following heights above the burner 38, 42, 47, 53, 60 & 68 millimetres[.]

The lamp, optical apparatus, & adjustments, I have referred to, have had reference to a horizontal line; and it is so, that nearly all the apparatus made in, or for England, have as yet been so constructed; but the *sea horizon* does not correspond with a line horizontal at the lighthouse;— it forms an angle with it, and that so much the greater as the light is higher above the level of the sea. At the North Foreland the two make an angle of about 14'.2, and at Whitby of about 16'.5. Hence if the chief ray of light be sent horizontally it will pass over the sea & be wasted, and indeed more light with it, even the 16'.5 seconds, which ought to fall on the sea. This condition is seen at the Whitby lighthouses by the Commissioners mode of examination a little modified. To correct this error for the lenticular bands, it would be sufficient to raise the

lamp an equal number of minutes, as $\frac{1}{6}$ of an inch for Whitby; but such a proceeding would increase the error for the reflectors, both above and below; and can only properly be met by instructions to the maker of the apparatus at first.

The French authorities, only take account of this difference between the Sea & true horizon, when the height of the light is 60 meters (about 200 feet) & upwards, above the Sea. For my own part I do not see why it should not be taken into account for any height of 50 feet and upwards. Twelve or thirteen years ago the Lundy light had its chief rays sent to the Sea horizon, & I made an instrument which was used by Mr. Wilkins for the proper adjustment of the reflectors⁹. Since then the reflectors have been changed for others on the catadioptric principle, & these have their rays directed horizontally as at first, & the same rule has held ever since. Mr. Chance tells me he is *now* constructing apparatus with the rays directed to the sea horizon[.]

Cases may arise where high light, not being a leading light, might better have its chief ray sent, not to the extreme sea horizon, but some intermediate distance, where in hazy weather, the light might require to be in some degree concentrated[.] Such cases are nautical in their nature; but if they occur, instructions should be given to the maker before hand; since the correction or adjustment can not properly be made afterwards[.]

The first application of a new and searching method of examination not applied until after the workmen have been fitting & handling the heavy parts of the apparatus in the lanthorn of the lighthouse, may discover, either derangements of the whole, or of parts. From the expressions of the makers, M. Sautter & Mr. Chance, I understand that these can be rectified in apparatus already erected; and can be prevented in apparatus to be made hereafter:— so that the test proposed makes no undue claim on the manufacturer.

The Royal Commission, when at Whitby, pointed out certain deficiencies in the illumination of the sea; and a waste upon the sky of a portion of light which being indicated by their mode of day examination. The lower reflectors, especially, appeared inoperative, not merely because only a little light could in any case fall upon them, but because if a good lamp had been in the center the adjustment of the reflectors was out[.] A like condition of matters was indicated when at the North Foreland.

Of the two lights at Whitby within 258 yards of each other, the North light lamp is much better than that at the South light; it gives a higher flame and burns more oil, and the appearance at Sea corresponds to this difference. But besides that there are differences in adjustment. An excursion to sea at night on the 9th was made, and at a given signal the whole of the lenticular band of the North light was covered up only the reflectors being left, to compare with the whole of the South light as a standard. In this state the North light was nearly equal to the South light in brilliancy, & in certain positions of the ship was quite equal to it. The light was thus covered up and uncovered again,

twice, and the observations were made at distances of four and seven miles. They showed that the upper reflectors at this house were well adjusted to cast the light upon the sea. Here, therefore at Whitby, it appears to me it would be best to make any proposed changes; for they could be carried out at the South lighthouse by Mr. Chance, the maker of the apparatus, who was present and who understands every point in the matter, and the North light could be left as a standard by which to estimate the improvement gained.

I am persuaded that the condition and character of the lamp has a most important influence over the results that have been & are to be acquired. Some persons call a flame $3\frac{1}{2}$ inches high which I and others would consider as only $2\frac{1}{2}$ or 2 inches in the effectual part. Some persons count from the top of the burner to the top of the tongues of flame; whereas the bright luminous part of the flame often begins $\frac{5}{8}$ of an inch above the burner, and as a body ceases, it may be, an inch or even two inches beneath the top of the longest smokeless tongues. The difference in the consumption of oil at the different lighthouses shows the great diversity existing amongst the flames of different lamps; to which, if one common standard of adjustment in respect of the optic apparatus be applied, it must as often be wrong as right. It appears to me that the first thing is to have an excellent & constant lamp; and that *all lighthouses* of the same order should have a lamp of the same quality;— that the glass chimney employed should have a gradually rounded shoulder & not a sharp square one which greatly disturbs the direction of the light;— that when the best form has been attained it should be adhered to;— & that the glass & iron chimney together should be continuous for 6 feet. Then the practicable constant size and condition of the flame of such a lamp should be determined, and a general gauge & measure of the bright part of it & its position in relation to the burner, be supplied to each lighthouse (which could easily be done by an outline drawing on open wire gauze) that the keeper may report whenever the lamp falls short of its required duty. Having such a lamp, it should be ascertained whether the foci for the adjustment of the optical apparatus at present adopted, are the best for it, as the French Authorities believe, or whether they could be advantageously altered; and then apparatus constructed in future should be made in conformity thereto, and finally tested in their place by the Royal Commission process.

If Mr. Chance were authorized to procure such a lamp for the South Whitby lighthouse (i.e. a lamp which being excellent could be easily & certainly repeated) and were then to adjust the Optic apparatus to it, the result could easily be tested by a comparison with the unaltered North light;— but it would be desirable to be able to ascertain separately, as far as may be, the effect due to the improved lamp, and that due to the re-adjustment of the glass.

With respect to the North Foreland lighthouse, M. Sautter the manufacturer met the Royal Commission & the Trinity Board there, and heard & saw

all that passed. I understood him to admit freely the principle advocated by the R. Commission, but to assert that the apparatus was in perfect adjustment for a *proper lamp*. Setting up the gauges at the burner, according to the focal planes assumed & adopted in France, the apparatus, with the exception of one or two prisms certainly was in beautiful adjustment to them. For my own part, I am of opinion that in relation to the generality of lamps as I have seen them burning, the foci at least of the lenticular part, are taken too high. The chief focus at the North Foreland has been placed 28 millimetres above the burners. As I saw the lamp burning on the Evening of Wednesday the 8th instant and the keeper said it was in its right & usual state, the chief plane of light was not above 22 millimetres above the burner; and if account be taken of the dip of the sea horizon, which is here about 14'.2, the burner ought to be raised on that account 4 millimetres more, making the distance below the focal plane 18 millimetres only instead of 28. If however the lamp were raised enough to correct this difference that would greatly throw out of adjustment both the upper & lower reflectors.

M. Sautter is of opinion that the apparatus requires no adjustment but is correct for a proper overflow lamp. He has full confidence in the French authorized foci. He is however ready to raise the lamp and to readjust the reflecting prisms to any degree the Trinity house may require. Though I think that the foci may perhaps be altered with advantage and intend making an investigation of their places when a good standard lamp is employed if the Trinity board desire it, I am not prepared to go hastily in opposition to the conclusions carefully drawn from theory, experiment, and long practical application by the authorities in France and therefore am of opinion that if MM. Sautter & Wilkins place a proper lamp in the lighthouse and leave it & the optical apparatus in that state of adjustment which the former approves of, and will be responsible for, the determination of any further change there may remain until after the effect has been ascertained of the alterations at Whitby the changes at the North Foreland itself and the re-examination of the places of the foci¹⁰[.]

M. Faraday

1. Robert Gordon.
2. George Bayly (1807–1888, B4). An Elder Brother of Trinity House, 1857–1888.
3. *Parliamentary Papers*, 1861 [2793], XXV, volume 1, pp. 90 gives Charles Weller (d.1866, age 84, *Gent. Mag.*, 1866, 1: 759), an Elder Brother of Trinity House, 1834–1863. Chaplin [1950], 86, 93.
4. James Dombrain (1793–1871, B1). Irish government official and a commissioner for Irish lights.
5. *Parliamentary Papers*, 1861 [2793], XXV, volume 1, pp. 90 at this point also gives Edward Forward Roberts as present. Roberts was a commander in Royal Navy and marine inspector, Dublin, for the Irish Lighthouse Commissioners. O'Byrne (1849), p. 981–2 and *Parliamentary Papers*, 1861 [2793], XXV, volume 2, p. 217.
6. George Halpin. Superintendent of the Irish Lighthouse Commissioners, *Parliamentary Papers*, 1861 [2793], XXV, volume 2, p. 217.

7. *Parliamentary Papers*, 1861 [2793], XXV, volume 1, pp. 90 at this point also gives Edward Parry Nisbet (d.1899, age 89, GRO) as present. He was an Elder Brother of Trinity House, 1857–1899, Chaplin [1950], 205.

8. Letter 3821.

9. Madan to Faraday, 23 August 1842; Faraday to Madan, c.24 August 1842; Madan to Faraday, 1 September 1842; Faraday to Madan, 2 September 1842; Madan to Faraday, 6 September 1842; Faraday to Madan, 13 September 1842; letters 1423, 1424, 1425, 1426, 1427 and 1428, volume 3.

10. This letter was read to Trinity House By Board, 21 August 1860, GL MS 30010/43, pp. 99–100. It was referred to various other Trinity House committees.

Letter 3829

Faraday to James Emerson Tennent

17 August 1860

From the original in the possession of Y. Watanabe

The Green Hampton Court | 17 Aug 1860

My dear Sir Emerson,

Your kindness surprises me – I am deeply thankful for it. I have just written to Mr. Dunbar & shall be in town tomorrow & Monday¹ to do as he may direct – & because you are so interested shall shew my thanks by letting you know the result for I perceive it would be a real pleasure to you to help me & Mr. Deacon[.]

Ever Gratefully Yours | M. Faraday

Sir Emerson Tennent Bart | &c &c &c

1. That is 20 August 1860.

Letter 3830

Faraday to James Timmins Chance

17 August 1860

From the original in RI MS F1 N/4/7

The Green Hampton Court | 17 Aug 1860

My dear Sir,

I have made my report to the Trinity House Yesterday¹.

I have received *no* written account or statement from the R Commissioners or the Astronomer Royal nor do I believe that the Trinity house has. We had no other means of gaining our information than you had with us[.]

I cannot send you my report though I should be very glad that you and all should see it:– it belongs to the Trinity house & therefore I have not the power. I expect they will write to you for I have advised that you should put a new & effectual lamp & readjust the South house if with such a lamp it may seem to you to require adjustment. Carefully leaving the North light alone in all things that it may serve for a standard of comparison. I have suggested that when the South light is rectified both for lamp and adjustment we ought

to have some means of comparing it at Sea with the North light so as to shew the effect of the adjustment with the old lamp & then both of adjustment & new lamp – i.e. to get if possible the effect of the right lamp & the effect of the right adjustment separately. Perhaps this may be a little difficult^[1]

If it was understood that you were to write to the Trinity Board do so – I should be wrong in any way to change or guide your intentions in respect of any understanding between you & the Deputy Master². I have said that you were quite willing to make any change that the Trinity house would require^[2]

Ever My dear Sir | Very Truly Yours | M. Faraday
Jas. Chance Esq | &c &c &c

1. Letter 3828.

2. Robert Gordon.

Letter 3831

Faraday to Peter Henry Berthon

18 August 1860

From the original copy in GL MS 30108/3/90.28

Royal Institution | 18 Aug 1860

Sir,

In reply to your letter of the 8th. Instant¹ respecting the answers given by the Honbl. W.E. Fitzmaurice to enquiries made prior to a Trial of his lime light in one of the Trinity House lighthouses I have to say that the answers are not so direct & practical as those which relate to another lime light by Mr. Prosser² and which I believe the Trinity House have under trial. Nevertheless I do not see any way to a safe & satisfactory decision in the matter than by granting the request of Major Fitzmaurice subject to all the conditions that were made in my letter of the 11 May 1860 respecting the Prosser lime light trial³. The South Foreland upper lighthouse seems to me the best place for the reasons before given. I would submit however to the Deputy Master⁴ and brethren whether it is expedient or desirable that they should put a new lime light on trial before they have obtained all the knowledge they can regarding lime lights in general & the one in particular which is being installed by a full consideration of the results of that lime light after its trial has been completed⁵^[1]

I have the honour to be | Sir | Your Very Obedient Servant |
M. Faraday
P.H. Berthon Esq | &c &c &c

1. Letter 3819.

2. Unidentified.

3. Letter 3776.

4. Robert Gordon.

5. This letter was read to Trinity House By Board, 21 August 1860, GL MS 30010/43, pp. 98–9.

Letter 3832

Faraday to James Timmins Chance

23 August 1860

From the original in RI MS F1 N/4/8

[Royal Institution embossed letterhead] |
Thursday, 23 Aug 1860

My dear Sir,

Let me ask you on one or two points – which I have omitted to note^[1][1] the distance in the fitting room to the first test place? [14 yards]¹ do. to the extreme test place [22 yards (= 66 feet)]

[2] the distance in the yard to the first observing place? [62 yards] to the furthest observing place? [105 yards]

[3] height of the focal plane above the burner? [28]mm

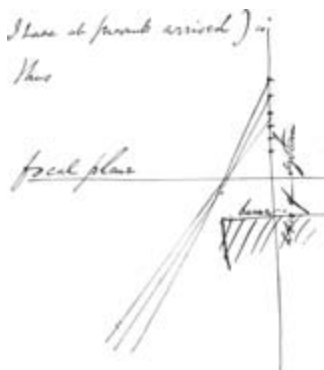
[4] the focal point of the upper reflectors either in relation to the burner or the focal plane? [10 mm above focal plane]

[5] Where do your foci cross in relation to the burner or cotton? [see other page (over)]

[6] Do the final diagram &c in rela[tion] to dip, refraction, &c refer to Whitby or only an imaginary case? [They refer to *Whitby*: viz: *apparent dip* = 17'40" *real* = 15' 14"]

Ever Truly Yours | M. Faraday

Sorry to trouble you but my memory lets things slip. MF

Endorsement: Questn: I presume that this question relates to the *lower* reflectors. My construction of foci (so far as I have at present arrived) is thus

1. Here and below, the text in square brackets was written by Chance and is that given in letter 3835.

Letter 3833**George Herbert to Faraday****23 August 1860****From the original in GL MS 30108/3/90.38**

Trinity House, London E.C | 23 Aug 1860

Sir,

I am directed by the Elder Brethren to transmit to you the enclosed Copy of a Letter from Major the Honorable W.E. FitzMaurice and to state that they would be glad to know if you would recommend the Board to comply with his request to exhibit his Light experimentally at Blackwall.

I am | Sir | Your most humble Servant | George Herbert | Assistant Secy
Professor Faraday | &c &c &c

Letter 3834**Faraday to Andrew Reid¹****24 August 1860****From the original in the possession of Rosalind Brennand**

Royal Institution | 24 Aug 1860

My dear Andrew,

I have yours & the contents – many thanks for your kind regularity[.]

I hope you will have better weather at the seaside than we have here – Rain at least every other day all day long. Very sad for the country.

We are pretty well – considering that yesterday was a fatiguing hard working marrying day²[.] However the Young folks at Kensington seemed very happy & set off I believe for Wales where they will find the water falls in high condition[.]

The News of Miss Hornblower is very cheering[.] She is not yet declared out of danger but we hope she is. Nor is she allowed to sit up yet – nor to see any one but her nieces and only one of them every other day[.]

Please tell Mr. Vincent this[.] Love to all our friends[.]

Ever Truly Yours | M. Faraday

No address to your letter & I cannot remember it.

1. Andrew Reid (1823–1896, Reid, C.L. (1914)). A nephew of Sarah Faraday and a printer in Newcastle.

2. That is the marriage, on 23 August 1860 (GRO), of Mary Anne Barnard (d.1895, age 57, GRO, daughter of Sarah Faraday's brother William Barnard) and Henry Irwin Cummins (d.1885, age 61, AC), rector of St Alban's, Wood Street, 1854–1885.

giving as *foci* – the points 20 – 25 – 30 – 36 – 42 – 49 millimetres above the focal plane, (*neglecting fractions* of a millimetre), in the axis of the system.

In the revolving light this point of intersection was only 14 mm. above the burner, in the same vertical line.

6. The *pencil* diagrams &c referred to the *Whitby* light – viz

Apparent dip (for ray at sea-horizon) 17' 48". Real (corresponding) dip at the Light itself = 15'.14"

I purpose sending you photographs of the flame of the lamp wh: you saw¹.

Ever most truly yours | James T. Chance

1. This is the reply to letter 3832.

Letter 3836

Faraday to James Timmins Chance

25 August 1860

From the original in RI MS F1 N/4/9

[Royal Institution embossed letterhead] | 25 Aug 1860

My dear Sir,

I have just time to acknowledge your letter¹ & thank you. I give you much trouble.

When I came home and compared your real experimental foci with those I had obtained by a first rough tentative proofs (on paper only) I was gratified to find that I was not far from you for I accept your determinations as real practical results. I had made a point 21 mm above the burner and 50 mm from the central vertical towards the side of the lower reflectors a common focus for the illuminating rays going to the middle of the face of each reflector. In like manner I had adopted for a first approximation a point 26 mm above the burner and 31 mm from the vertical axis of the system on the side opposite to the upper reflectors as a common focus for them. This would give on the vertical axis foci varying from 45 to 125 mm – instead of the constant French focus of 38 mm_[.]

I had taken a point higher & nearer to the vertical axis but think that experiment with such a flame as one has a right to expect will give the former numbers or something near to them. I wonder what you will get by experiment_[.] How I should like to come & experiment with you_[.]

With sincere respects to Mrs. Chance – and what I forgot very many thanks for her kind intentions regard the Infant Orphan Asylum & little Leighton²_[.]

Believe me to be | Every Truly Yours | M. Faraday
James Chance Esq | &c &c &c

1. Letter 3835.
2. This referred to Faraday's request for a proxy vote for Minnie Leighton for a place in the Infant Orphan Asylum (see note 1, letter 3793). Elizabeth Chance had been a subscriber since 1857. See the 1861 *Report of the Infant Orphan Asylum* RLSA 90/21/10/18, p. 64.

Letter 3837**Faraday to George Herbert****25 August 1860****From the original copy in GL MS 30108/3/90.39**

Royal Institution | 25 Aug 1860

Sir,

As far as I am concerned it is not necessary for the Honorable Major Fitzmaurice to shew any more experiments or preliminary arrangements prior to the serious and full trial of his method at a lighthouse¹ to which the Trinity letter of the 8th² and my letter of the 18 instant referred³. They cannot in any way supply the place of that proof; or give the information & evidence required; or shew any thing that must not be shewn again at that trial – As far as I can perceive they would only consume time & distract attention. Nevertheless there can be no objection to them but unless there be some *new point* introduced which has not as yet been brought under my consideration I should not think it necessary that I should see them[.]

I am Sir | Your Very Obedient humble Servant | M. Faraday
Geo Herbert Esq | &c &c &c

1. See letter 3833.
2. Letter 3819.
3. Letter 3831.

Letter 3838**James Timmins Chance to Faraday****25 August 1860****From the original in GL MS 30108/5, f. 42–3**

Hamstead Birmingham | 25 Aug 1860

My dear Sir,

I gave you the foci yesterday¹ of the lower prisms of the two lights, as if there were only six prisms.

It was an inadvertence on my part.

There were eight lower prisms in each light.

The positions of the foci were:

Fixed light

20. 23. 26. 30. 34. 38. 43. 48 mm

and for the Revolving light

16. 19. 23. 26. 30. 34. 39. 44 mm

above the focal plane, the “stop” or “guage-tube” having been placed 18 mm above the metallic burner for the fixed Light and 14 mm for the Revolving one.

Yours very truly | J.T. Chance
Professor Faraday | &c | London

1. Letter 3835.

Letter 3839

James Timmins Chance to Faraday

27 August 1860

From the original in GL MS 30108/5, f. 44–5

Hamstead Birmingham | 27 Aug 1860

My dear Sir,

Many thanks for yr note¹.

Your & my sections of the flame for the lower reflectors are wonderfully near; because please to observe that mine (18 mm above the burner for the common intersection) is for the *lower inner angles*, & yours (21 mm) is for the *middle* of the inner faces; this wd make between one & two mm difference. And, also, I was in great doubt between 18 mm & 20 mm for *my* point in the outer flame.

We *really*, therefore, coincide. This relieves my mind greatly, because I was anxious to have your own independent results.

I like very much your suggestions for the upper reflectors: I will try your foci *soon*.

I shall of course repeat experiments for the lower reflectors so as to be quite sure.

I truly hope that the Trinity Board will not be in a hurry in deciding about the lamp for the “Smalls”: that is – not without your concurrence & advice. The *means* of *accurate* & firm adjustment both centrically & horizontally, as well as in regard to the vertical height of the burner, is (as you know) a matter of very great importance.

At Whitby there was no provision for adjustment. At North Foreland, a very unsatisfactory provision.

Most truly yours | James T. Chance
Professor Faraday

1. Letter 3836.

Letter 3840**Faraday to Peter Henry Berthon¹****1 September 1860****From the original copy in GL MS 30108/5, f. 55***Report on Focal points*

Royal Institution | 1 Sep 1860

In reference to the examination proposed at the end of my report of the 16 August 1860² of the Focal points heretofore adopted in France and also in this country I have to state that from considerations founded upon the size of a good lamp flame I came to the conclusion that the focal point for the lenticular or dioptric part was right or very nearly so but that those adopted for the upper & lower reflectors was much too low. Those for the lower reflectors may be represented by a common intersecting point 10 mm above the burner & 48 mm from the axis on the side towards the reflectors. My first judgment was to alter this point by raising it so that it should be 20 mm above the burner & 50 mm from the axis. Mr. James Chance invited me to see two fine first class lights for Russia one of which being a fixed light he had of his own judgment & experience adjusted to foci different to the above. I found the effect of this alteration to be very excellent and I found moreover that his numbers & mine were so near to each other as to render them in effect coincident³[.] I consider the result therefore a full practical confirmation of the numbers & I do not hesitate to recommend this alteration for the adoption of the Trinity house[.]

With regard to the upper reflectors my preliminary judgment was for a far greater change. The French authorities (which we in England have thus far followed) is for a common focus for all the upper reflectors placed in the axis of the flame, & raised 38mm above the burner. I think the common focus ought to be 27 mm above the burner and 32 mm from the axis on the side from the reflectors. I have endeavoured experimentally with a good lamp to establish or correct this estimate and my results gave a point 30 mm above the burner & 32 mm aside from the axis as an excellent place: but I desire to carry out this change upon a whole pannel of upper reflectors before I recommend it finally to the Trinity Board⁴.

M. Faraday

Endorsed by Faraday: (Lamp must have been very good. Wilkins reports that burnt 7 pints? of oil in 4 hours⁵).

1. Recipient identified on the basis of the printed version of this letter in *Parliamentary Papers*, 1861 [2793] XXV, volume 1, pp. 92–3.

2. Letter 3828.

3. See letter 3839.

4. This letter was read to Trinity House Court, 4 September 1860, GL MS 30004/28, p. 110. It was agreed to implement Faraday's recommendations.

5. See letter 3841.

Letter 3841

Faraday to James Timmins Chance

4 September 1860

From the original in RI MS F1 N/4/10

Royal Institution | 4 Sep 1860

My dear Sir,

I do not know how much room I shall want, so I take something larger than a sheet of note paper. Let me acknowledge yours of the 1st¹, and then say, that I was yesterday at the Trinity House, and was authorized fully to write to you in its name²; of which you will hear generally from the Secretary³:- and let me say at once, that in your reference to expence, and even of your own willingness to defray it for the good of the cause, that cannot be thought of for a moment. The Trinity House desire, by me, certain things to be done, both at your works and at Whitby; and of course will pay the expences⁴. I have told the Deputy Master⁵ that you were quite willing to help in every way that you could.

Focal Points. I have reported to the Trinity House my theoretical conclusions regarding a change in these⁶:- and the degree of change. I have also reported what you have done in respect of the *lower reflectors*:- have stated what I saw at your works, and my perfect satisfaction with the results as given by the Russian lights;- and have recommended a point 20 mm above the burner and 50 mm from the axis, towards the reflectors, as the focus hereafter to be used for the adjustment of the *lower reflectors* for a chief horizontal beam. The dip to be taken into account whenever that is necessary.

For the *upper reflectors* I have made a first report (by theory) of a point, 27 mm above the burner and 32 mm from the axis, on the side *opposite* to the reflectors. From certain experiments I made here with a very excellent lamp, I deduced a point, 30 mm above the burner and 32 mm from the axis. Now can you help us (I should not like to go elsewhere), by putting up a lamp and an upper pannel of reflectors, and adjusting the latter to *this* focus; I would then come down and we would see what the result is. The lamp I used was a very excellent one; perhaps too excellent to be taken as a standard; (though I see no reason why not). It burnt between 6 and 7 pints of oil in 4 hours⁷; but it was very steady and without smoke all the time. Still, I should be glad if you could adjust a *second upper pannel* of reflectors, to a focus 30 mm above the burner and only 28 mm from the axis; that we may make our experiments more decisive & instructive.

As to *Whitby*:- leave the North light as it is and deal only with the South light. I mentioned to you to put a new lamp, such as you approve and will be responsible for. It is our opinion that the person who supplies the optical part,

ought to supply and be responsible for the lamp in future. Then readjust the lamp for the dioptric part, having respect to the dip to the horizon;— readjust the lower reflectors from the focal point decided upon to the sea horizon; and readjust the upper reflectors *from the focal point we shall determine upon when you have the experimental pannels ready.*

When however the Whitby light is thus adjusted, we want to make the experiment as instructive as possible. Now the light includes 180° of the horizon; and there are four sets of pannels or frames, each of about 45°. I propose to leave the first upright set of pannels unchanged (except for the dioptric part which will alter with the lamp); to correct the upper and lower reflectors of the second upright set, to the foci we shall determine upon;— to correct the reflectors of the third set to the *other trial foci* we shall adopt;— and to leave the fourth set of reflectors unchanged. Then in sailing round the lighthouse we shall have the successive observations and always the North light to refer to as a standard.

You have the Smalls light in hand. Though the time of the contract is drawing near, yet I am authorized to wish you to delay the adjustment of the parts, until we have determined the new foci. I need not say how glad I shall be to have the two experimental pannels ready for observation soon.

I fancy I shall have to come down to you soon to examine two Red sea lights for the Board of Trade. To give the power of examining them, (and lights generally) as I should like to do in the future;— even a fixed light ought to stand on a *revolving* platform;— for I shall want to observe from the flame place to a distant point which may (upon a scale) represent the horizon. Can this be?

You will probably have heard that the Russian Engineer wrote to me, and that I gave him my opinion on the lights.

I shall be very glad to hear & see your results as to the light thrown up into the air;— and all else (as you well know) that you obtain. The flame of a lamp is favourable as respects the sea light. We shall have more anxiety on that point with the electric light.

I may have to run up & down to & from Spon lane, but I must not be always a trouble to you. I must find some place near at hand, if we are always to examine the apparatus upon the works; and I certainly think that is best.

Ever my dear Sir | Most truly Yours | M. Faraday
James Chance Esq | &c &c &c

1. Not found.

2. On this see Trinity House Court, 4 September 1860, GL MS 30004/28, p. 111.

3. Peter Henry Berthon.

4. On this see Trinity House By Board, 28 August 1860, GL MS 30010/43, pp. 103–4.

5. Robert Gordon.

6. See letter 3840 for the various points in this paragraph.

7. See endorsement to letter 3840.

Letter 3842**James Timmins Chance to Faraday****5 September 1860****From the original in GL MS 30108/5, f. 58–9**

Hamstead Birmingham | 5 Sep 1860

My dear Sir,

I have just received your interesting communication¹.

Everything shall be prepared immediately for trying *two* upper reflecting panels at *Spon Lane*. I want to arrange about a proper screen to receive the light, & other matters; but everything will be ready by Monday next².

Please to say by the return of post when I may expect to see you here.

Could you bring some kind of photometer?

I have ordered one to be made; but it will not be ready for your arrival.

What you call a *trouble* in reference in taking up you abode with me, I consider a great pleasure & honor conferred on myself & my family.

Pray do not go to any hotel, or such like.

I quite understand, & will attend to, the instructions wh: you give concerning the Whitby Lights. The different trials will be most valuable & instructive.

Most truly yours | J.T. Chance

1. Letter 3841.

2. That is 10 September 1860.

Letter 3843**Faraday to James Timmins Chance****7 September 1860****From the original in RI MS F1 N/4/11**

The Court, Hampton Green | Friday, 7 Sep

My dear Sir,

I have only just had yours here¹. I will come down on *Tuesday Morning* the 11th; same hour as before – to the same station, of which I cannot remember the name just now. (Edgbaston?)

I am very happy to think of being with you & very much obliged to you & Mrs. Chance for the kindness. But *think of this*. I do not want any body to interpret the circumstances as an indication of favouritism. I would much rather deny myself a pleasure than give rise to *that*:– and as I may want to spend two nights at Birmingham I thought it would be a good & an early opportunity to cut up that thought either here or in France, if there were an Inn,– at or near Spon Lane works,– I would not mind how small, if clean[.]

Ever Truly Yours | M. Faraday

J. Chance Esq | &c &c &c

Endorsement: 1860

1. Letter 3842.

Letter 3844

Ernst Becker to Faraday

12 September 1860

From the original in IET MS SC 2

Heidelberg (im Waldhorn) | 12 Sep 1860

My dear Mr. Faraday,

Before I left England, you have allowed me to write to you from time to time, and to tell you what I am doing. Six months have passed since that time, so rapidly indeed, that I can hardly believe it. My life has been rather monotonous, & in such cases time seems to fly very fast, and if one stops then occasionally & looks back upon that portion of one's life, old friends, not seen since, recur first to the mind & one cannot conceive that it is really so long since we last met them, or heard of them.

The month of March I spent in visits to relations and friends in different parts of Germany & in the beginning of April, I took my abode at Bonn for 3 months and began the work which I had set me for the next twelvemonth, viz: to make myself acquainted with the present state of physical science in all its branches which includes the recovery of what I knew, but had forgotten,— and the reading up of what has been *added* during the last ten years. By changing my place of residence during that year, I intended to become acquainted with the different professors of physical science at several of our Universities_[.] Accordingly, in the beginning of July I came to Heidelberg, where I am still, & I have had the pleasure of seeing more or less of Plücker & Beer¹ at Bonn, of Kirchhoff², Bunsen and Helmholtz³ here, & last week I went to Carlsruhe to the Congress of Chemists where I expected to find & did find many old acquaintances from Germany & England. However, my principal work is done in my own room & consists in nothing but reading – and rather hard and dry work it is. Not that after two years' abstinence I did not feel the pleasure of being able to devote all my time to science again; but in experimental science, to become acquainted with new facts or to form a clear notion of phenomena, without either making an experiment, or seeing it made,— is difficult & appears dry, if compared with what it would be, if I had the assistance of experiment and observation. I feel, that without this, I shall require much longer time before I find myself sufficiently prepared, & yet, that my notions will not be so clear. To procure all the apparatus and instruments necessary for an experimental study of physical science, would be equivalent to the purchase of a whole physical laboratory, which exceeds the means of any private individual; on the other hand the existing physical laboratories of our Universities are not accessible to me, before I have attached myself to one of them as a

lecturer in physics (and then only by permission of the Professor) and yet this is the very position for which my present studies are to form the *preparation*.— When at Giessen, the other day, on a visit to my old friend and master Buff⁴, I mentioned to him the difficulties I laboured under, & he expressed his opinion, that my present mode of study was not the right one, that I ought not to go on *reading* up the *whole* of the science, but begin at once a special experimental investigation of my own, purchase the necessary apparatus, which need not be very expensive, & that I would find that I profit a great deal more in that way, not only in that branch to which such investigation belongs, but also in other branches of physics.— This is the precise course which, according to my idea, I would have followed *after* the intended *general* preparation. Buff thinks, that the latter is useless, that the first will to a great extent fulfil the same purpose better, & that, if there are other branches in which I should feel the necessity of reading, that might be done bye and bye, while the principal work ought to consist in an experimental investigation, and not in reading.

I have since considered the subject on all sides & I think, I shall follow Buff's advice; that the course he proposes is an infinitely more interesting & agreeable one, but his consideration *alone* would not have induced me to follow it.

I had intended to spend several months next winter at Berlin; but under the circumstances, I just mentioned, when I am obliged to purchase apparatus &c, I shall not select so expensive a place as Berlin for my residence, but rather choose one of our small Universities, perhaps Giessen itself, where I have old friends, and particularly the kind advice of Buff, while anywhere else my acquaintance with the professors is new & I cannot expect their taking any interest in me.— It remains yet for me to choose a subject for the special investigation I am about to begin.—

Now I have given you a short sketch of my life & of what has occupied my mind. I may add, that since I came to Heidelberg, I have taken my mother⁵ to me, & so given her a new home, while I have formed one for myself; should I go to Giessen in November, she would come with me again.

At Carlsruhe, I heard from Gladstone that you are well and vigorous and that you often meet on the lighthouse Committee. I hope you have not entirely discontinued the subject of the connection of gravitation with the other forces. Dear Mrs. Faraday, I am afraid, will not have much enjoyed the retreat at Hampton Court, bad as the weather has been; I trust, however, that she is well. With my kindest regards to her & Miss Barnard

I remain | Yours very truly | E. Becker

1. August Beer (1825–1863, NDB). Professor of Mathematics at the University of Bonn.

2. Gustav Robert Kirchhoff (1824–1887, DSB). Professor of Physics at Heidelberg, 1854–1875.

3. Hermann von Helmholtz (1821–1894, DSB). Professor of Physiology at Heidelberg, 1858–1871.

4. Heinrich Buff (1805–1878, NDB). Professor of Physics at Giessen from 1838.
 5. Johanetta Chistine Becker, née Weber (1789–1878, Pangels (1996), 42).

Letter 3845

Faraday to James Timmins Chance

14 September 1860

From the original in RI MS F1 N/4/12

Royal Institution | 14 Sep 1860

My dear Sir,

I caught the Deputy Master¹ to day and advanced matters[.]

You will receive a letter at once authorizing you. &c &c

The Trinity house (by desire of the Board of Trade) ask me for a general estimate of expence of the experiments I am making;— and so I have to ask you:— as it must be charged separately from the service of the lighthouse. I think all I did with you & all we shall have to do at Whitby in the first instance may be considered as experiment:— except perhaps the lamp; which if it remains permanently, will be charged to the lighthouse. — Can you send me an idea of a general sum; about &c[.]

In the mean time you can go on with the alterations at the Whitby South lighthouse:— but the Deputy Master will not be ready to visit it with the Commission before Wednesday the 3rd of October, — because of the Queens visit to the continent² &c. I shall want *to see it a couple of days before hand*[.]

The *North light* to remain untouched either as to lamp or adjustment[.]

The *South light*

To have a good lamp — raised so that a point in the axis 28 mm above the burner shall be in a line passing through the center of the lenticular band to the sea horizon — the chimney of the lamp to be six feet high from the bottom of the glass[.]

To adjust the lower reflectors of all the octants alike to the sea horizon;— namely by a focus 20 mm above the burner & 50 mm from the axis towards the reflectors[.]

To adjust the four octants of upper reflectors also to the sea horizon but from different foci — there being for the most Northern octant 20 mm above the burner[.]

and — 30— from the axis

for the next toward the South, 28 mm up

and 30— aside from axis

for the third — 30³ mm up in the axis

for the most southern octant

28 mm up

and 40— aside from the axis

We *must* have the French Focus in so that I have left
 the 20 mm up and
 40– aside – out
 I think I have mentioned all the points.
 Ever your truly Obligated | M. Faraday
 Jas. Chance Esq | &c &c &c

1. Robert Gordon.
2. The Queen left for a visit to Coburg on 22 September 1860, returning on 17 October 1860. Weintraub (1987), 280–3.
3. Written in pencil above this number is '38'. See letter 3848.

Letter 3846

Faraday report to Trinity House

14 September 1860

From the original copy in GL MS 30108/5, f. 64–5

Report on experiments at Messrs. Chance's works and at Whitby in relation to focal points.

Royal Institution | 14 Sep 1860

I have been to Birmingham and worked for two days, with Mr. James Chance, upon the determination of the best *focal points* for the upper reflecting prisms. I still see reason to expect much improvement by the change referred to in my report of the 1st instant¹, and now propose to carry out that change, experimentally, at Whitby.

For this purpose, I propose (as before), that the North Whitby light be left untouched, for a standard. That Mr. Chance provide for the South light a good overflow lamp, with a continuous chimney 6 feet in length from the bottom of the glass, and adjust it so, that a ray from the sea horizon passing through the middle of the Lenticular zone, shall intersect the axis of the lamp at a point 28 mm above the burner. That the lower reflecting prisms of all the Octants *save the third* counting from the north, be adjusted to the sea horizon, from a focus 20 mm above the burner, and 50 mm from the lamp axis towards the reflectors. That the upper reflecting prisms (save those of octant No. 3) be adjusted to the sea horizon, but from different foci:– the north pannel or No. 1 to a focus 20 mm above the burner and 30 mm from the lamp axis on the side from the reflecting prisms:– pannel No. 2 by a focus 28 mm above the burner and 30 mm aside:– the South pannel or No. 4 by a focus 28 mm above and 40 mm aside. The Octant No. 3 to have both the upper & lower prisms adjusted by the French foci and not to the Sea horizon but to the true horizon.

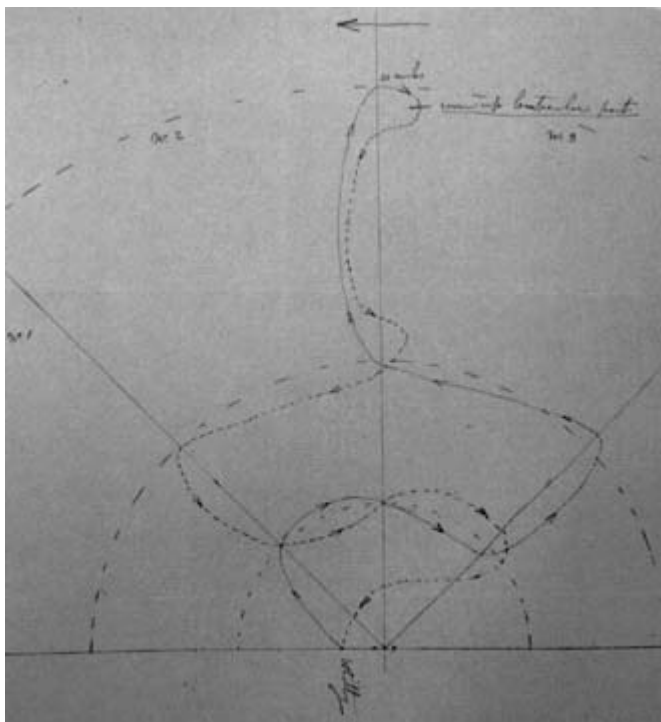
I then propose to go to sea at night time, and examine the effect of these different adjustments at distances up to 18 or 20 miles; moving on some such course as that, a plan of which, is herewith sent (marked P), that the effect of

each adjustment may be observed and compared with the North Light:— and, I propose that, at the extreme distance, the lenticular band shall be screened off, so that in returning, the effect of the reflecting prisms only shall be seen & compared; the Northern light still remaining unchanged.

This sea trip will probably decide the best arrangements:— but if cause appears for trial of any other foci, — or any other arrangement, such arrangements can be made in a day or two, and a second night-examination at sea be made.

The expence incurred by the experiments at Birmingham, by those now recommended at Whitby, and for apparatus I have had occasion to order, will probably not exceed 300 pounds²[.]

M. Faraday



1. Letter 3840.

2. This letter was read to Trinity House By Board, 18 September 1860, GL MS 30010/43, pp. 123–4. It was agreed to carry out the experiments.

Letter 3847**Alfred Phillipps Ryder to Faraday****14 September 1860****From the original in GL MS 30108/5, f. 68***Private*

Royal Commission, Lights, Buoys, and Beacons,
7, Millbank Street, S.W., London | 14 Sep 1860

Dear Sir,

I return the enclosed with thanks – I have sent a copy to Adl. Hamilton that he may see how complete a course of experiments you propose to make. My impression as I stated to Mr. Chance at Whitby & since is that the comparisons would be most effective & decisive if the Yacht was stopped anchored if possible about 5 miles off the light a distance which we reached when we were there – And that the ill: app be *turned* by preconcerted signal.

If you steam about to get views of the diff. phases you probably diminish or increase [the] distance which would affect the accuracy of the comparison – and as at that distance 5m (– a distance to which we should attain for accurate inferences) you would have to steam a considerable distance, from the full action of one set of prisms to that of the other, including the distance necessary for turning such a time would elapse between 2 observations, that the last impression would have faded from the retina – I am aware of the great value of the standard, No. Light but still after a few minutes have elapsed, I think a diff of opinion might arise as to whether the prisms looked at were more or less bright than those that preceded them^[1]. If you agree with me I feel certain that the plans I suggest could be adopted – and the comparison made rapidly – that no risk should be incurred as respects the passing mariner.

Yours truly | Alfred Ryder

Letter 3848**Faraday to James Timmins Chance****15 September 1860****From the original in RI MS F1 N/4/13**

R.I | 15 Sep 1860

My dear Sir,

I think I have written 28 mm instead of 38 mm for the height of the French focus for the *third* octant at Whitby¹_[1]. If so I have no doubt you will correct it.

Ever Truly Yours | M. Faraday
Jas Chance Esq | &c &c &c

1. In letter 3845.

Letter 3849**George Herbert to Faraday****15 September 1860****From the original in GL MS 30108/5, f. 71**

Trinity House, London, EC | 15 Sep 1860

Sir,

I beg to transmit herewith, for your information, Copy of a Letter addressed to Messrs. Chance Brothers, in relation to Experiments at Whitby¹, and I am desired by the Elder Brethren to request you will furnish me with an approximate estimate of the probable expenses that may be incurred by you in carrying out your investigations as to the most advantageous positions of the Foci of the Dioptric Apparatus.

I am | Sir | Your most humble Servant | George Herbert | Assistant Secretary
Professor Faraday | &c &c &c

1. Herbert to Chance Bros, 15 September 1860, GL MS 30108/5, f. 70.

Letter 3850**Faraday to James Timmins Chance****17 September 1860****From the original in RI MS F1 N/4/14**

RI | 17 Sep 1860

My dear Sir,

Yet another change. I think we ought to have *one Octant* adjusted as regards the reflectors altogether by the French regulation i.e. Octant No. 3. Notwithstanding the elevation of the lamp, the true comparative effect of the different adjustment will be seen when the lenticular zone is veiled. I send you a copy of a paper¹ which I do not call a report because I have no right to send you reports. You will see the part marked in the Margin & the general interest of the whole. Please let me have it again[.]

Ever Truly Yours | M. Faraday
Jas Chance Esq | &c &c &c

1. Presumably letter 3846.

Letter 3851**Faraday to George Herbert****17 September 1860****From the original copy in GL MS 30108/5, f. 62–3**

The Green, Hampton Court | 17 Sep 1860

My dear Sir,

I could have sent you the accompanying report¹ on Friday² but have waited until now for the estimate of probable expence referred to at the end.

I have not yet received it from Mr. Chance so send the report as it is thinking you may want it tomorrow³ & will send the sum when I know it[.]

The use of the North light in the experiments is to have a reference to the former state of the S light there I think it ought not to be altered. You will see by the report I now send that one of the Octants the 3rd is adjusted to the French focal point so that there will be a full comparison between it & the other arrangements with the lenticular zone both on & off.

Ever Truly Yours | M. Faraday
Geo Herbert Esq | &c &c &c

1. Letter 3846.
2. That is 14 September 1860.
3. See note 2, letter 3846.

Letter 3852

Faraday to Gottfried Wilhelm Osann¹

17 September 1860

From the original in Deutsches Postmuseum

[Royal Institution embossed letterhead] | 17 Sep 1860

Very Very sorry – I cannot find the papers.

Very Truly Yours | M. Faraday
Professor Osann | &c &c &c

Address: Professor Osann | &c &c &c | Hotel Provence | Leicester Square

1. Gottfried Wilhelm Osann (1797–1866, P2, 3). Professor of Physics and Chemistry at Würzburg.

Letter 3853

James Timmins Chance to Faraday

18 September 1860

From the original in GL MS 30108/5, f. 72

Glass Works, Birmingham | 18 Sep 1860

My dear Sir,

I quite understand all your wishes¹, wh: shall be attended to.

I do not regret omitting the focus (upper reflectors) 40 aside & 20 up. I have tried well the 40 aside & 28 up, & I like it very much for availing ourselves of the light on the sea.

In order to compare the 3 octants (Nos: 1. 2. 4) of new foci, with the *French* foci in the remaining Octant (No. 3), I should have thought it better to adjust the French panels No.3 to the *sea*-horizon, and not to the true horizon as you recommend.

There is no doubt about the question of dip – and by leaving out the dip in the French octant, I fear that an exact idea of the French foci, *as compared* with the new ones, will not be formed.

This is the only point on wh: I should be inclined at all to differ from you. Be kind enough to reconsider thus. But no doubt you have good reasons for your arrangement_[.]

I leave on Thursday² for Whitby. If you write by tomorrow (Wednesday's) post, please to address me, on the envelope, *Hamstead* Birmingham: if you write after tomorrow, please to address me at the lighthouses – Whitby; or the Royal Hotel:– if you like.

Most truly yours | J.T. Chance

1. See letter 3850.

2. That is 20 September 1860.

Letter 3854

Faraday to James Timmins Chance

21 September 1860

From the original in RI MS F1 N/4/15

Hampton Court Green¹ | 21 Sep 1860

My dear Sir,

I have only just had your letter here². So write to the Royal Hotel, Whitby.

You just touch the thing that completely puzzles me namely the French arrangement_[.] I am most anxious not to meddle with the North light at all but keep that as a record of the past state. Then I think we cannot spare two octants for the French adjustments. Yet as M. Sautre [sic] & the French authorities say that such a dip as the North Foreland – which is near 200 feet is unimportant, I should be sorry not to include *proof* of the effect:– though having that as the French have it we do not correctly compare the pure effect of different foci.

I cannot but think that the Trinity house *will expect to see one of the octants as M. Sautre would have it and have the power of comparing it with an octant as we should have it* – the lamp being the same & to this purpose I had directed Octants 2 & 3. Of the other two octants 1 & 4 I should be sorry to lose the instruction we hope to derive from them. No. 1. I expect will throw the light more to the horizon. No. 4 more below it – and even more below it than No. 2. Yet you speak well of it as if you wished for it. If we throw either out in order to introduce the *French foci subordinate to the Sea horizon*, I should wish it to be Octant 4. (focus 28 above & 40 aside). What do you think of that.

I asked you for a round sum as estimate of expences³_[.] Has it escaped your memory or have you sent it in to the Trinity House? I have to add a few pounds to it before it goes in_[.]

Ever Your | M. Faraday

J.T. Chance Esq | &c &c &c

Could you get the correct bearings by the Compass of the division lines between the Octants. I suppose the agent will be kind enough to give them to you?

Yours | MF

1. 'Royal Institution' crossed through beneath this.
2. Letter 3853.
3. In letter 3845.

Letter 3855

Faraday to James Timmins Chance

25 September 1860

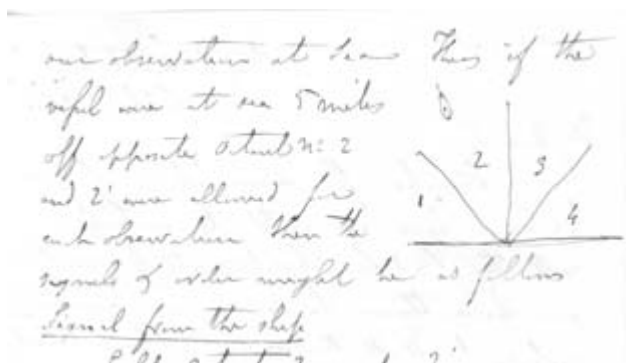
From the original in RI MS F1 N/4/16

Royal Institution | 25 Sep 1860

My dear Sir,

Thanks for your letter of yesterday¹. I have no right to interfere with the Red Sea lights at present and leave them to you. I gave notice to MM. Parkes and Wilkins that the lamps must be with them.

When at Whitby M Massilin said it would be easy to turn the illuminating apparatus but the nuts broke & there was not time^[.] Now can this be done and that safely so as to allow of the partial revolution of the illuminating apparatus so as to bring any of the Octants into a given direction and that without disturbance of the adjustment in relation to the lamp & the sea horizon for as Captn. Ryder suggest[ion]² that would observation at Sea. Thus if the vessel were at sea 5 miles off opposite Octant No. 2 and 2' were allowed for each observation then the signals & order might be as follows follows



Signal from the ship

held Octant 2 on for 2'

bring — 1 — 2'

bring — 2 on for 2'

bring — 3 — 2'

bring — 4 — 2'

bring — 3 — 2'

bring — 2 in & then stop until the next signal from the ship_[.] This would include only 10' for the actual moving time and the longest interval during which the mariner would be deprived of the South light would be only 6' for No.4.— Then the ship might sail off[f] to 10 miles and repeat signal & miles and repeat. Then we would if practicable shade off the lenticular part – & observe at three distances back again with the reflectors only. only_[.]

I have been to the Trinity House today & ascertained that this removal of the light for 6' would not produce any harm & might be allowed. If you therefore can make the apparatus rotate (by hand of course) without introducing harm or error do so and let know when done_[.]

Ever My dear Sir | Truly Yours | M. Faraday
J.T. Chance Esq | &c &c &c

1. Not found.
2. In letter 3847.

Letter 3856

Faraday to William Matheson Deacon

25 September 1860

From the original in RI MS F1 N/5/5

H. Court | 25 Sep 1860

My dear friend,

The enclosed will explain itself. Captn. Owen¹ wishes he could be useful to you and proposed that you should call on him at the Trinity House either next Thursday – or the Monday or Tuesday² following at any hour between 10 and 2 ocl_[.]

Ever affectionately Yours | M. Faraday
Captn. Deacon

1. John Fulford Owen (d.1872, age 68, GRO). An Elder Brother of Trinity House, 1850–1872, Chaplin [1950], 205.
2. That is 27 September, 1, 2 October 1860.

Letter 3857

Peter Henry Berthon to Faraday

27 September 1860

From the original in GL MS 30108/3/90.43

Trinity House | 27 Sep 1860

My dear Sir,

The Hon Major FitzMaurice having, in reply to an enquiry put to him as the Experiments he is desirous of exhibiting to the Brethren of his Light, replied to the enclosed Effect, – I have been instructed by the Deputy Master¹ to forward you a Copy of his Reply, (R)² and to request that you will favour

him (the Dy Ms) with your opinion whether, as you have already stated that you do not see that any beneficial result will be obtained by inspecting the Light at the Buoy Wharf, you consider that the Major may, with any probable advantage, be allowed to exhibit it at some Light House such as the Orfordness – in fact whether, from the information he has as yet supplied, in reply to your string of queries³, you are of opinion that its applicability to Light House purposes has been so sufficiently established as to warrant its being *experimentally* introduced in a Light House.

Believe me to be | My dear Sir | Yours very truly | P.H. Berthon
M. Faraday Esq | &c &c &c

1. Robert Gordon.
2. This is in GL MS 30108/3/90.44.
3. See notes 2, 3 and 4, letter 3859.

Letter 3858

Faraday to Henry Stevens¹

29 September 1860

From the original in UCLA UL Henry Stevens Collection #801, Box 39, folder 6

[Royal Institution embossed letterhead] | 29 Sep 1860

Sir,

I received some time ago the enclosed letter dated Nov 23 1859 referring to a package No. 710a[.]

I have just received a package from you which has no number on it or has lost its number but which *I suppose to be No. 710* since it contains the Smithsonian Report for 1858 & also the Coast survey for 1857 and other papers[.]

I beg with many thanks to acknowledge the receipt of these papers & have the honor to be

Sir | Your Very faithful Servant | M. Faraday
Henry Stevens Esqr | &c &c &c

1. Henry Stevens (1819–1886, B3, DAB). American-born bookdealer in London from 1845.

Letter 3859

Faraday to Peter Henry Berthon

29 September 1860

From the original copy in GL MS 30108/3/90.42

Royal Institution | 29 Sep 1860

My dear Sir,

I do not see what is to be gained by Major Fitzmaurice entering into the same “experiments that he originally shewed to the late Chairman of

the Trinity Board”¹[.] I think that the subject can be advanced now only by its full trial in a lighthouse as I have said before. I cannot say that I think the applicability to light house purposes has been established as yet – the trial in the lighthouse is to establish or disprove & I think that the latter is quite as probable as the former. The data as regards expence are to me very unsatisfactory for though the proposition to apply this lime light to lighthouses is above two years old yet no estimate of the outlay & the current expence of lighting for a given period as 6 or 12 months has been supplied to be used as a test (when the trial is made) of the preconsidered & trust-worthy condition of the plan. The amount of liability to derangement is also very uncertain. These remarks apply to one lime light as much as another[.] Speaking in the interest of the Trinity house I cannot see any advantage in trying two lime lights at once when one will probable supply all the information & proof that is required. I think the South Foreland is an admirable place for the experimental trial because of the extreme observation that can be brought to bear upon it & because of the presence of the low light as a continual standard[.]

Let me again refer you to my letters of the 20 August 1858² – 20 August 1859³ – 18 August 1860⁴ and 25 August 1860⁵[.] I can only repeat what I have on different occasions there said⁶[.]

I am My dear Sir | Very Sincerely Your Obligated Servant | M. Faraday
P.H. Berthon | &c &c &c

1. See letter 3857.

2. Letter 3505.

3. Letter 3633.

4. Letter 3831.

5. Letter 3837.

6. This letter was read to Trinity House Court, 2 October 1860, GL MS 30004/28, p. 120–1. It was agreed to inform Fitzmaurice that he could carry out trials of his light at the experimental light station at Blackwall at his own cost.

Letter 3860

Faraday to James Timmins Chance

1 October 1860

From the original in RI MS F1 N/4/17

[Royal Institution embossed letterhead], Albemarle St |
1 Oct 1860

My dear Sir,

Where you are I don't know or whether you had my letter¹ about rotating the S. light apparatus: but I dare say you have received it. The piece of reflecting prism which you sent me was left at the Whitby light but can you let me have *on the part of the Trinity house two such pieces corresponding to near the top & the bottom of the upper set of reflectors*² that I may practically consider

the best method of observing a distant object in them? Shall we be able to rotate the S Light?

Ever Yours Truly | M. Faraday
J.T. Chance Esq | &c &c &c

1. Letter 3855.
2. From 'two' to here this passage is underlined in another hand.

Letter 3861

Faraday to James Timmins Chance

2 October 1860

From the original in RI MS F1 N/4/18

The Green, Hampton Court | Tuesday, 2 Oct 1860,
11 o'clk. A.M.

My dear Sir,

Our posting has been sadly embarrassed. I have only just received yours of the 29th Sept¹.

The Deputy Master² has I think appointed the 11th for the reunion at Whitby & has told Capt. Ryder so as his letter tells me. The 4th would not do because of the moon. I will write to the Trinity house immediately to clear up the point & will say that I have that I have said the 11th to you_[.]

I intend to do as you say come to York on Monday the 8th & to Whitby the next morning.

I hope you will get this in time to free you for Birmingham_[.]

As the Trinity house will *as I suppose* not be at Whitby before the Thursday the 11th do not mind me at Whitby. If I find somebody who can turn the apparatus for me that will be all I shall need & that will do on the Thursday Morning_[.]

Ever My dear Sir | Yours | M. Faraday
J.T. Chance Esq | &c &c &c

1. Not found.
2. Robert Gordon.

Letter 3862

Faraday to Benjamin Vincent

12 October 1860

From the original in WIHM MS FALF

Royal Hotel, Whitby | Friday, 12 Oct 1860

My dear friend,

I fear we are tied up here for some days longer & cannot stir. We are hard at work in the lighthouse and have to wait for the wind – Perhaps we

may be able to go out tonight – perhaps tomorrow night – and perhaps not_[.] If I do not see you on Sabbath day¹ you will know the cause. Both bodies i.e. the Trinity house & the Royal Commission are here & I cannot leave them and though they respect the Sabbath we might have to work up *to* the Sabbath or within an hour of it so I must just wait & trust_[.]

Could you give the enclosed to Mr. Anderson & could you also advance him 4 or 5 pounds until I come back? It will help me & him greatly_[.]

As to the great matter I say little of it here²_[.] It is constantly in my thoughts but I cannot write much about. Nor is that needed – it is in better hands than mine and in his working & guidance in whom I hope to trust for surely he made us and not we ourselves and he guides his own as a shepherd his sheep_[.]

With deep love I am | Very dear brother | Yours | M. Faraday
Mr. Vincent

1. That is 14 October 1860.

2. Presumably a reference to Faraday becoming, on 21 October 1860, for the second time, an Elder of the London Sandemanian Church. See Cantor (1991), 60.

Letter 3863

Faraday to Charles-Louis Barreswil¹

18 October 1860

From the original in Bibliothèque de Versailles

Royal Institution, London | 18 Oct 1860

My dear Sir,

I search for some better means than mere words, to thank you for your constant kindness in keeping me in continual remembrance;– in sending me time after time your *Repertoire de Chimie*². I would I had some scientific fruit of my own growth to send you in return. I even wait in hopes. But the thoughts of three score & ten years will not fructify with me, as those of past times occasionally have done; and so I have to pray you to accept thanks:– bare but earnest thanks – And though I cannot work, I rejoice to look on and see, how, under the energetic exertions of you and such as you, the great cause of Scientific development is advancing_[.]

Believe me to be My dear Sir | Your Very Grateful Servant | M. Faraday
Chas. Barreswil Esqr | &c &c &c

1. Charles-Louis Barreswil (1817–1870, DSB). French chemist.

2. This was a journal of the *Société Chimique de France*.

Letter 3864**Faraday to Robert Gordon****18 October 1860****From the original copy in GL MS 30108/5, f. 90-1**

The Green, Hampton Court | 18 Oct 1860

My dear Admiral,

In reference to your enquiry at our parting at Whitby I may now say that the experiments in London Birmingham & Whitby which you allowed me to enter upon & carry out have been very successful and useful. They have enabled me to decide upon a departure from the focal regulations of the French authorities: and the comparison of the French regulations with three others has been practically carried out. You, with the Trinity brethren & members of the Royal Commission have seen the result both at Sea & on land;— have approved I believe unanimously of the change proposed and you have given orders accordingly.

The change consists in the first place in directing the chief & guiding ray to the sea horizon & not to the horizontal horizon or dead level and in the second place in altering the focus for the upper reflectors from a point 38 mm up in the axis of the flame to another 28 mm up and 30 on one side;— and that for the lower reflectors from a point 9 mm up and 50 aside to another spot 25 mm up & 40 aside; the latter change being finely determined by experiments on Monday last¹. The result is that much of the light which would be directed by the French adjustments up into the sky by the new arrangement brought down & spread over the sea between the horizon & shore;— without any sacrifice of light at the sea horizon itself. Proof was obtained at the same time of the kind of lamp best suited in the service and the watchfulness required upon the part of the keepers respecting its action & proper condition[.]

The experiments have led to a refined mode & nicety of adjustment which can be practised by the manufacture with facility; nor need the Trinity house hesitate to demand a degree of accuracy which could not heretofore be called for. They have also shewn the value of the final examination of the optical apparatus within the lighthouse as recommended by the R Commission and under the exceedingly frequent obscurity of the Sea horizon how best to carry it into effect. New instruments as well as a new process have been invented and I cannot but express most earnestly my thanks for the manner in which Mr. Chance has enabled me by his own personal thought & exertions to pursue these experiments both at Birmingham & at Whitby to the best practical results.

I have the honor to be | My dear Admiral | Your faithful Servant |
M. Faraday
Admiral Gordon | &c &c &c &c

1. That is 15 October 1860.

Letter 3865**Faraday report to Trinity House****19 October 1860****From the original copy and printed copy in GL MS 30108/5, f. 92–4 and 96***Report on Experiments at Whitby in relation to the focal points of lighthouse apparatus*

Royal Institution | 19 Oct 1860

The experiments referred to in the Report of the 14th September¹ have now by the authority of the Trinity House been carried out not only in London & Birmingham but also at Whitby; and the Deputy Master² with certain of the Brethren have there entered into an examination of the results.

The south light at Whitby was inferior on the former visit to the North light, though both lamps were of the same construction. That in the North house was left unchanged to serve as a standard, that in the South house was changed for one with four wicks & a plentiful over flow; and the light it now gives is not merely considerably more than before, but more than that of the North Light. It has burnt well from the first. The average proportion of oil consumed in a fortnight is 15 pints for the South Light and 13 pints for the North Light per 12 hours: these quantities accord with the proportion of light which they really give.

In reference to the Focal changes and adjustments the following arrangements were made. There were four equal Octants, or eights, of glass apparatus, with a central lamp to the whole. Each had its three parts: the *lenticular band*, the *upper*, and the *lower* reflectors. The lenticular part is the most powerful:— the upper reflectors come next; and the lower reflectors are last. The adjustment is made, first by the elevation of the lamp for the lenticular band and after that by the position of the prisms for the reflector part. The lenticular part is (in the Whitby case) adjusted for all the Octants at once. The French focus for this part has been adopted, namely, 28 mm above the burner; but the French authorities send the chief ray on a horizontal level for a light of such a height as Whitby; whereas, I have sent it down to the sea horizon. No. III Octant was selected to represent the French adjustment; but it was impossible, whilst regulating the lenses of the other Octants, to separate this from them; so that it had an advantage in the comparison, as respects this lens part:— As to the reflectors, however, the focus for the upper set is 38 mm above the burner in the axis of the flame; and 9 mm up and 50 aside for the lower set:— these were adjusted accurately in the lighthouse to the *true horizon or dead level*.

The other Octants had each a pair of common foci for the reflectors No. I focus for the upper reflectors and was 20 mm up & 30 aside – for lower reflectors 20 up – 50 aside

II — 28– up 30 — 20 – 50 – III (was the French) — 38– up 0 — 9 – 50 – IV — 28– up 40 — 9 – 50 – the octants I, II, and IV had their adjustments made to the *Sea horizon*.

The intention of all these arrangements was to discover & establish (what was expected on principle) that much light hitherto thrown up into the sky, might be disposed of on the sea between the distance & inshore, without any diminution of the light on the sea horizon;— and, that if particular regions in certain directions short of the horizon required an especial amount of light, which foci were proper for such a purpose.

The night sea voyage was so arranged that the various Octants (with all the light on) could be observed at different distances as the ship went out; the effect of the change from the one to the other being remarked; and at the same time a continual comparison with the unchanging North Light be made. On returning towards land the lens part of all the octants was blinded, so that a like comparison of the reflectors without the lenses could be made over the same series of distances[.]

When in shore i.e. about a mile or $1\frac{1}{2}$ miles off the Octants I, II, & IV surpassed the French Octant as was expected;— the same effect continued when further out but diminished in proportion. At 10, 12 & 14 miles out the difference between the Octants diminished; but the French Octant never surpassed No. II or No. I and was as some thought rather beneath them;— the constant N light was most important in helping to settle these differences. When the lenticular bands were screened off the reflector bands alone gave bright light but there were differences of the same kind though greater in proportion.

Every one present made their observations for themselves but I believe with a like result. I had expected No. II to be the best if not *the* best arrangement and I am happy to believe the Deputy Master & brethren came to that conclusion since they directed that it should be the type & pattern of adjustment for all the octants of both the Whitby Lighthouses[.]

Like observations to those just described had been made on shore and with the like result; but we had not then the same advantage of observing at very long distances, nor that of comparing with the standard North light.

On the Monday³ we wrought at the Lighthouse for the purpose of verifying or correcting the focus for the lower reflectors. Mr. Chance, in making the adjustment, found that numbers varying somewhat from those I had given, and even more from the French numbers, caused the rays to be more parallel; and as they were to supply the sea horizon such parallelism would be an advantage[.] The numbers were 25 mm up and 40 aside. These numbers were used on the night of the sea observations. By trial I became satisfied of the reality of the advantage, and recommend these numbers to be adopted for the lower reflectors[.]

All the time we were at Whitby (8 or 9 days) Mr. Chance & myself were occupied in learning, practising new methods of adjustment & correction, and using new instruments;— and I cannot say too much in thanking Mr. Chance for the earnest & intelligent manner in which he has considered & worked every

Letter 3866**Faraday and Sarah Faraday to Mary Deacon****20 October 1860****From the original in RI MS F1 L/2/1**

[Royal Institution embossed letterhead] | 20 Oct 1860

Dear Mary,

Put the enclosed into any shape that will remind you of our love. It cannot measure it, or even indicate, except to a loving mind, in that respect like the Givers; and then any thing may serve for all things are exalted by such a feeling. We hope & trust there is great happiness in store for you & William¹; and though the course of your life seems as if in some respects it might be different to ours, I believe it will be the same in the matter of affection.— With warmest love to you both We are dear Mary

Your Affectionate Uncle & Aunt | Sarah & M. Faraday

1. They had married on 4 October 1860. GRO.

Letter 3867**Thomas Stevenson to Faraday****22 October 1860****From the original in IET MS SC 2***Private*

Edinburgh | 22 Oct 1860

My Dear Sir,

When at Aberdeen the other day with the Astronomer Royal¹ I recollected of my unfulfilled promise to let you know our average consumption of oil in the Northern Lighthouses. This has been found to be five (5) hours to one gallon, and whenever they go below that, the keepers are pulled up. In contrasting the whole expenditure of oil at the Northern Lights as compared with the other Boards, this result may not perhaps appear as the Scotch Lights are not lighted from sunset till sunrise. They light and extinguish by a Table in which 40 mins. are allowed for twilight. In this way there is a saving effected of about $\frac{1}{10}$ th of the whole consumption.

If you should have occasion to try any photometric experiments allow me to suggest a form which after trials of other forms seems very satisfactory. Instead of using the wedge full of coloured liquid, which necessitates the *using of different parts of the glass sides*, I tried experiments first with a telescope and them with an India rubber bag having glasses placed on each end so that on compressing the bag the glasses were brought nearer each other & the extent of absorbing medium decreased. I ultimately on the suggestion of a friend however recurred to the plan I had thought of before which consisted

of a telescopic arrangement placed vertically & having a right angled prism at the bottom for rendering the horizontal rays vertical.



In the accompanying sketch *a b c d e f* represent the larger tube containing the coloured absorbing medium *g.h.* the sliding or eye piece tube having a glass at *g* by which the fluid is prevented from entering the sliding tube. *i* is the totally reflecting prism. The sliding tube is graduated on the side so that when *g* is close to the bottom *cd* the scale on the side is at zero. Of course the reading on the scale increases as *g* is removed from *cd* that is as the thickness of the absorbing medium traversed by the rays is increased. I hope the above is intelligible and trusting you will excuse this long letter I remain

My Dear Sir | Yours faithfully | Thomas Stevenson
Professor Faraday | &c &c

Endorsed by Faraday: Consumption of oil 1.6 pints per hour first order

1. George Biddell Airy. See Airy, W. (1896), 241.

Letter 3868

Faraday to Thomas Stevenson

25 October 1860

From the original in NLS MS 785, f. 46

Royal Institution | 25 Oct 1860

My dear Sir,

I hasten to return you my sincere thanks for your kindness in remembering me; and for the data both oil – & photometric¹ –

In photometry I am always puzzled about the standard of light – the eyes of the same person vary so much & so rapidly – and the appreciation varies so greatly with change of colour & difference of intensity – I think Your

telescopic apparatus is excellent in its arrangement – but it is only by trial one learns to know the circumstances_[.]

Ever My dear Sir | Most Truly Yours | M. Faraday
Thomas Stevenson Esqr | &c &c &c

1. See letter 3867.

Letter 3869

Faraday to Ernst Becker

25 October 1860

From the original in RI MS F1 E23

The Green, Hampton Court | 25 Oct 1860

My dear Dr. Becker,

It was a great delight to me to receive your very pleasant and affectionate letter last month¹. It told us so much about your proceedings & concerns, as to shew that you knew we had a very strong interest in all you were doing, and were likely to do; and it has given great pleasure to Dr. Bence Jones and some other friends, besides. First I rejoice that you have your mother² with you, and I cannot help imagining all the interest she will feel in hearing of your doing, and looking at you, & realizing the change and improvement that some years have made. A mother must make something of a home to you; and by home I imagine an arrangement very different to a Palace. You must have had a strong feeling for *home joys & happiness*:– for the *calm & peace* of such a place, to leave a Palace for the hopes of it; especially when that Palace was one where you were esteemed so highly_[.] I think the very feelings that induced you to make the change will help to create that home at last which will be the reward to you for it. Well, whatever change you may make in your life, we shall not alter in our thoughts about you.

I have been greatly interested in reading your account of your proceedings, at Bonn, Heidelberg, & Giessen. I am not competent to form an opinion of the best mode of pursuing science in Germany by a German mind; but the advice of Buff³ is that which would soonest fall in with my own thoughts and ways. I could not imagine much progress by reading only, without the experimental facts & trials which could be suggested by the reading. I was never able to make a fact my own without seeing it;– and the descriptions of the best works altogether failed to convey to my mind, such a knowledge of things as to allow myself to form a judgment upon them. It was so with *new* things. If Grove, or Wheatstone, or Gassiot, or any other told me a new fact & wanted my opinion, either of its value, or the cause, or the evidence it could give in any subject, I never could say any thing until I had seen the fact. For the same reason I never could work, as some Professors do most extensively, by students or pupils. All the work had to be my own. I know very well that

my mind is particularly constituted; that it is deficient in appreciation: and, further, that the difficulty is made greater by a failing memory. Nevertheless you will understand how my thoughts fall in with Buffs opinion; and how terrified I should be to set about learning Science from Books only. However, what we call accident has, in my life had much to do with the matter; for I had to work & prepare for others before I had earned the privilege of working for myself, and I have no doubt that was my great instruction and introduction into physical science.

You have seen many of my friends. When you see them again, or any who think kindly of me, commend me to them. I long to know more of Scientific men than I do; but I dare not try to increase my privileges in that direction by writing, for such an occupation soon grows up, & then becomes too large for my head to carry. I am indeed even beginning to be sorrowful in *reading*, because I cannot store up what I read;—cannot keep it in remembrance.

Our friends here are I think pretty well. Dr. Bence Jones has only lately returned from the Continent. Tyndall is at home & well at work. Wheatstone, Gassiot, &c I see little of just now. I heard that Sir Benj Brodie thought of giving up the Presidency of the Royal Society, but cannot say⁴.

I feel as if I were leaving much unsaid in this letter, but it w<ill> not come to mind. I do not forget you & all your kindness. Nor does my wife & Niece who send their kindest remembrances.

Believe me to be | My dear Dr. Becker | Ever faithfully Yours |
M. Faraday

Address: Dr. E. Becker | &c &c &c | Heidelberg | (in Waldhom) | Baden

1. Letter 3844.

2. Johanetta Chistine Becker, née Weber (1789–1878, Pangels (1996), 42).

3. Heinrich Buff (1805–1878, NDB). Professor of Physics at Giessen from 1838.

4. Brodie continued in the presidency until November 1861.

Letter 3870

Faraday to John Gibson Macvicar¹

25 October 1860

From the original in RCP

Royal Institution | 25 Oct 1860

My dear Sir,

I am very much obliged by your kindness. I shall have the opportunity of looking at your Tractate² carefully after a week or two but I am obliged to make a wonderful difference in respect of the manner in which I accept or hold at arms length a fact or body of facts and the superstructure which we of necessity raise upon them. Thanking you most heartily for your kindness

I am My dear Sir | Most truly Yours | M. Faraday
Revd. John G Macvicar | &c &c &c

1. John Gibson Macvicar (1800–1884, ODNB). Minister at Moffat from 1853 and scientific writer.
2. MacVicar (1860).

Letter 3871**Faraday to James Timmins Chance****26 October 1860****From the original in RI MS F1 N/4/19**

Royal Institution | Friday, 26 Oct 1860

My dear Sir,

I got your note today & accompanying it one from Mr. Parkes¹. Yours says come on Thursday², & Mr. Parkes says there is no hurry. Moreover he says I am to hear from the Board of Trade³. I do not know what to do amongst three announcements all in different stages[.] So I think I will decide on coming to you on Monday next the 29th. Leaving here by the 9.15 getting to Birmingham about 1 o'clk – getting a good lunch at the Railway hotel & securing a bedroom – reaching Spon Lane by 2.8 – and then working on by day light and night time as it may be needful⁴. Then if necessary I could be at the Works also next morning. But you must not let me put you out by what I say and any change *you* make shall suit me.

I am just going to write Mr. Parkes word: I do not know whether he intends to be at Birmingham[.] That I must leave to him[.]

Ever Truly Yours | M. Faraday

Jas. Chance Esq | &c &c &c

1. Neither found.

2. That is 1 November 1860.

3. Letter 3873.

4. Which is what Faraday did. Faraday to Booth, 1 November 1860, TNA MT 9/11, towards end of file M2379.

Letter 3872**Thomas Stevenson to Faraday****26 October 1860****From the original in GL MS 30108/5, f. 97**

Edinburgh, 84 George Street | 26 Oct 1860

My Dear Sir,

I beg to acknowledge receipt of your letter of the 24th [sic]¹.

I send you in case it may be useful some photometric experiments lately made by my brother² on the *vertical* divergence of an annular lens with a flame of our full standard height and volume from which you will observe that the most minute beam dips downwards a little. You will perhaps remember that

I shewed a tracing of it at Whitby. The observations were made at the sloping face of Salisbury Crags and the light was in a temporary lantern placed at the South & on the back of Canongate. We would also have tried the prisms but had none fitted up at the time. The levels of each station was [sic] carefully ascertained with the spirit level and from these angular values marked on the vertical line were computed.

Do not put yourself to the trouble of acknowledging receipt of this but if any further explanation be required I need not say how glad I shall be to afford it.

I remain | My dear Sir | Yours very truly | Thomas Stevenson
Professor Faraday | &c &c

Endorsed by Faraday: What was the height of the focal plane above the burner? | What is the standard of intensity or how was it ascertained? | What is the cause of diffusing at the focal plane?

1. Letter 3868.

2. David Stevenson (1815–1886, ODNB). Engineer to the Northern Lighthouse Board.

Letter 3873

James Booth¹ to Faraday

30 October 1860

From the original press copy in TNA MT4/72, p. 4976

30 Oct 1860

11711

state to you that Messrs. Wilkins and Company of Long Acre who is under contract with the Board of Trade for the supply of the dioptric lighting apparatus which H.M. Government is about to present to the Egyptian Government for the use of those lighthouses as existing in the Red Sea have informed my Lords that the Apparatus is ready for inspection at Messrs Chance Glass Works Birmingham.

My Lords would feel obliged if you could make it convenient to proceed to Birmingham to inspect the Apparatus and to favor them with your Report thereon.

My Lords will also be glad to be informed of the Amount of expenses incurred by you on this inspection which will at once be repaid to you by this Department.

Mr. Parkes of 19 Parliament Street, the Engineer superintending the construction of the Lighthouses and Messrs. Wilkins and Company have been instructed to place themselves in communication with you in order that proper arrangements may be made for your inspection of the Apparatus.

A copy of the Specifications is provided for your information²

I am | Sir | Your obedient Servant

M. Faraday Esq: F.R.S. | &c &c &c | Royal Institution | Albemarle Street

1. James Booth (c.1796–1880, ODNB). Secretary of the Board of Trade, 1850–1865. Author identified on the basis that Faraday to Booth, 1 November 1860, TNA MT 9/11, towards end of file M2379, is the reply.

2. A press copy of this follows this letter in the manuscript.

Previous Publication of Letters

This notes where the text of the letters in this volume have been previously published. It does not repeat the citation to letters that have only been located in published works. Nor does it note quotations of parts of letters used in critical studies of Faraday. Apart from Bence Jones (1870b), where there are substantial changes from Bence Jones (1870a), differences between different editions of texts are not noted.

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- 3035 Bence Jones (1870a), 2: 369–70, (1870b), 2: 364–5. Kahlbaum and Darbishire (1899), 252–3.
- 3044 Williams *et al.* (1971), 2: 808.
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- 3061 *The Ubyyssey*, 16 November 1923, p. 8, col. b. *Electrician*, 25 November 1938, p. 640.
- 3067 Williams *et al.* (1971), 2: 826.
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- 3071 Bence Jones (1870a), 2: 374, (1870b), 2: 369.
- 3072 Bence Jones (1870a), 2: 374, (1870b), 2: 369.
- 3075 Thompson (1910), 1: 312.
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- 3085 Williams *et al.* (1971), 2: 828–9.
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- 3120 Bence Jones (1870a), 2: 376–7, (1870b), 2: 371–2. Kahlbaum and Darbishire (1899), 267–7.
- 3124 Williams *et al.* (1971), 2: 835–6.
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- 3174 Williams *et al.* (1971), 2: 846–8.
- 3177 Williams *et al.* (1971), 2: 848.
- 3180 Williams *et al.* (1971), 2: 849–50.
- 3183 Williams *et al.* (1971), 2: 850–1.
- 3190 Williams *et al.* (1971), 2: 851–3.
- 3191 Kahlbaum and Darbishire (1899), 269–73.
- 3192 Hartmann (1939), 394–5. Williams *et al.* (1971), 2: 853–4.
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- 3203 Schoenbein (1856e). Kahlbaum and Darbishire (1899), 275–9.
- 3207 *Proc. Roy. Med. Chir. Soc.* 1857, 1: 71–4. Williams *et al.* (1971), 2: 857–9.
- 3209 Larmor (1907), 1: 150.
- 3216 Williams *et al.* (1971), 2: 859–60.
- 3219 Gladstone (1874), 83.
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- 3224 Kahlbaum and Darbishire (1899), 279–80.
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- 3247 Roscoe (1906), 138–9.
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- 3335 Schoenbein (1858a). Kahlbaum and Darbshire (1899), 285–92.
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- 3338 Williams *et al.* (1971), 2: 879–80.
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- 3353 Campbell and Garnett (1882), 288, (1884), 201.
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- 3425 Bence Jones (1870b), 2: 394.
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- 3462 Kahlbaum and Darbishire (1899), 301–7.
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- 3500 Williams *et al.* (1971), 2: 910.
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The Correspondence of Michael Faraday

Michael Faraday (1791–1867) was one of the most important men of science in nineteenth century Britain. His discoveries of electro-magnetic rotations (1821) and electro-magnetic induction (1831) laid the foundations of the modern electrical industry. His discovery of the magneto-optical effect and diamagnetism (1845) led him to formulate the field theory of electro-magnetism, which forms one of the cornerstones of modern physics.

These and a whole host of other fundamental discoveries in physics and chemistry, together with his lecturing at the Royal Institution, his work for the state (including Trinity House), his religious beliefs and his lack of mathematical ability, make Faraday one of the most fascinating scientific figures ever.

All these aspects of his life and work and others, such as his illnesses, are reflected in his correspondence. This volume, in which just over 70% of the 841 letters are previously unpublished, covers the latter half of the 1850s and most of 1860. Topics include: Faraday's work on regelation, the transmission of light through gold and his attempts to bring gravity into his general scheme of forces; the offer by Queen Victoria, and his acceptance, of a Grace and Favour House at Hampton Court; his advice to Trinity House, the Board of Trade and the Royal Commission on Lighthouses; his investigation of the deterioration of the stonework of the relatively new Houses of Parliament; the conservation issues surrounding the National Gallery's pictures; and his appointment by Emperor Napoleon III to be a Commander of the Legion of Honour.

Major correspondents included the Astronomer Royal G.B. Airy, the new Secretary of Trinity House P.H. Berthon, the Birmingham glassmaker J.T. Chance, the French chemist and politician J.B.A. Dumas, the Assistant Secretary of the Board of Trade T.H. Farrer, the German mathematician Julius Plücker, the Cambridge trained mathematical natural philosophers James Clerk Maxwell, George Gabriel Stokes and William Thomson, Faraday's colleague at the Royal Institution John Tyndall and the Swiss chemist Christian Schoenbein whose daughter died while in London.

Frank A J L James is Professor of the History of Science at the Royal Institution, President of the British Society for the History of Science and past President of the Newcomen Society. He has written on nineteenth century physics and chemistry in their various contexts and with Geoffrey Cantor and David Gooding wrote the biography *Michael Faraday* (Macmillan/Humanities Press). He has also edited a collection of essays on the history of the Royal Institution *The Common Purposes of Life* (Ashgate).

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