Æt. 1 *to* 12 (1791 *to* 1804).

MICHAEL FARADAY* was born in the working class, of a very religious family. For two generations at least those who preceded him shared the extreme views in favour of toleration and disestablishment which caused, first, the deposition of the Rev. John Glas, and afterwards the secession of his son-in-law, R. Sandeman, from the Presbyterian Church of Scotland. That the revealed will of Christ should be the supreme and only law, not only in all church questions, but in every thought and word and deed, was the belief of those who were nearest to Faraday in his infancy; and this he held throughout his life, as though it had been a special revelation to himself.

His father, James, was the third of ten children born at Clapham in Yorkshire. He was a blacksmith; his eldest brother worked as slater, grocer, and millowner, another brother was a farmer, another a packer, another a shopkeeper, and the youngest a shoemaker. Another of the brothers died young, in the year Michael was born; and a letter from the mother of the young man shows the strength of the religious feeling in mother and son.

When twenty-five, in 1786, James Faraday married Margaret Hastwell, daughter of a farmer near Kirkby Stephen. Soon after their marriage they came to Newington in Surrey, where Michael, their third child, was born, September 22, 1791, in a house probably long since pulled down. The father obtained work at Boyd's, in Welbeck Street; and when Michael was about five years old, after living a short time in Gilbert Street, they removed to rooms over a coach-house in Jacob's Well Mews, Charles Street, Manchester Square. The home of Michael Faraday was in these mews for nearly ten years; and his family remained there until 1809, when they moved to 18 Weymouth Street.

Faraday himself has pointed out where he played at marbles in Spanish Place, and where, years later, he took care of his little sister in Manchester Square. He says, "My education was of the most ordinary description, consisting of little more than the rudiments of reading, writing, and arithmetic at a common day-school. My hours out of school were passed at home and in the streets."

Only a few yards off was a bookseller's shop, No. 2 Blandford Street; there, as a boy of thirteen, in 1804, he went on trial for a year to Mr. George Riebau. Once when walking with a niece they passed a little news-boy, when he said, "I always feel a tenderness for those boys, because I once carried newspapers myself."

* An account of "Faraday as a Discoverer" having been already given to the world by one eminently qualified for the task, it has been deemed advisable in this place to give a narrative of the chief events of his personal history, with such indications of his character and opinions as may be read in his written correspondence and private memorials. This service has been kindly rendered by Dr. Bence Jones, F.R.S., Secretary to the Royal Institution, the devoted friend of Faraday, in whose hands have been placed the letters and manuscripts from which the substance, and, for the most part, the words of the present notice have been taken.—W. S., Sec. R.S.

Æt. 13 *to* 19 (1805 *to* 1811).

On the 7th of October, 1805, when fourteen, Faraday was apprenticed; and, in consideration of his faithful service, no premium was given to Riebau.

Four years later his father wrote (in 1809), "Michael is bookbinder and stationer, and is very active at learning his business. He has been most part of four years of his time out of seven. He has a very good master, and mistress, and likes his place well: he had a hard time for some while at first going; but, as the old saying goes, he has rather got the head above water, as there is two other boys under him."

Faraday himself says, "Whilst an apprentice I loved to read the scientific books which were under my hands, and amongst them delighted in Marcet's 'Conversations on Chemistry,' and the electrical treatises in the 'Encyclopædia Britannica.' I made such simple experiments in chemistry as could be defrayed in their expense by a few pence per week, and also constructed an electrical machine, first with a glass phial, and afterwards with a real cylinder, as well as other electrical apparatus of a corresponding kind." He told a friend that Watts on the Mind first made him think, and that his attention was turned to science by the article "Electricity" in an encyclopædia he was employed to bind.

"My master," he says, "allowed me to go occasionally of an evening to hear the lectures delivered by Mr. Tatum in natural philosophy at his house, 53 Dorset Street, Fleet Street. I obtained a knowledge of these lectures by bills in the streets and shop-windows near his house. The hour was eight o'clock in the evening. The charge was 1*s.* per lecture, and my brother Robert [who was three years older and followed his father's business] made me a present of the money for several. I attended twelve or thirteen lectures between February 19, 1810, and September 26, 1811. It was at these lectures I first became acquainted with Magrath, Newton, Nicol, and others."

He learned perspective of a Mr. Masquerier, that he might illustrate these lectures. "Masquerier lent me Taylor's Perspective, a 4to volume, which I studied closely, copied all the drawings, and made some other very simple ones, as of cubes or pyramids, or columns in perspective, as exercises of the rules. I was always very fond of copying vignettes and small things in ink; but I fear they were mere copies of the lines, and that I had little or no sense of the general effect and of the power of the lines in producing it." How he was educating himself at this time and the subjects that interested him, may be seen in a manuscript volume (a shadow of the future) which he called "The Philosophical Miscellany, being a collection of notices, occurrences, events, &c. relating to the arts and sciences collected from the public papers, reviews, magazines, and other miscellaneous works. Intended," he says, "to promote both amusement and instruction, and also to corroborate or invalidate those theories which are continually starting into the world of science. Collected by M. Faraday, 1809–10."

In 1811 (æt. 19) he became acquainted, at Mr. Tatum's, with Mr.

Huxtable and Mr. Benjamin Abbott; the first was a medical student, the other, who belonged to the Society of Friends, was employed in a house of business in the city.

Mr. Huxtable lent him Parkes's 'Chemistry,' which Faraday bound for him, and the third edition of Thompson's 'Chemistry.'

Æt. 20 (1812).

Among the few notes Faraday made of his own life are the following:—

"During my apprenticeship I had the good fortune, through the kindness of Mr. Dance, who was a customer of my master's shop and also a member of the Royal Institution, to hear four of the last lectures of Sir H. Davy in that locality [he always sat in the gallery over the clock]. The dates of these lectures were February 29, March 14, April 8 and 10, 1812. Of these I made notes, and then wrote out the lectures in a fuller form, interspersing them with such drawings as I could make. The desire to be engaged in scientific occupation, even though of the lowest kind, induced me, whilst an apprentice, to write, in my ignorance of the world and simplicity of my mind, to Sir Joseph Banks, then President of the Royal Society. Naturally enough, 'No answer,' was the reply left with the porter."

On Sunday, July 12, 1812, three months before his apprenticeship was over, he wrote the first of a series of letters to his friend Mr. Benjamin Abbott (who was a year and a half younger than himself), from which a full view can be gained of what he was by nature, and what his self-education at this time had made him.

"I have lately made a few simple galvanic experiments merely to illustrate to myself the first principles of the science. I was going to Knight's to obtain some nickel, and bethought me that they had malleable zinc. I inquired and bought some; have you seen any yet? The first portion I obtained was in the thinnest pieces possible,—observe, in a flattened state. It was, they informed me, thin enough for the electric smoke, or, as I before called it, De Luc's electric column. I obtained it for the purpose of forming disks, with which and copper, to make a little battery. The first I completed contained the immense number of seven pair of plates!!! and of the immense size of halfpence!!!!!! I, sir, I, my own self, cut out seven disks of the size of halfpences each! I, sir, covered them with seven halfpence, and I interposed between seven, or rather six, pieces of paper soaked in a solution of muriate of soda!!! But laugh no longer, dear A., rather wonder at the effects this trivial power produced; it was sufficient to produce the decomposition of sulphate of magnesia, an effect which extremely surprised me." And then he describes how he built up a larger battery, and obtained greater and further effects, and reasons on the results, and urges his friend to think of these things, and "let me, if you please, sir, if you please let me know your opinion." On the Monday he adds a postscript: "I am just now involved in a fit of vexation. I have an excellent prospect before me,

and cannot take it up for want of ability. Had I perhaps known as much of mechanics, mathematics, mensuration, and drawing as I do perhaps of some other sciences, that is to say, had I happened to employ my mind with these instead of other sciences, I could have obtained a place, an easy place, too, and that in London, at 5', 6', 7', £800 per annum. Alas! alas! Inability. I must ask your advice on the subject, and intend, if I can, to see you next Sunday; one necessary branch of knowledge would be that of the steam-engine, and, indeed, anything where iron is concerned."

In his next letter he says, speaking of fresh experiments with his battery, "I must trust to your experiments more than my own; I have no time, and the subject requires several;" and in a letter written August 11, "Pyrotechny is a beautiful art, but I never made any practical progress in it, except in the forming a few bad squibs; so that you will gain little from me on that point."

In his next letter (August 19) he says, "I cannot see any subject except chlorine to write on. Be not surprised, my dear A., at the ardour with which I have embraced this new theory. I have seen Davy himself support it. I have seen him exhibit experiments (conclusive experiments) explanatory of it; and I have heard him apply these experiments to the theory, and explain and enforce them in (to me) an irresistible manner. Conviction, sir, struck me, and I was forced to believe him, and with that belief came admiration."

In a letter dated about a fortnight before his apprenticeship was out he writes, "Your commendations of the MS. lectures [of Davy] compel me to apologize most humbly for the numerous (very, very numerous) errors they contain. If I take you right, the negative words 'no flattery' may be substituted by the affirmative 'irony;' be it so, I bow to the superior scholastic erudition of Sir Ben. There are in them errors that will not bear to be jested with, since they concern not my own performance so much as the performance of Sir H., and those are errors in theory; there are, I am conscious, errors in theory, and those errors I would wish you to point out to me before you attribute them to Davy."

In the last letter before the great change came (October 1, 1812), he says, "I rejoice in your determination to pursue the subject of electricity, and have no doubt that I shall have some very interesting letters on the subject. I shall certainly wish to (and will if possible) be present at the performance of the experiments; but you know I shall shortly enter on the life of a journeyman, and then I suppose time will be more scarce than it is even now."

On the 8th of October he went as journeyman bookbinder to a Mr. De la Roche, then a French emigrant in London. His master was a very passionate man, and troubled his assistant much; so much, that he felt he could not remain in that place, though every inducement was held out to him. His master liked him; and, to tempt him to stay, said "I have no child, and if you will stay with me you shall have all I have when I am gone."

In his first letter to his friend Abbott, after his apprenticeship was ended,

October 11, he says, "As for the change which you suppose to have taken place with respect to my situation and affairs, I have to thank my late master, it is but little. Of liberty and of time I have, if possible, less than before, though I hope my circumspection has not at the same time decreased. I am well aware of the irreparable evils that an abuse of those blessings will give rise to. These were pointed out to me by common sense; nor do I see how anyone who considers his own station and his own free occupations, pleasures, actions, &c. can unwittingly engage himself in them. I thank that Cause to whom thanks are due that I am not in general a profuse waster of those blessings which are bestowed on me as a human being; I mean health, sensation, time, and temporal resources. Understand me here, for I wish not to be mistaken: I am well aware of my own nature; it is evil, and I feel its influence strongly. I know, too, that——; but I find that I am passing insensibly to a point of divinity; and as these matters are not to be treated lightly, I will refrain from pursuing it."

To his friend Huxtable he writes on the 18th: "Conceiving it would be better to delay my answer until my time was expired, I did so; that took place Oct. 7, and since then I have had by far less time and liberty than before. With respect to a certain place I was disappointed, and am now working at my old trade, the which I wish to leave at the first convenient opportunity. I am at present in very low spirits, and scarce know how to continue on in a strain that will be any way agreeable to you."

"Under the encouragement of Mr. Dance," he says, "I wrote to Sir Humphry Davy, sending, as a proof of my earnestness, the notes I had taken of his last four lectures; the reply was immediate, kind, and favourable. After this I continued to work as a bookbinder, with the exception of some days during which I was writing as an amanuensis for Sir H. Davy, at the time when the latter was wounded in the eye from an explosion of the chloride of nitrogen."

On the 24th of December, 1812, Sir Humphry Davy wrote to Faraday:— "Sir, I am far from displeased with the proof you have given me of your confidence, and which displays great zeal, power of memory, and attention. I am obliged to go out of town, and shall not be settled in town till the end of January; I will then see you at any time you wish. It would gratify me to be of any service to you; I wish it may be in my power. I am, Sir, Your obedient humble Servant."

Æt. 21 (1813).

He "went," he says, "to the City Philosophical Society, which was founded in 1808 at Mr. Tatum's house, and, I believe, by him. He introduced me as a member of the Society in 1813. Magrath was Secretary to the Society. It consisted of thirty or forty individuals, perhaps all in the humble or moderate rank of life. Those persons met every Wednesday evening for mutual instruction. Every other Wednesday the members were alone, and considered and discussed such questions as were brought forward by

each in turn. On the intervening Wednesday evenings friends also of the members were admitted, and a lecture was delivered, literary or philosophical, each member taking the duty, if possible, in turn (or in default paying a fine of half a guinea). This Society was very moderate in its pretensions, and most valuable to the members in its results." [I remember, too, says one of the members, we had a "class-book," in which, in rotation, we wrote essays, and passed it to each other's houses.]

Sir H. Davy, at his first interview, advised him to keep in business as a bookbinder, and he promised to give him the work of the Institution, as well as his own and that of as many of his friends as he could influence.

One night, in Weymouth Street, he was startled by a loud knock at the door, and on looking out he saw a carriage from which the footman had alighted and left a note for him. This was a request from Sir H. that he would call on him the next morning. Sir H. then referred to their former interview, and inquired whether he was still in the same mind, telling him that if so he would give him the place of assistant in the laboratory of the Royal Institution, from which he had on the previous day ejected its former occupant. The salary was to be 25*s.* a week, with two rooms at the top of the house.

In the minutes of the meeting of Managers on the 1st of March, 1813, is this entry :—"Sir Humphry Davy has the honour to inform the Managers that he has found a person who is desirous to occupy the situation in the Institution lately filled by William Payne. His name is Michael Faraday. He is a youth of twenty-two years of age. As far as Sir H. Davy has been able to observe or ascertain, he appears well fitted for the situation. His habits seem good, his disposition active and cheerful, and his manner intelligent. He is willing to engage himself on the same terms as given to Mr. Payne at the time of quitting the Institution.

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

As early as the 8th of March, Faraday dates his first letter from the Royal Institution to his friend Abbott.

"I have been employed," he says, "to-day in part in extracting the sugar from a portion of beetroot, and also in making a compound of sulphur and carbon—a combination which has lately occupied in a considerable degree the attention of chemists."

A month later he says :—"When writing to you I seize that opportunity of striving to describe a circumstance or an experiment clearly, so that you will see I am urged on, by selfish motives partly, to our mutual correspondence ; but though selfish yet not censurable.

"Agreeable to what I have said above, I shall at this time proceed to acquaint you with the results of some more experiments on the detonating compound of chlorine and azote ; and I am happy to say I do it at my ease, for I have escaped (not quite unhurt) from four different and strong explosions of the substance. Of these the most terrible was when I was

holding between my thumb and finger a small tube containing $7\frac{1}{2}$ grains of it. My face was within 12 inches of the tube, but I fortunately had on a glass mask. It exploded by the slight heat of a small piece of cement that touched the glass above half an inch from the substance, and on the outside. The explosion was so rapid as to blow my hand open, tear off a part of one nail, and has made my fingers so sore that I cannot yet use them easily. The pieces of tube were projected with such force as to cut the glass face of the mask I had on."

On the 1st of June he writes :—"The subject upon which I shall dwell more particularly at present has been in my head for a considerable time, and it now bursts forth in all its confusion. The opportunities that I have lately had of attending and obtaining instruction from various lecturers in their performance of the duty attached to that office, has enabled me to observe the various habits, peculiarities, excellencies, and defects of each of them, as they were evident to me during the delivery. I did not wholly let this part of the things occurrent escape my notice ; but, when I found myself pleased, endeavoured to ascertain the particular circumstance that had affected me ; also, when attending to Mr. Brande and Mr. Powell in their lectures, I observed how the audience were affected, and by what their pleasure and their censure was drawn forth.

"It may perhaps appear singular and improper that one who is entirely unfit for such an office himself, and who does not even pretend to any of the requisites for it, should take upon him to censure and to commend others, to express satisfaction at this, to be displeased with that, according as he is led by his judgment, when he allows that his judgment is unfit for it ; but I do not see, on consideration, that the impropriety is so great. If I am unfit for it, it is evident that I have yet to learn ; and how learn better than by the observation of others ? If we never judge at all we shall never judge right ; and it is far better to learn to use our mental powers (though it may take a whole life for the purpose) than to leave them buried in idleness, a mere void." And then for three letters he goes on with his ideas on lecture-rooms, lectures, apparatus, diagrams, experiments, audiences ; and when urged, two years later, to complete his remarks, he answers, Dec. 31, 1816 :—"With respect to my remarks on lectures, I perceive I am but a mere tyro in the art, and therefore you must be satisfied with what you have, or expect at some future time a recapitulation, or rather revision of them."

"During this spring Magrath and I established the mutual-improvement plan, and met at my rooms up in the attics of the Royal Institution, or at Wood Street at his warehouse. It consisted perhaps of half a dozen persons, chiefly from the City Philosophical Society, who met of an evening to read together, and to criticise, correct, and improve each other's pronunciation and construction of language. The discipline was very sturdy, the remarks very plain and open, and the results most valuable. This continued for several years." Saturday night was the time of meeting

at the Royal Institution, in the furthest and uppermost room in the house, then Faraday's place of residence.

He says:—"In the autumn Sir H. Davy proposed going abroad, and offered me the opportunity of going with him as his amanuensis, and the promise of resuming my situation in the Institution upon my return to England. Whereupon I accepted the offer, left the Institution on the 13th of October, and, after being with Sir H. Davy in France, Italy, Switzerland, the Tyrol, Geneva, &c. in that and the following year, returned to England and London the 23rd April 1815."

Whilst abroad he kept a daily journal, "not," he said, "to instruct or to inform, or to convey even an imperfect idea of what it speaks; its sole use is to recall to my mind at some future time the things I see now, and the most effectual way to do that will be, I conceive, to write down, be they good or bad, my present impressions." From this journal, and from his letters to his mother and his friend Benjamin Abbott, only a few characteristic passages can be given here.

In his journal he wrote, Wednesday, 13th October:—"This morning formed a new epoch in my life. I have never before, within my recollection, left London [he had as an infant gone to Newcastle and Whitehaven, by sea chiefly] at a greater distance than twelve miles, and now I leave it perhaps for many years, to visit spots between which and home whole realms will intervene. 'T is indeed a strange venture at this time to trust ourselves in a foreign and hostile country, where also so little regard is had to protestations and honour, that the slightest suspicion would be sufficient to separate us for ever from England, and perhaps from life. But curiosity has frequently incurred dangers as great as these, and therefore why should I wonder at it in the present instance. If we return safe, the pleasures of recollection will be highly enhanced by the dangers encountered; and a never-failing consolation is that, whatever be the fate of our party, variety, a great source of amusement, and pleasure must occur."

Some idea of the variety of his observations may be got from this note, 28th October, Dreux:—"I cannot help dashing a note of admiration to one thing found in this part of the country—the pigs! At first I was positively doubtful of their nature; for though they have pointed noses, long ears, rope-like tails, and cloven feet, yet who would have imagined that an animal with a long thin body, back and belly arched upwards, lank sides, long slender feet, and capable of outrunning our horses for a mile or two together, could be at all allied to the fat sow of England! When I first saw one, which was at Morlaix, it started so suddenly, and became so active in its motions on being disturbed, and so dissimilar in its actions to our swine, that I looked out for a second creature of the same kind before I ventured to decide on its being a regular or an extraordinary production of nature; but I find they are all alike, and that what at a distance I should judge to be a greyhound, I am obliged, on a near approach, to acknowledge a pig."

Æt. 22 (1814).

To his mother he writes, April 14, 1814, from Rome:—"When Sir H. Davy first had the goodness to ask me whether I would go with him, I mentally said, 'no, I have a mother, I have relations here,' and I almost wished that I had been insulated and alone in London; but now I am glad that I have left some behind me on whom I can think, and whose actions and occupations I can picture in my mind. Whenever a vacant hour occurs I employ it by thinking on those at home. In short, when sick, when cold, when tired, the thoughts of those at home are a calm and refreshing balm to my heart. Let those who think such thoughts are useless, vain, and paltry think so still. I envy them not their more refined and more estranged feelings. Let them look about the world unencumbered by such ties and heart-strings, and let them laugh at those who, guided more by nature, cherish such feelings. For me, I still cherish them, in opposition to the dictates of modern refinement, as the first and greatest sweetness in the life of man."

In a letter to his friend Abbott, dated September 6, 1814, he says:—"I fancy that when I set my foot in England I shall never take it out again; for I find the prospect so different from what it at first appeared to be, that I am certain, if I could have foreseen the things that have passed, I should never have left London. In the second place, enticing as travelling is (and I appreciate fully its advantages and pleasures), I have several times been more than half decided to return hastily home; but second thoughts have still induced me to try what the future may produce, and now I am only detained by the wish of improvement. I have learned just enough to perceive my ignorance, and, ashamed of my defects in everything, I wish to seize the opportunity of remedying them. The little knowledge I have gained in languages makes me wish to know more of them, and the little I have seen of men and manners is just enough to make me desirous of seeing more; added to which, the glorious opportunity I enjoy of improving in the knowledge of chemistry and the sciences continually, determines me to finish this voyage with Sir Humphry Davy; but if I wish to enjoy those advantages I have to sacrifice much; and though those sacrifices are such as an humble man would not feel, yet I cannot quietly make them. Travelling, too, I find, is almost inconsistent with religion (I mean modern travelling), and I am yet so old-fashioned as to remember strongly (I hope perfectly) my youthful education, and upon the whole, *malgré* the advantages of travelling, it is not impossible but that you may see me at your door when you expect a letter."

Æt. 23 (1815).

On the 25th January 1815, he writes:—"You tell me I am not happy, and you wish to share my difficulties. I have nothing important to tell you, or you should have known it long ago; but, since your friendship makes you feel for me, I will trouble you with my trifling affairs.

"It happened, a few days before we left England, that Sir H.'s valet declined going with him, and in the short space of time allowed by circumstances, another could not be got. Sir H. told me he was very sorry, but that if I would do such things as were absolutely necessary for him until he got to Paris, he should there get another. I murmured, but agreed. At Paris he could not get one; at Lyons he could not get one; at Montpellier he could not get one; nor at Genoa, nor at Florence, nor at Rome, nor in all Italy; and I believe at last he did not wish to get one; and we are just the same now as we were when we left England. This, of course, throws things into my duty which it was not my agreement, and is not my wish to perform, but which are, if I remain with Sir H., unavoidable. These, it is true, are very few; for having been accustomed in early years to do for himself, he continues to do so at present, and he leaves very little for a valet to perform; and as he knows that it is not pleasing to me, and that I do not consider myself as obliged to do it, he is always as careful as possible to keep those things from me which he knows would be disagreeable. But Lady Davy is of another humour. She likes to show her authority, and at first I found her extremely earnest in mortifying me. This occasioned quarrels between us, at each of which I gained ground and she lost it; for the frequency made me care nothing about them and weakened her authority, and after each she behaved in a milder manner. Sir H. has also taken care to get servants of the country, ycleped *lacquais de place*, to do everything she can want, and now I am somewhat comfortable; indeed at this moment I am perfectly at liberty, for Sir H. has gone to Naples to search for a house or lodging to which we may follow him, and I have nothing to do but see Rome, write my journal, and learn Italian."

About the same time he writes to his friend Huxtable:—

"Since Sir H. has left England he has made a great addition to chemistry in his researches on the nature of iodine. He first showed that it was a simple body. He combined it with chlorine and hydrogen, and lately with oxygen, and thus has added three acids of a new species to the science. He combined it with the metals, and found a class of salts analogous to the hyperoxymuriates. He still further combined these substances, and investigated their curious and singular properties.

"The combination of iodine with oxygen is a late discovery, and the paper has not yet perhaps reached the Royal Society. It confirms all Sir H.'s former opinions and statements, and shows the inaccuracy of the labours of the French chemists on the same subjects.

"Sir Humphry also sent a long paper lately to the Royal Society, on the ancient Greek and Roman colours, which will be worth your reading when it is printed."

A fortnight after his return to England he was engaged as assistant in the laboratory at a salary of 30*s*. a week, and apartments were given to him.

Æt. 24 (1816).

On the 17th of January, 1816, Faraday began a course of seventeen Lectures on Chemistry, at the City Philosophical Society, which extended over two years and a half. He called them "an account of the inherent Properties of Matter, of the forms in which matter exists, and of simple elementary substances." During the year he gave six or seven lectures on the general properties of matter, on the attraction of cohesion, on chemical affinity, on radiant matter, on oxygen, chlorine, iodine, and fluorine, on hydrogen, and on nitrogen. He wrote his first lectures at full length, whilst of the latter lectures he only made notes, putting the experiments very distinctly apart, and he kept very much to this plan during the rest of his life.

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

Early in February he thus wrote to his friend Abbott:—"Be not offended that I turn to write you a letter, because I feel a disinclination to do anything else; but rather accept it as a proof that conversation with you has more power with me than any other relaxation from business,—business I say; and I believe it is the first time for many years that I have applied it to my own occupations. But at present they actually deserve the name; and you must not think me in a laughing mood, but in earnest. It is now 9 o'clock P.M., and I have just left the laboratory and the preparation for to-morrow's two lectures. Our double course makes me work enough; and to them add the attendance required by Sir H. in his researches, and then if you compare my time with what is to be done in it, you will excuse the slow progress of our correspondence on my side. Understand me, I am not complaining; the more I have to do the more I learn, but I wish to avoid all impression on your side that I am lazy—suspicions, by-the-by, which a moment's reflection convinces me can never exist."

In consideration of the additional labour caused to him by Mr. Brande's

lectures in the laboratory, his salary at the Institution was increased to £100 per annum.

This year Faraday began a common-place book, in which he continued to make entries on all subjects for fifteen years. Some of the earliest are on the production of oxygen, on the combustion of zinc and iron in condensed air, on a course of lectures on geology delivered at the Royal Institution by Mr. Brande, and an account of Zerah Colburn, thirteen years old, the American calculating boy. Sir H. Davy sent him with a note, saying "his father will explain to you the method the son uses, in confidence; I wish to ascertain if it can be practically used."

He wrote in this year:—"When Mr. Brande left London in August, he gave the Quarterly Journal in charge to me; it has very much of my time and care, and writing through it has been more abundant with me. It has, however, also been the means of giving me earlier information on some new objects of science."

Æt. 25 (1817).

In 1817 he gave five lectures at the City Philosophical Society on the atmosphere, on sulphur and phosphorus, on carbon, on combustion, and on the metals generally. He had a paper in the Quarterly Journal on the escape of gases through capillary tubes. The entries in his common-place book consist of geological notes of South Moulton Slate, Tiverton, Hulverston, Taunton, Somerton, and Castle Cary; a multitude of chemical queries or questions to be worked at, among which are the exciting effects of different vapours and gaseous mixtures; compounds of chlorine and carbon made out in the autumn of 1820; electricity, magnetism; a pyrometer; extracts from Shakspeare, Lalla Rookh, Rambler, &c.

At the end of the year he tells his friend Abbott that he can see less of him, "in consequence of an arrangement I have made with a gentleman recommended to me by Sir H. Davy; I am engaged to give him lessons in mineralogy and chemistry, three times a week, in the evenings, for a few months."

Æt. 26 (1818).

In 1818 five lectures were given by Faraday at the City Philosophical Society, on gold, silver, &c., on copper and iron, on tin, lead, and zinc, and on alkalies and earths. He had six papers in the Quarterly Journal, of which the most important was on sounds produced by flame in tubes.

In his common-place book there is a long course of lectures on oratory, by Mr. B. H. Smart; questions for Dorset Street; an experimental agitation of the question of electrical induction, "Bodies do not act where they are not—query, is not the reverse of this true? Do not all bodies act where they are not; and do any of them act where they are? Query, the nature of courage; is it a quality or a habit?" Chemical questions.

On July 1st he gave a lecture to the City Philosophical Society. It is entitled "Observations on the Inertia of the Mind." As this lecture is wholly

written out, it probably was one of the essays contained in the class-book of the Society.

Towards the end of the year Faraday wrote his first letter to M. G. De la Rive, the father of the present M. Auguste De la Rive. He says :—

"Dear Sir,—Your kindness, when here, in requesting me to accept the honour of a communication with you on the topics which occur in the general progress of science, was such as almost to induce me to overstep the modesty due to my humble situation in the philosophical world, and to accept of the offer you made me. But I do not think I should have been emboldened thus to address you had not Mr. Newman since then informed me that you again expressed a wish to him that I should do so; and fearful that you should misconceive my silence I put pen to paper, willing rather to run the risk of being thought too bold than of incurring the charge of neglect towards one who had been so kind to me in his expressions. My slight attempts to add to the general stock of chemical knowledge have been received with favourable expressions by those around me; but I have, on reflection, perceived that this arose from kindness on their parts, and the wish to incite me on to better things. I have always, therefore, been fearful of advancing on what has been said, lest I should assume more than was intended; and I hope that a feeling of this kind will explain to you the length of time which has elapsed between the time when you requested me to write and the present moment when I obey you.

"I am not entitled, by any peculiar means of obtaining a knowledge of what is doing at the moment in science, to deserve your attention, and I have no claims in myself to it. I judge it probable that the news of the philosophical world will reach you much sooner through other more authentic and more dignified sources, and my only excuse even for this letter is obedience to your wishes, and not on account of anything interesting for its novelty." He then describes a new process for the preparation of gas for illumination. He ends, "I am afraid that, with all my reasons, I have not been able to justify this letter. If my fears are true I regret at least; it was your kindness that drew it from me, and to your kindness I must look for an excuse."

Æt. 27 (1819).

In 1819 he had no paper in the Quarterly Journal. He gave one lecture at the City Philosophical Society on the Forms of Matter. Matter he classifies into four states, which depend on differences in the essential properties, and cautiously says, "thus a partial reconciliation is established to the belief that all the variety of this fair globe may be converted into three kinds of radiant matter."

His common-place book contains scarcely any scientific notices.

On July the 10th he started by coach for a three weeks' walking tour in Wales, with his friend Magrath. He kept a journal, and his descriptions of the scenery, of the copper works of Swansea, the mines of Anglesea, and the slate-quarries of Bangor, are still of interest.

Æt. 28 (1820).

This year was one of the most important in the life of Faraday; he had his first paper read to the Royal Society on two new compounds of chlorine and carbon, and on a new compound of iodine, carbon, and hydrogen; and with Mr. Stodart, the surgical instrument maker, he published, in the Quarterly Journal of Science, experiments on the alloys of steel, made with a view to its improvement.

In his common-place book, among the chemical questions, we find chemical lessons, or a plan of lessons in chemistry, and processes for manipulation, the germ of his work on Chemical Manipulation. There is also a list headed "Lecture Subjects," including application of statics to chemistry, approximation of mechanical and chemical philosophy, application of mathematics to actual service and use in the arts, series of mechanical arts, as tanning.

On the 20th of April he writes to M. G. De la Rive:—"I never in my life felt such difficulty in answering a letter as I do at this moment your very kind one of last year. I was delighted on receiving it to find that you had honoured me with any of your thoughts, and that you would permit me to correspond with you by letter. Mr. Stodart and myself have lately been engaged in a long series of experiments and trials on steel, with the hope of improving it, and I think we shall in some degree succeed. We are still very much engaged in the subject; but if you will give me leave I will, when they are more complete, which I expect will be shortly, give you a few notes on them. I succeeded by accident a few weeks ago in making artificial plumbago, but not in useful masses. We have lately had some important trials for oil in this metropolis, in which I, with others, have been engaged. They have given occasion for many experiments in oil, and the discovery of some new and curious results; one of the trials only is finished, and there are four or five more to come. As soon as I can get time, it is my intention to trace more closely what takes place in oil by heat."

June 26 he sends a long abstract of the paper on Steel, and ends:—"Now I think I have noticed the most interesting points at which we have arrived. Pray pity us, that after two years' experiments we have got no further; but I am sure if you knew the labour of the experiments you would applaud us for our perseverance at least. We are still encouraged to go on, and I think the experience we have gained will shorten our future labours.

"If you should think any of our results worth notice in the 'Bibliothèque,' this letter is free to be used in any way you please. Pardon my vanity for supposing anything I can assist in doing can be worth attention; but you know we live in the good opinion of ourselves and of others, and therefore naturally think better of our own productions than they deserve."

Early the following month there is evidence that an entire change took

place in the state of his mind. Among his friends was Mr. Edward Barnard, one of a family living in Paternoster Row, with which he had long been intimate, and which agreed with his own family in its religious views. Faraday proposed to, and ultimately was accepted by, Mr. Barnard's sister, Sarah.

Æt. 29 (1821).

March 11, Sir H. Davy wrote :—" Dear Mr. Faraday, I have spoken to Lord Spencer, and I am in hopes that your wishes may be gratified; but do not mention the subject till I see you." This wish was probably to bring his wife to the Institution. In June he was appointed superintendent of the house and laboratory, in the absence of Mr. Brande.

All obstacles were removed, and the marriage took place on the 12th of June. Mr. Faraday, desiring that the day should be considered just like any other day, offended some of his near relations by not asking them to his wedding.

In a letter to his wife's sister, previous to the marriage, he says, " There will be no bustle, no noise, no hurry occasioned even in one day's proceeding. In externals, that day will pass like all others, for it is in the heart that we expect and look for pleasure."

A month later, at a meeting of the congregation, he was fully admitted as a member of the Sandemanian Church.

His common-place book shows that he read little. In a letter, May 19, to M. G. De la Rive, he says, " Mr. Stodart and myself are continuing our experiments on steel, which are very laborious."

On July 12, a paper was read to the Royal Society on a new Compound of Chlorine and Carbon, by Phillips and Faraday. This, as well as Faraday's previous paper on two Chlorides of Carbon, was printed in the Philosophical Transactions. In the Quarterly Journal he had a short paper on the Vapour of Mercury at common temperatures.

On the 12th of September he writes the following letter to M. G. De la Rive :—

" You partly reproach us here with not sufficiently esteeming Ampère's experiments on electro-magnetism. Allow me to extenuate your opinion a little on this point. With regard to the experiments, I hope and trust that due weight is allowed to them; but these you know are few, and theory makes up the great part of what M. Ampère has published, and theory in a great many points unsupported by experiments, when they ought to have been adduced. At the same time, M. Ampère's experiments are excellent, and his theory ingenious ; and for myself, I had thought very little about it before your letter came, simply because, being naturally sceptical on philosophical theories, I thought there was a great want of experimental evidence. Since then, however, I have engaged on the subject, and have a paper in our Institution journal, which will appear in a week or two, and that will, as it contains experiment, be immediately applied by M. Ampère in sup-

port of his theory much more decidedly than it is by myself. I intend to enclose a copy of it to you, and only want the means of sending it.

"I find all the usual attractions and repulsions of the magnetic needle by the conjunctive wire are deceptions, the motions being not attractions or repulsions, nor the result of any attractive or repulsive forces, but the result of a force in the wire, which, instead of bringing the pole of the needle nearer to or further from the wire, endeavours to make it move round it in a never-ending circle and motion whilst the battery remains in action. I have succeeded not only in showing the existence of this motion theoretically, but experimentally, and have been able to make the wire revolve round a magnetic pole, or a magnetic pole round the wire, at pleasure. The law of revolution, and to which all the other motions of the needle and wire are reducible, is simple and beautiful. Conceive a portion of connecting wire north and south, the north end being attached to the positive pole of a battery, the south to the negative; a north magnetic pole would then pass round it continually in the apparent direction of the sun from east to west above, and from west to east below. Reverse the connexions with the battery, and the motion of the pole is reversed. Or if the south pole is made to revolve, the motions will be in the opposite directions, as with the north pole.

"If the wire be made to revolve round the pole, the motions are according to those mentioned. For the apparatus I used there were but two plates, and the direction of the motions was of course the reverse of those with a battery of several pair of plates, and which are given above. Now I have been able experimentally to trace this motion into its various forms, as exhibited by Ampère's helices, &c., and in all cases to show that dissimilar poles repel as well as attract, and that similar poles attract as well as repel, and to make, I think, the analogy between the helice and common bar-magnet far stronger than before; but yet I am by no means decided that there are currents of electricity in the common magnet. I have no doubt that electricity puts the circles of the helice into the same state as those circles are in that may be conceived in the bar-magnet; but I am not certain that this state is directly dependent on the electricity, or that it cannot be produced by other agencies, and therefore, until the presence of electrical currents be proved in the magnet by other than magnetical effects, I shall remain in doubts about Ampère's theory."

Oct. 8th he writes to J. Stodart, Esq.:—

"I hear every day more and more of those sounds, which, though only whispers to me, are, I suspect, spoken aloud amongst scientific men, and which, as they in part affect my honour and honesty, I am anxious to do away with, or at least to prove erroneous in those parts which are dishonourable to me. You know perfectly well what distress the very unexpected reception of my paper on Magnetism in public has caused me, and you will not therefore be surprised at my anxiety to get out of it, though I give trouble to you and others of my friends in doing so. If I under-

stand aright, I am charged (1) with not acknowledging the information I received in assisting Sir H. Davy in his experiments on this subject; (2) with concealing the theory and views of Dr. Wollaston; (3) with taking the subject whilst Dr. Wollaston was at work on it; and (4) with dishonourably taking Dr. Wollaston's thoughts, and pursuing them without acknowledgment to the results I have brought out.

"There is something degrading about the whole of these charges; and were the last of them true, I feel that I should not remain on the terms I now stand at with you or any scientific person. Nor can I indeed bear to remain suspected of such a thing. My love for scientific reputation is not yet so high as to induce me to obtain it at the expense of honour, and my anxiety to clear away this stigma is such, that I do not hesitate to trouble you, even beyond what you may be willing to do for me."

He proceeds then to justify himself, and says, "The cause of my making the experiments detailed in my paper, was the writing of the historical Sketch of Electromagnetism that has appeared in the last two Numbers of the 'Annals of Philosophy.'"

On the 30th of October he writes directly to Dr. Wollaston, saying:—"I heard from two or three quarters that it was considered that I had not behaved honourably, and that the wrong I had done I had done to you; I immediately wished and endeavoured to see you, but was prevented by the advice of my friends, and am only now at liberty to pursue the plan I intended to have taken at first.

"If I have done any one wrong it was quite unintentional, and the charge of behaving dishonourably is not true. I am bold enough, sir, to beg the favour of a few minutes' conversation with you on this subject, simply for these reasons, that I can clear myself, that I owe obligations to you, that I respect you, that I am anxious to escape from unfounded impressions against me, and, if I have done any wrong, that I may apologise for it."

The following day Dr. Wollaston writes:—"You seem to me to labour under some misapprehension of the strength of my feelings upon the subject to which you allude. As to the opinions which others may have of your conduct, that is your concern, not mine; and if you fully acquit yourself of making any incorrect use of the suggestions of others, it seems to me that you have no occasion to trouble yourself much about the matter. But if you are desirous of any conversation with me, and could with convenience call to-morrow morning between ten and half-past ten, you will be sure to find me."

In a letter to M. G. De la Rive a fortnight later, he does not allude to the distress of mind he had gone through.

On Christmas Day he succeeded in making a wire through which a current of voltaic electricity was passing obey the magnetic poles of the earth in the way it does the poles of a bar-magnet.

Mr. George Barnard, who was with him in the laboratory at the time,

writes :— 'All at once he exclaimed, 'Do you see, do you see, do you see, George!' as the small wire began to revolve. One end I recollect was in the cup of quicksilver, the other attached above to the centre. I shall never forget the enthusiasm expressed in his face, and the sparkling in his eyes!"

Æt. 30 (1822).

In 1822, a paper on the Alloys of Steel by Stodart and Faraday was read to the Royal Society, and printed in the Transactions. In the Quarterly Journal of Science he had two papers on the Changing of Vegetable Colours as an alkaline property, and on some Bodies possessing it; and on the Action of Salts on Turmeric Paper.

The results of the paper on steel were of no practical value, and this, one of his first and most laborious investigations, is strikingly distinguished from all his other works by ending in nothing.

This year he began a fresh manuscript volume, which he called "Chemical Notes, Hints, Suggestions, and Objects of Pursuit." To it he transferred many of the queries out of his common-place book, but he separated his subjects under different heads. He puts as a sort of preface, "I already owe much to these notes, and think such a collection worth the making by every scientific man. I am sure none would think the trouble lost after a year's experience." When a query got answered, he drew a pen through it, and wrote the date of the answer across it. In this book are the first germs, in the fewest possible words, of his future work.

The last week in July he went with his friend Richard Phillips to Mr. Vivian's, near Swansea, to introduce a new process into the copper-works, and for a trial at Hereford, which was put off. At the end of a fortnight he returned to London.

His letters to Mrs. Faraday, who went to Ramsgate, are full of affection, and the account of his "escape from the large mansion and high company" on the Sunday, and other passages, show how strongly religious feeling was at work in him.

Æt. 31 (1823).

Two papers this year were read to the Royal Society, and printed in the Transactions—one on Fluid Chlorine, the other on the Condensation of several Gases into Liquids; and he had four papers in the Quarterly Journal of Science—one on Hydrate of Chlorine, one on the Change of Musket-balls in Shrapnell Shells, on the Action of Gunpowder on Lead, on the purple tint of Plate-glass affected by Light. In a letter to Prof. G. De la Rive, March 24, he says :—"I have been at work lately, and obtained results which I hope you will approve of. I have been interrupted twice in the course of experiments by explosions, both in the course of eight days. One burnt my eyes, the other cut them, but I fortunately escaped with slight injury only in both cases, and am now nearly well. During the winter I took the opportunity of examining the hydrate of chlorine, and analyzing

it; the results, which are not very important, will appear in the next number of the Quarterly Journal (over which I have no influence). Sir H. Davy, on seeing my paper, suggested to me to work with it under pressure, and see what would happen by heat &c. Accordingly I enclosed it in a glass tube, hermetically sealed, heated it, obtained a change in the substance, and a separation into two different fluids; and upon further examination I found that the chlorine and water had separated from each other, and the chlorine gas, not being able to escape, had condensed into the liquid form. To prove that it contained no water, I dried some chlorine gas, introduced it into a long tube, condensed it, and then cooled the tube, and again obtained fluid chlorine. Hence what is called chlorine gas is the vapour of a fluid. I have written a paper, which has been read to the Royal Society, and to which the President did me the honour to attach a note, pointing out the general application and importance of this mode of producing pressure with regard to the liquefaction of gases. He immediately formed liquid muriatic acid by a similar means, and, pursuing the experiments at his request, I have since obtained sulphurous acid, carbonic acid, sulphuretted hydrogen, euchlorine, and nitrous oxide in the fluid state, quite free from water. Some of these require great pressure for this purpose, and I have had many explosions.

"I send you word of these results because I know your anxiety to hear of all that is new, but do not mention them publicly (or at least the latter ones, until you hear of them, either through the journals, or by another letter from me, or from other persons), because Sir Humphry Davy has promised the results in a paper to the Royal Society for me, and I know he wishes first to have them read there; after that they are at your service.

"I expect to be able to reduce many other gases to the liquid form, and promise myself the pleasure of writing you about them."

March 25, Monday, he writes to his friend Huxtable:—"I met with another explosion on Saturday evening, which has again laid up my eyes. It was from one of my tubes, and was so powerful as to drive the pieces of glass like pistol-shot through a window. However, I am getting better, and expect to see as well as ever in a few days. My eyes were filled with glass at first."

On May the 1st his certificate was read for the first time at the Royal Society:—

"Mr. Michael Faraday, a gentleman eminently conversant in chemical science, and author of several papers, which have been published in the Transactions of the Royal Society, being desirous of becoming a Fellow thereof, we, whose names are undersigned, do of our personal knowledge recommend him as highly deserving that honour, and likely to become a useful and valuable member."

Twenty-nine names follow; the first six were Wm. H. Wollaston, J. G. Children, Wm. Babington, Sir W. Herschel, J. South, Davies Gilbert. The

certificate had to be read at ten successive meetings before the ballot came on.

On the 30th of May he wrote to H. Warburton, Esq. :—"Sir, I have been anxiously waiting the opportunity you promised me of a conversation with you, and from late circumstances am now still more desirous of it than at the time when I saw you in the Committee. I am sure you will not regret the opportunity you will afford for an explanation; for I do not believe there is anything you would ask *after you have communicated with me*, that I should not be glad to do. I am satisfied that many of the feelings you entertain on the subject in question would be materially altered by granting my request. At the same time, as I have more of your opinions by report than otherwise, I am perhaps not well aware of them. It was only lately that I knew you had any feeling at all on the subject. You would probably find yourself engaged in doing justice to one who cannot help but feel that he has been injured, though he trusts unintentionally. I feel satisfied you are not in possession of all the circumstances of the case, but I am also sure you will not wish willingly to remain ignorant of them. Excuse my earnestness and freedom on this subject, and consider for a moment how much I am interested in it."

At the foot of the copy of this letter Faraday made the following notes :— "In relation to Davy's opposition to my election at the R. S. : Sir H. Davy angry, May 30 ; Phillips's report through Mr. Children, June 5 ; Mr. Warburton called first time, June 5, evening ; I called on Dr. Wollaston, and he not in town, June 9 ; I called on Dr. Wollaston and saw him, June 14 ; I called at Sir H. Davy's, and he called on me, June 17."

Many years ago he gave a friend the following facts, which were written down at the time: Sir H. Davy told him that he must take down his certificate. Faraday replied that he had not put it up : that he could not take it down as it was put up by his proposers. Sir Humphry then said, he must get his proposers to take it down. Faraday answered that he knew they would not do so. Then, said Sir H., I, as President will take it down. Faraday replied, that he was sure Sir Humphry Davy would do what he thought was for the good of the Society.

One of Faraday's proposers told him that Sir H. had walked for an hour round the courtyard of Somerset House, trying to convince Faraday's informant that Faraday ought not to be elected. However, the storm passed away, but not without leaving its effects; and on the 29th of June Sir H. Davy ends a note—"I am, dear Faraday, very sincerely, your well-wisher and friend."

July 8, Mr. Warburton wrote :—"I have read the article in the Royal Institution Journal, vol. xv. p. 288, on Electromagnetic Rotation, and without meaning to convey to you that I approve of it unreservedly, I beg to say that upon the whole it satisfies me, as I think it will Dr. Wollaston's other friends. Having everywhere admitted and maintained that, on the score of scientific merit, you were entitled to a place in the Royal Society, I

never cared to prevent your election, nor should I have taken any pains to form a party in private to oppose you. What I should have done would have been to take the opportunity, which the proposing to ballot for you would have afforded me, to make remarks in public on that part of your conduct to which I objected. Of this I made no secret, having intimated my intention to some of those from whom I knew you would hear of it, and to the President himself. When I meet with any of those in whose presence such conversation may have passed, I shall state that my objections to you as a Fellow are and ought to be withdrawn, and that I now wish to forward your election."

Aug. 29, Faraday writes to Mr. Warburton :—

"I thank you sincerely for your kindness in letting me know your opinion of the statement; though your approbation of it is not unreserved, yet it very far surpasses what I expected; and I rejoice that you do not now think me destitute of those moral feelings which you remarked to me were necessary in a Fellow of the Royal Society.

"Conscious of my own feelings and the rectitude of my intentions, I never hesitated in asserting my claims, or in pursuing that line of conduct which appeared to me to be right. I wrote the statement under this influence without any regard to the probable result; and I am glad that a step which I supposed would rather tend to aggravate feelings against me has, on the contrary, been the means of satisfying the minds of many, and of making them my friends. Two months ago I had made up my mind to be rejected by the Royal Society as a Fellow, notwithstanding the knowledge I had that many would do me justice; and in the then state of my mind rejection or reception would have been equally indifferent to me. Now that I have experienced so fully the kindness and liberality of Dr. Wollaston, which has been constant throughout the whole of this affair, and that I find an expression of goodwill strong and general towards me, I am delighted by the hope I have of being honoured by Fellowship with the Society; and I thank you sincerely for your promise of support in my election, because I know you would not give it unless you sincerely thought me a fit person to be admitted."

Faraday was the original Secretary of the Athenæum Club; but finding the occupation incompatible with his pursuits, resigned in May 1824. The original prospectus and early list of members have his name attached to them.

This year he was elected Corresponding Member of the Academy of Sciences, Paris, of the Accademia dei Georgofili di Firenze, Honorary Member of the Cambridge Philosophical Society and the British Institution.

Æt. 32 (1824).

Faraday was elected Fellow of the Royal Society, January 8th. This year he published only a historical statement in the Quarterly Journal of Science on the liquefaction of gases, showing that carbonic acid, ammonia,

arseniuretted hydrogen, chlorine, sulphurous acid had been liquefied before his own experiments in 1823. He joined Mr. Brande in the delivery of the morning course of chemical lectures at the Institution. In July he went to the Isle of Wight with Mrs. Faraday, and returned again in August to bring her home. He was elected an Honorary Member of the Cambrian Society of Swansea, and a Fellow of the Geological Society. This year the President and Council of the Royal Society appointed a committee for the improvement of glass for optical purposes, consisting of Fellows of the Royal Society and members of the then Board of Longitude.

Æt. 33 (1825).

Faraday was made Director of the Laboratory of the Royal Institution, and therein he had three or four evening meetings of the members of the Institution, from which came the Friday evening meetings of the members. He was elected a Member of the Royal Institution, and a Corresponding Member of the Society of Medical Chemists, Paris. He had a paper on new compounds of carbon and hydrogen, and on certain other products obtained during the decomposition of oil by heat, read to the Royal Society, and printed in the Transactions; one of these substances was benzol. He had a paper in the Quarterly Journal on some cases of the formation of ammonia, and on the means of testing the presence of minute portions of nitrogen in certain states.

In May a subcommittee, consisting of Mr. Herschel, Mr. Dollond, and Mr. Faraday, was appointed to have the direct superintendence and performance of experiments on the manufacture of optical glass. "It was my business to investigate particularly the chemical part of the inquiry. Mr. Dollond was to work and try the glass, and ascertain practically its good or bad qualities, whilst Mr. Herschel was to examine its physical properties, reason respecting their influence and utility, and make his competent mind bear upon every part of the inquiry. In March 1829 the committee was reduced to two by the retirement of Mr. Herschel, who about that period went to the Continent."

In July he left London by steamboat for Scotland. After visiting the damask works, he went to Leith to see the glass works. He minutely describes the geology of Salisbury Craig, Arthur's Seat, and Craigleith quarries, and then went to Rubislaw (Bleaching Liquor Works), Aberdeen. Here he made many experiments for the proprietors, with whom he stayed.

Æt. 34 (1826).

He had a paper on the Mutual Action of Sulphuric Acid and Naphthaline printed in the Philosophical Transactions, and another on the existence of a limit to Vaporization, and in the Quarterly Journal of Science four papers—on Pure Caoutchouc and the Substances by which it is accompanied in the state of Sap or Juice, on the Fluidity of Sulphur at common temperatures, on a peculiar perspective appearance of aërial light and shade, and on the confinement of Dry Gases over Mercury.

There were seventeen meetings of the members of the Royal Institution held on Friday evenings during this season, and at these Faraday gave seven discourses—on Pure Caoutchouc; on Brunel's Condensed Gas-engine; on Lithography; on the existence of a limit to Vaporization; on Sulphovinic and Sulphonaphthalic Acid; on Drummond's Light; on Brunel's Tunnel at Rotherhithe.

This year he was relieved from the duty of chemical assistant at the lectures given at the Institution, because of his occupation in research, and he was made an honorary member of the Westminster Medical Society.

In his chemical notes there is an analysis of "committee glass" and Saxony gunpowder, and remarks on calico printing and soap making, and soda from common salt.

In July he again was in the Isle of Wight.

Æt. 35 (1827).

Faraday gave his first course of lectures in the theatre of the Institution in April on Chemical Philosophy.

He writes:—"The President and Council of the Royal Society applied to the President and Managers of the Royal Institution for leave to erect on their premises an experimental room with a furnace, for the purpose of continuing the investigation on the manufacture of optical glass. They were guided in this by the desire which the Royal Institution has always evinced to assist in the advancement of science; and the readiness with which the application was granted showed that no mistaken notion had been formed in this respect. As a member of both bodies, I felt much anxiety that the investigation should be successful. A room and furnaces were built at the Royal Institution in September 1827, and an assistant was engaged, Sergeant Anderson of the Royal Artillery. He came on the 3rd of December."

He had four papers in the Quarterly Journal of Science:—1, on the Fluidity of Sulphur and Phosphorus at common temperatures. "In this," he says, "I published some time ago [the year previous] a short account of an instance of the existence of fluid sulphur at common temperatures; and though I thought the fact curious, I did not esteem it of such importance as to put more than my initials to the account. I have just learned through the 'Bulletin Universel' for September, p. 78, that Signor Bellani had observed the same fact in 1813, and published it in the 'Giornale di Fisica.' M. Bellani complains of the manner in which facts and theories which have been published by him are afterwards given by others as new discoveries; and though I find myself classed with Gay-Lussac, Sir H. Davy, Daniell, and Bostock, in having thus erred, I shall not rest satisfied without making restitution, for M. Bellani in this instance certainly deserves it at my hand." 2, on the probable decomposition of certain gaseous compounds of carbon and hydrogen during sudden expansion; 3, on transference of Heat by change of Capacity in Gas; and 4, Experiments on the Nature of Labarraque's

Disinfecting Soda Liquid. There were nineteen Friday evening meetings at the Royal Institution. Faraday gave an account of the magnetic phenomena developed by metals in motion, on the chemical action of chlorine and its compounds as disinfectants, and on the progress of the Thames tunnel. In this year he published his "Chemical Manipulations," in one volume, 8vo. A second edition appeared in 1830, and a third in 1842.

He was made a Correspondent of the Société Philomathique, Paris.

Æt. 36 (1828).

He had a few words in the Quarterly Journal on anhydrous crystals of sulphate of soda. He gave four of the Friday evening lectures: Illustrations of the new Phenomena produced by a current of Air or Vapour recently observed by M. Clement; on the reciprocation of Sound; and also a discourse on the Nature of Musical Sound. The matter belonged to Mr. Wheatstone, but was delivered by Mr. Faraday. The last evening was on the recent and present state of the Thames tunnel.

He was made a Fellow of the Natural Society of Science of Heidelberg.

He was invited to attend the meetings of the Board of Managers of the Institution; and he received his first (gold) medal, one of a series of ten given to Members of the Royal Institution (as a reward for chemical discoveries) by Mr. John Fuller, a Member.

Æt. 37 (1829).

He gave the Bakerian lecture at the Royal Society on the Manufacture of Glass for Optical purposes.

This most laborious investigation led to no good in the direction that was originally expected, but the use of the glass manufactured, as described afterwards, became of the utmost importance in his diamagnetic and magneto-optical researches, and it led to the permanent engagement, in 1832, of Mr. Charles Anderson as Faraday's assistant in all his researches, "to whose rare steadiness, exactitude, and faithfulness in the performance of all that was committed to his charge Faraday was much indebted."

He gave Friday evening discourses on Mr. Robert Brown's discovery of Active Molecules in bodies, either organic or inorganic; on Brard's test of the action of weather on building stones; on Wheatstone's further investigations on the resonances or reciprocal vibrations of volumes of air; on Brunel's block machinery at Portsmouth; on the phonical or nodal figures of elastic laminæ; on the manufacture of glass for optical purposes.

He was made a member of the Scientific Advising Committee of the Admiralty, Patron of the Library of the Institution, Honorary Member of the Society of Arts, Scotland.

At the end of June he writes to Colonel Drummond, Lieutenant-Governor of the Royal Academy, Woolwich:—"I should be happy to undertake the duty of lecturing on chemistry to the gentlemen cadets of Woolwich, provided that the time I should have to take for the purpose from professional

business at home were remunerated by the salary. . . . . For these reasons [which he gives] I wish you would originate the terms rather than I. . . . . I consider the offer a high honour, and beg you to feel assured of my sense of it. I should have been glad to have accepted or declined it, independent of pecuniary motives; but my time is my only estate, and that which would be occupied in the duty of the situation must be taken from what otherwise would be given to professional business."

At Christmas he for the first time gave the Juvenile Lectures.

Æt. 38 (1830).

This year he had a paper in the Institution Journal supplementary to his former paper in 1826 on the limits of vaporization.

His Friday evening discourses were on Aldini's proposed method of preserving men exposed to flame; on the Transmission of Musical sounds through solid conductors and their subsequent reciprocation; on the Flowing of Sand under Pressure; on the application of a New Principle in the Construction of Musical Instruments; on the laws of Coexisting Vibrations in strings and rods, illustrated by the kaleidophone.

The following recollections from about 1823 to 1830 are by Mrs. Faraday's youngest brother, Mr. George Barnard, the artist:—

"All the years I was with Harding I dined at the Royal Institution. After dinner we nearly always had our games just like boys—sometimes at ball, or with horse chestnuts instead of marbles, Faraday appearing to enjoy them as much as I did, and generally excelling us all. Sometimes we rode round the theatre on a velocipede (and tradition remains that in the earliest part of a summer morning Faraday has been seen going up Hampstead Hill on his velocipede).

"At this time we had very pleasant conversaziones of artists, actors, and musicians at Hullmandel's, sometimes going up the river in his eight-oar cutter, cooking our own dinner, enjoying the singing of Garcia and his wife and daughter (afterwards Malibran), indeed of all the best Italian singers, and the society of most of the Royal Academicians, such as Stanfield, Turner, Westall, Landseer, &c.

"After Hullmandel's excellent suppers, served on a dozen or two small tables in his large rooms, we had charades, Faraday and many of us taking parts with Garcia, Malibran, and the rest.

"My first and many following sketching trips were made with Faraday and his wife. Storms excited his admiration at all times, and he was never tired of looking into the heavens. He said to me once, 'I wonder you artists don't study the light and colour in the sky more, and try more for effect.' I think this quality in Turner's drawings made him admire them so much. He made Turner's acquaintance at Hullmandel's, and afterwards often had applications from him for chemical information about pigments. Faraday always impressed upon Turner and other artists the great necessity there was to experiment for themselves, putting washes and tints of all their pig-

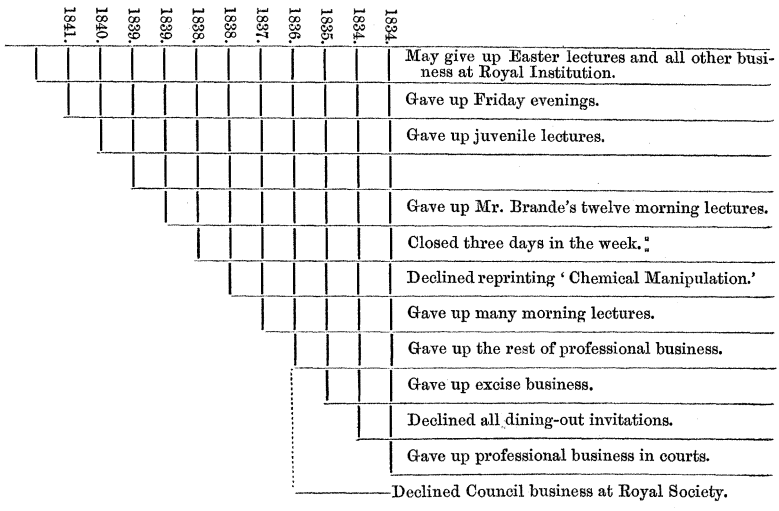
ments in the bright sunlight, covering up one half, and noticing the effect of light and gases on the other.

"On one of our sea-side excursions we were bathing together, when Faraday, who was a fair swimmer, on coming in was overtaken by a tremendous wave which overtopped his head, and dashed him with violence on the beach, bruising him much. He impressed on me never to think any one could stand against such a breaker; that one should turn round and dive through it, throwing one's self off the ground. Faraday did not fish at all during these country trips, but just rambled about geologizing or botanizing."

If Faraday's scientific life had ended here it might well have been called a noble success. He had made two leading discoveries, the one on electro-magnetic motions, the other on the condensation of several gases into liquids. He had carried out two important and most laborious investigations on the alloys of steel and on the manufacture of optical glass. He had made many communications to the Royal Society, and many more to the Quarterly Journal of Science. From assistant in the laboratory he had become its director. He was constantly lecturing in the great theatre, and he had probably prolonged the existence of the Royal Institution by taking the most active part in the establishment of the Friday evening meetings.

But when we turn to the eight volumes of manuscripts of his 'Experimental Researches,' which he bequeathed to the Royal Institution, we find that he was just going to begin to work. The first of these large folio volumes begins in 1831 with paragraph 1, and continues in the seventh to paragraph 15,389 in 1856. The results of this work he has collected himself in four volumes octavo. The three volumes on electricity were published in 1839, in 1844, and in 1855; the last volume, on chemistry and physics, he published in 1859. Whenever he was about to investigate a subject, he wrote out, on separate slips of paper, different queries regarding it which his genius made him think were "naturally possible" to be answered by experiment. He slightly fixed them one beneath another, in the order in which he intended to experiment. As a slip was answered it was removed, and others were added in the course of the investigation, and these in their turn were worked out and removed. If no answer was obtained, the slip remained to be returned to at another time. Out of the answers the manuscript volumes were formed, and from these the papers were written for the Royal Society, where they were always read before the popular account of them was given to the Royal Institution at a Friday evening meeting.

When nearly fifty years of age, he became so seriously troubled with want of memory and giddiness that he thought he should be unable to do any more, and in his most exact way he drew up the following table of the work he had given up temporarily during the first ten years that his experimental investigations in electricity had lasted:—



Æt. 39 (1831).

In this year the first series of 'Experimental Researches in Electricity' was read to the Royal Society. It contained experiments (1) on the Induction of Electric Currents, (2) on the Evolution of Electricity from Magnetism, (3) on a new Electrical Condition of Matter, and on Arago's Magnetic Phenomena. He had also in the Transactions a paper on a peculiar class of acoustical figures, and on certain forms assumed by groups of particles upon vibrating elastic surfaces. In the Quarterly Journal of Science he had a paper on a peculiar class of optical deceptions, which gave rise to the chromatrope.

He gave five Friday discourses on a peculiar class of Optical Deceptions; on Oxalamide, discovered by M. Dumas; on Light and Phosphorescence (being an account of experiments recently made in the Royal Institution by Mr. Pearsall, Chemical Assistant); on Trevelyan's recent Experiments on the production of Sound during the conduction of Heat; and on the Arrangements assumed by Particles upon Vibrating Elastic Surfaces.

He was elected an Honorary Member of the Imperial Academy of Sciences, Petersburg.

In a letter to his friend, Richard Phillips, he first complains of his memory. "My memory gets worse and worse daily, I will not therefore say I have not received your Pharmacopœia." Three months later he thanks him for the last edition of the Pharmacopœia, and says, "I am busy just now again on electro-magnetism, and think I have got hold of a good thing, but can't say. It may be a weed instead of a fish that, after all my labour, I may at last pull up. I think I know why metals are magnetic when in motion, though not (generally) when at rest."

Nov. 29.—Two months later he again writes, and this time from

Brighton:—"We are here to refresh. I have been working and writing a paper that always knocks me up in health, but now I feel well again and able to pursue my subject, and now I will tell you what it is about. The title will be, I think, 'Experimental Researches in Electricity':—I. On the Induction of Electric Currents; II. On the Evolution of Electricity from Magnetism; III. On a new Electrical Condition of Matter; IV. On Arago's Magnetic Phenomena. There is a bill of fare for you, and, what is more, I hope it will not disappoint you. Now the pith of all this I must give you very briefly, the demonstrations you shall have in the paper when printed.

"I. When an electric current is passed through one of two parallel wires, it causes at first a current in the same direction through the other, but this induced current does not last a moment, notwithstanding the inducing current (from the voltaic battery) is continued; all seems unchanged, except that the principal current continues its course. But when the current is stopped, then a return current occurs in the wire under induction, of about the same intensity and momentary duration, but in the opposite direction to that first formed. Electricity in currents therefore exerts an inductive action like ordinary electricity, but subject to peculiar laws. The effects are a current in the same direction when the induction is established, a reverse current when the induction ceases, and a *peculiar state* in the interim. Common electricity probably does the same thing; but as it is at present impossible to separate the beginning and the end of a spark or discharge from each other, all the effects are simultaneous and neutralize each other.

"II. Then I found that magnets would induce just like voltaic currents, and by bringing helices and wires and jackets up to the poles of magnets, electrical currents, were produced in them, these currents being able to deflect the galvanometer, or to make, by means of the helix, magnetic needles, or in one case even to give a spark. Hence the evolution of *electricity from magnetism*. The currents were not permanent; they ceased the moment the wires ceased to approach the magnet, because the new and apparently quiescent state was assumed just as in the case of the induction of current; but when the magnet was removed, and its induction therefore ceased, the return currents appeared as before. These two kinds of induction I have distinguished by the terms *volta-electric* and *magneto-electric* induction. Their identity of action and results is, I think, a very powerful proof of M. Ampère's theory of magnetism.

"III. The new electrical condition which intervenes by induction between the beginning and end of the inducing current gives rise to some very curious results. It explains why chemical action or other results of electricity have never been as yet obtained in trials with the magnet. In fact the currents have no sensible duration. I believe it will explain perfectly the *transference of elements* between the poles of the pile in decomposition; but this part of the subject I have reserved until the present experiments are com-

pleted; and it is so analogous, in some of its effects, to those of Ritter's secondary piles, De la Rive and Van Beck's peculiar properties of the poles of a voltaic pile, that I should not wonder if they all proved ultimately to depend on this state. The condition of matter I have dignified by the term *Electrotonic*, THE ELECTROTONIC STATE. What do you think of that? Am I not a bold man, ignorant as I am, to coin words? but I have consulted the scholars, and now for IV.

"IV. The new state has enabled me to make out and explain all Arago's phenomena of the rotating magnet or copper plate. I believe, perfectly; but as great names are concerned (Arago, Babbage, Herschel, &c.), and as I have to differ from them, I have spoken with that modesty which you so well know you and I and John Frost * have in common, and for which the world so justly commends us. I am even half afraid to tell you what it is. You will think I am hoaxing you, or else in your compassion you may conclude I am deceiving myself. However, you need do neither, but had better laugh, as I did most heartily, when I found that it was neither attraction nor repulsion, but just one of my *old rotations* in a new form. I cannot explain to you all the actions, which are very curious; but in consequence of the electrotonic state being assumed and lost as the parts of the plate whirl under the pole, and in consequence of magneto-electric induction, currents of electricity are formed in the direction of the radii,—continuing, for simple reasons, as long as the motion continues, but ceasing when that ceases. Hence the wonder is explained that the metal has powers on the magnet when moving, but not when at rest. Hence is also explained the effect which Arago observed, and which made him contradict Babbage and Herschel, and say the power was repulsive; but, as a whole, it is really tangential. It is quite comfortable to me to find that experiment need not quail before mathematics, but is quite competent to rival it in discovery; and I am amazed to find that what the high mathematicians have announced as the *essential condition* to the rotation, namely, that *time is required*, has so little foundation, that if the time could by possibility be anticipated instead of being required, *i. e.* if the currents could be formed *before* the magnet came over the place instead of *after*, the effect would equally ensue. Adieu, dear Phillips. Excuse this egotistical letter from yours, very faithfully."

Æt. 40 (1832).

The second series of Experimental Researches in Electricity was this year the Bakerian lecture on Terrestrial Magneto-electric Induction, and on the Force and Direction of Magneto-electric Induction generally.

His Friday discourses were, (1) on Dr. Johnson's Researches on the Reproductive Power of Planariæ; (2) recent experimental Investigation of Volta-electric and Magneto-electric Induction; (3) Magneto-electric In-

* A pushing acquaintance, who, without claim of any kind, got himself presented at Court.

duction, and the explanation it affords of Arago's Phenomena of Magnetism exhibited by moving Metals; (4) Evolution of Electricity, naturally and artificially, by the inductive action of the Earth's Magnetism; (5) on the Crispation of Fluids lying on vibrating Surfaces; and on Morden's Machinery for manufacturing Bramah's locks.

He was made Hon. Member of Philadelphia College of Pharmacy, and of Chemical and Physical Society, Paris; Fellow of the American Academy of Arts and Sciences, Boston; Member of the Royal Society of Science, Copenhagen; D.C.L. of Oxford University; and he received the Copley medal.

He collected the different papers, notes, notices, &c. published in octavo up to this year, and he added this preface to the volume:—"Papers of mine published in octavo in the Quarterly Journal of Science and elsewhere, since the time that Sir H. Davy encouraged me to write the 'Analysis of Caustic Lime.' Some I think (at this date) are good, others moderate, and some bad; but I have put *all* into the volume, because of the utility they have been to me, and none more than the bad in pointing out to me in future, or rather after times, the faults it became me to watch and avoid. As I never looked over one of my papers a year after it was written without believing, both in philosophy and manner, it would have been much better done, I still hope this collection may be of great use to me."

In December, the Royal Institution being in trouble, a committee reported on all the salaries. "The Committee are certainly of opinion that no reduction can be made in Mr. Faraday's salary, £100 per annum, house, coals, and candles, and beg to express their regret that the circumstances of the Institution are not such as to justify their proposing such an increase of it as the variety of duties which Mr. Faraday has to perform, and the zeal and ability with which he performs them, appear to merit."

Æt. 41 (1833).

The third series of Experimental Researches contained the Identity of Electricities derived from different sources, and the relation by measure of common and voltaic electricity. The fourth series consisted of a new law of Electric Conduction, and on Conducting-power generally. The fifth series was on Electro-chemical Decomposition, new conditions of Electro-chemical Decomposition, influence of Water in Electro-chemical Decomposition, and Theory of Electro-chemical Decomposition. The sixth series was on the Power of Metals and other Solids to induce the combination of gaseous bodies.

He sent a short note to the editors of the Philosophical Magazine on a means of preparing the Organs of Respiration so as considerably to extend the time of holding the breath, with remarks on its application in cases in which it is required to enter an irrespirable atmosphere, and on the precautions necessary to be observed in such cases.

His Friday discourses were on the Identity of Electricity derived from different sources; on the practical prevention of Dry Rot in Timber; on the investigation of the Velocity and Nature of the Electric Spark and Light by Wheatstone; on Mr. Brunel's new mode of constructing Arches for Bridges; on the mutual relations of Lime, Carbonic Acid, and Water; on a new law of Electric Conduction; and on the power of Platina and other solid substances to determine the combination of gaseous bodies.

In the early part of the year Mr. Fuller had founded a professorship of chemistry at the Royal Institution, with a salary of about £100 a year. Mr. Faraday was appointed for his life, with the privilege of giving no lectures. He was made Corresponding Member of the Royal Academy of Sciences of Berlin, and Hon. Member of the Hull Philosophical Society.

Æt. 42 (1834).

The seventh series of Experimental Researches was on Electro-chemical Decomposition (continued): on some general conditions of Electro-decomposition; on a new measure of Volta Electricity; on the Primary and Secondary character of bodies evolved in Electro-decomposition; on the definite nature and extent of Electro-chemical Decomposition; on the absolute quantity of Electricity associated with the Particles or Atoms of Matter.

The eighth series was on the Electricity of the Voltaic Pile, its source, quantity, and general characters; on simple Voltaic Circles; on the Intensity necessary for Electrolyzation; on associated Voltaic Circles on the Voltaic Battery; on the resistance of an Electrolyte to Electrolytic Action; general remarks on the active Voltaic Battery. The ninth series was on the influence by induction of an Electric Current on itself, and on the inductive action of Electric Currents generally.

He gave four Friday discourses, the first on the principle and action of Ericsson's Caloric engine. The other lectures were on Electro-chemical Decomposition; on the definite action of Electricity; and on new applications of the products of Caoutchouc.

He was made Foreign Corresponding Member of the Academy of Sciences and Literature of Palermo.

Æt. 43 (1835).

The tenth series of Experimental Researches was on an improved form of the Voltaic Battery, some practical results respecting the Construction and Use of the Voltaic Battery.

He gave Friday discourses on Melloni's recent discoveries on Radiant Heat; on the Induction of Electric Currents; on the Manufacture of Pens from Quills and Steel, illustrated by Morden's machinery; on the Condition and Use of the Tympanum of the Ear.

In July he went with Mrs. Faraday from Brighton to Dieppe, spending a week in Paris, and some days at Geneva; he stayed two days at Chamouni. He writes to his friend Magrath:—"We are almost surfeited with

magnificent scenery; and for myself I would rather not see it than see it with an exhausted appetite. The weather has been most delightful, and everything in our favour, so that the scenery has been in the most beautiful condition. Mont Blanc, above all, is wonderful, and I could not but feel, what I have often felt before, that painting is very far beneath poetry in cases of high expression, of which this is one. No artist should try to paint Mont Blanc, it is utterly out of his reach. He cannot convey an idea of it, and a formal map, or a common-place model, conveys more intelligence, even with respect to the sublimity of the mountain, than his highest efforts can do; in fact he must be able to dip his brush in light and darkness before he can paint Mont Blanc. Yet the moment one sees it Lord Byron's expressions come to mind, and they seem to apply. The poetry and the subject dignify each other."

On the 20th of April Sir James South wrote to him to say that he would have a letter from Sir Robert Peel acquainting him with the fact that, had Sir R. Peel remained in office, a pension would have been given him. On the 23rd he wrote a letter to Sir James South, which, however, his father-in-law prevented him from sending. He said, "I hope you will not think that I am unconscious of the good you meant me, or undervalue your great exertions for me, when I say that I cannot accept a pension whilst I am able to work for my living. Do not from this draw any sudden conclusion that my opinions are such and such. I think that Government is right in rewarding and sustaining science. I am willing to think, since such approbation has been intended me, that my humble exertions have been worthy, and I think that scientific men are not wrong in accepting the pensions; but still I may not take a pay which is not for services performed whilst I am able to live by my labours."

In the 'Times' of Saturday, 28th Oct. 1835, under the head of Tory and Whig Patronage to Science and Literature, is the following conversation, copied from Fraser's Magazine:—

"*Mr. F.* I am here, my Lord, by your desire; am I to understand that it is on the business which I have partially discussed with Mr. Young? (Lord M.'s Secretary.) *Lord Melbourne.* You mean the pension, don't you? *Mr. F.* Yes, my Lord. *Lord M.* Yes, you mean the pension, and I mean the pension too. I hate the name of the pension. I look upon the whole system of giving pensions to literary and scientific persons as a piece of gross humbug; it was not done for any good purpose, and never ought to have been done. It is a gross humbug from beginning to end. *Mr. F.* (rising, and making a bow). After all this, my Lord, I perceive that my business with your Lordship is ended. I wish you a good morning." Faraday said that the report of this conversation was full of error; however he wrote:—

"*To the Right Hon. Lord Viscount Melbourne, First Lord of the Treasury.*

"October 26.

"My Lord,—The conversation with which your Lordship honoured me

this afternoon, including, as it did, your Lordship's opinion of the general character of the pensions given of late to scientific persons, induces me respectfully to decline the favour which I believe your Lordship intends for me; for I feel that I could not, with satisfaction to myself, accept at your Lordship's hands that which, though it has the form of approbation, is of the character which your Lordship so pithily applied to it."

This note, Mr. F. says, "was left by myself, with my card, at Lord Melbourne's office on the same evening, *i. e.* of the day of our conversation."

On the 6th of November Faraday wrote to Sir James South:—

"And now, my dear Sir, pray let me drop . . . . . I know you have serious troubles of your own. Do not let me be one any longer either to you or to others. You have my most grateful feelings for all the kindness you have shown to him who is ever truly yours."

The intervention of Miss Fox and Lady Mary Fox, caused Lord Melbourne to write the following letter:—

"November 24.

"Sir,—It was with much concern that I received your letter declining the offer which I considered myself to have made in the interview which I had with you in Downing Street, and it was with still greater pain that I collected from that letter that your determination was founded upon the certainly imperfect, and perhaps too blunt and inconsiderate manner in which I had expressed myself in our conversation. I am not unwilling to admit that anything in the nature of censure upon any party ought to have been abstained from upon such an occasion; but I can assure you that my observations were intended only to guard myself against the imputation of having any political advantage in view, and not in any respect to apply to the conduct of those who had or hereafter might avail themselves of a similar offer. I intended to convey that, although I did not entirely approve of the motives which appeared to me to have dictated some recent grants, yet that your scientific character was so eminent and unquestionable as entirely to do away any objection which I might otherwise have felt, and to render it impossible that a distinction so bestowed could be ascribed to any other motive than a desire to reward acknowledged desert and to advance the interest of philosophy.

"I cannot help entertaining a hope that this explanation may be sufficient to remove any unpleasant or unfavourable impression which may have been left upon your mind, and that I shall have the satisfaction of receiving your consent to my advising His Majesty to grant to you a pension equal in amount to that which has been conferred upon Professor Airy and other persons of distinction in science and literature."

The same day Faraday wrote:—"My Lord, your Lordship's letter, which I have just had the honour to receive, has occasioned me both pain and pleasure—pain, because I should have been the cause of your Lordship's writing such a one, and pleasure, because it assures me that I am not unworthy of your Lordship's regard.

" As, then, your Lordship feels that, by conferring on me the mark of approbation hinted at in your letter, you will be at once discharging your duty as First Minister of the Crown, and performing an act consonant with your own kind feelings, I hesitate not to say I shall receive your Lordship's offer both with pleasure and with pride."

The pension was granted December 24, but in the interval he was much troubled by some, who thought that a contradiction to the injurious statement in the 'Times' against Lord Melbourne ought to be made.

To one Faraday writes:—"The pension is a matter of indifference to me, but other results, some of which have already come to pass, are not so. The continued renewal of this affair, to my mind, tempts me at times to what might be thought very ungenerous under the circumstances, namely, even at this late hour a determined refusal of the whole."

On the 8th of December he, however, published a letter in the 'Times,' in which he says, "I beg leave thus publicly to state that neither directly nor indirectly did I communicate to the Editor of Fraser's Magazine the information on which that article (an extract of which was published in the 'Times' of the 28th) was founded, or further, either directly or indirectly, any information to or for any publication whatsoever."

This year he was made Corresponding Member of the Royal Academy of Medicine, Paris; Hon. Member of the Royal Society of Edinburgh, Institution of British Architects, and Physical Society of Frankfort; Hon. Fellow of the Medico-Chirurgical Society of London; and he was awarded one of the Royal Medals by the Royal Society.

Æt. 44 (1836).

This year the whole course of Faraday's scientific work was changed by his appointment as Adviser to the Trinity House. He published one paper in the Philosophical Magazine on the general Magnetic Relations and Characters of the Metals, which he begins by saying, "general views have long since led me to an opinion, which is probably also entertained by others, though I do not remember to have met with it, that *all* the metals are magnetic in the same manner as iron."

He gave four Friday discourses on Silicified Plants and Fossils; on Magnetism of Metals as a general character; on Plumbago, and on Pencils, Morden's Machinery; and considerations respecting the nature of Chemical Elements.

The 3rd of February he wrote to Capt. Pelly, Deputy Master of the Trinity House:—

"I consider your letter to me as a great compliment, and should view the appointment at the Trinity Honse, which you propose, in the same light; but I may not accept even honours without due consideration.

"In the first place, my time is of great value to me, and if the appointment you speak of involved anything like periodical routine attendances, I do not think I could accept it. But if it meant that in consultation, in the

examination of proposed plans and experiments, in trials, &c. made as my convenience would allow, and with an honest sense of a duty to be performed, then I think it would consist with my present engagements. You have left the title and the sum in pencil. These I look at mainly as regards the character of the appointment; you will believe me to be sincere in this, when you remember my indifference to your proposition as a matter of interest, though *not as a matter of kindness.*

"In consequence of the goodwill and confidence of all around me I can at any moment convert my time into money, but I do not require more of the latter than is sufficient for necessary purposes. The sum therefore of £200 is quite enough in itself, but not if it is to be the indicator of the character of the appointment; but I think you do not view it so, and that you and I understand each other in that respect; and your letter confirms me in that opinion. The position which I presume you would wish me to hold is analogous to that of a standing counsel.

"As to the title, it might be what you pleased almost. Chemical adviser is too narrow; for you would find me venturing into parts of the philosophy of light not chemical. Scientific adviser you may think too broad (or in me too presumptuous); and so it would be, if by it was understood all science. It was the character I held with two other persons at the Admiralty Board in its former constitution.

"The thought occurs to me whether, after all, you want such a person as myself. This you must judge of; but I always entertain a fear of taking an office in which I may be of no use to those who engage me. Your applications are, however, so practical, and often so chemical, that I have no great doubt in the matter."

On the 4th he was made Scientific Adviser in experiments on lights to the Corporation.

For thirty years nearly he held this post. What he did may be seen in the portfolios, full of manuscripts, which Mrs. Faraday has given to the Trinity House, in which, by the marvellous order and method of his notes and indices, each particle of his work can be found and consulted immediately.

His first work was to make a photometer. Throughout the whole year he was busy on the subject, making three photometers, and ascertaining the capability and accuracy of the instruments. He also experimented on the preparation of oxygen for the Bude light, drawing up the most exact tables for the record of the manufacture; for example, the 10th of November he says, "hence oxygen costs very nearly twopence per cubical foot; exactly 1·909 pence."

He was made Senator of the University of London; Hon. Member of the Society of Pharmacy of Lisbon and of the Sussex Royal Institution; Foreign Member of the Society of Sciences of Modena, and the Natural-History Society of Basle.

Æt. 45 (1837).

This year the 'Eleventh Series of Experimental Researches in Electricity'

was communicated to the Royal Society. It was on Induction: Induction an action of contiguous particles; absolute charge of Matter; Electrometer and Inductive Apparatus employed; Induction in Curved Lines; Specific Inductive Capacity; general results as to Induction.

His work for the Trinity House consisted in examining the Trinity lamp, the French lamp, and the Bude lamp, as to intensity of light and price: "pressed Mr. Gurney, by letter, to give us his best lamp at once and not to lose time." Two of his four Friday discourses were on the views of Professor Mossotti as to one general law accounting for the different Forces in Matter; on Dr. Marshall Hall's views of the Nervous System.

He was elected Honorary Member of the Literary and Scientific Institution, Liverpool.

Æt. 46 (1838).

The twelfth series of Researches was published this year.—On Induction (continued): Conduction or Conductive Discharge; Electrolytic Discharge; Disruptive Discharge, Insulation, Spark, Brush, Difference of Discharge at the positive and negative surfaces of conductors. The thirteenth series was also on Induction (continued): Disruptive Discharge (continued). Peculiarities of positive and negative discharge either as spark or brush; Glow Discharge; Dark Discharge. Convection or Carrying Discharge. Relation of a vacuum to Electrical Phenomena. Nature of the Electrical Current. The fourteenth series was on the nature of the Electric Force or Forces. Relation of the Electric and Magnetic Forces, and notes on Electrical Excitation. The fifteenth series was a notice of the character and direction of the Electric Force of the Gymnotus.

For the Trinity House he a second time reported on the new Gurney lamp, comparing it in light and cost with the French lamp.

He gave four Friday discourses this year.

He was made Honorary Member of the Institution of Civil Engineers; Foreign Member of the Royal Academy of Sciences, Stockholm; and he received the Copley Medal.

Æt. 47 (1839).

At the end of July he was four days at Orfordness for the Trinity House, measuring and comparing at sea and on land the Argand lamp, the French lamp, and the Bude lamp.

He gave four Friday discourses, two of which were on the Electric powers of the Gymnotus and Silurus. An account of Gurney's oxv-oil-lamp.

During thirteen years, Miss Reid, a niece of Mrs. Faraday s, had lived at the Institution, and she has thus given her recollections of Mr. Faraday during these and the following six years:—

"There could be very few regular lessons at the Institution; there were so many breaks and interruptions. Sometimes my uncle would give me a few sums to do, and he always tried to make me understand the why and wherefore of everything I did. Then occasionally he gave me a reading-lesson. How patient he was, and how often he went over and over the

same passage when I was unusually dense. He had himself taken lessons from Smart, and he used to practise reading with exaggerated emphasis occasionally.

"In the earlier days of the juvenile lectures he used to encourage me to tell him everything that struck me, and where my difficulties lay when I did not understand him fully. In the next lecture he would enlarge on those especial points, and he would tell me my remarks had helped him to make things clear to the young ones. He never mortified me by wondering at my ignorance, never seemed to think how stupid I was. I might begin at the very beginning again and again; his patience and kindness were unfailing.

"A visit to the laboratory used to be a treat when the busy time of the day was over.

"We often found him hard at work on experiments connected with his researches, his apron full of holes. If very busy he would merely give a nod, and aunt would sit down quietly with me in the distance, till presently he would make a note on his slate and turn round to us for a talk, or perhaps he would agree to come upstairs to finish the evening with a game at bagatelle, stipulating for half an hour's quiet work first to finish his experiment. He was fond of all ingenious games, and he always excelled in them. For a time he took up the Chinese puzzle, and, after making all the figures in the book, he set to work and produced a new set of figures of his own, neatly drawn, and perfectly accurate in their proportions, which those in the book were not. Another time, when he had been unwell, he amused himself with *Papyro-plastics*, and with his dexterous fingers made a chest of drawers and pigeon-house, &c.

"When dull and dispirited, as sometimes he was to an extreme degree, my aunt used to carry him off to Brighton, or somewhere, for a few days, and they generally came back refreshed and invigorated. Once they had very wet weather in some out of the way place, and there was a want of amusement, so he ruled a sheet of paper and made a neat draught-board, on which they played games with pink and white lozenges for draughts. But my aunt used to give up almost all the games in turn, as he soon became the better player, and, as she said, there was no fun in being always beaten. At bagatelle, however, she kept the supremacy, and it was long a favourite, on account of its being a cheerful game requiring a little moving about.

"Often of an evening they would go to the Zoological Gardens and find interest in all the animals, especially the new arrivals, though he was always much diverted by the tricks of the monkeys. We have seen him laugh till the tears ran down his cheeks as he watched them. He never missed seeing the wonderful sights of the day—acrobats and tumblers, giants and dwarfs; even Punch and Judy was an unfailing source of delight, whether he looked at the performance or at the admiring gaping crowd.

"He was very sensitive to smells; he thoroughly enjoyed a cabbage

rose, and his friends knew that one was sure to be a welcome gift. Pure Eau de Cologne he liked very much; it was one of the few luxuries of the kind that he indulged in; musk was his abhorrence, and the use of that scent by his acquaintance annoyed him even more than the smell of tobacco, which was sufficiently disagreeable to him. The fumes from a candle or oil-lamp going out would make him very angry. On returning home one evening, he found his rooms full of the odious smell from an expiring lamp; he rushed to the window, flung it up hastily, and brought down a whole row of hyacinth-bulbs and flowers and glasses.

"Mr. Magrath used to come regularly to the morning lectures, for the sole purpose of noting down for Mr. F. any faults of delivery or defective pronunciation he could detect. The list was always received with thanks; although his corrections were not uniformly adopted, he was encouraged to continue his remarks with perfect freedom. In early days he always lectured with a card before him with *Slow* written upon it in distinct characters. Sometimes he would overlook it and become too rapid; in this case Anderson had orders to place the card before him. Sometimes he had the word 'Time' on a card brought forward when the hour was nearly expired."

Æt. 48 (1840).

Early in this year the sixteenth series of Experimental Researches appeared. It was on the Source of Power in the Voltaic Pile:—1. Exciting electrolytes, &c., being conductors of thermo and feeble currents; 2. Inactive Conducting Circles containing an electrolytic fluid; 3. Active Circles excited by solution of Sulphuret of Potassium. The seventeenth series came a few days after. Also on the Source of Power in the Voltaic Pile (continued): 4. The exciting Chemical Force by temperature; 5. The exciting Chemical Force affected by dilution; 6. Differences in the Order of the Metallic Elements of Voltaic Circles; 7. Active Voltaic Circles and Batteries without metallic contact; 8. Considerations of the sufficiency of chemical action; 9. Thermoelectric evidence; 10. Improbable nature of the assumed Contact Force.

He gave three Friday discourses.

The previous year, Dr. Hare, Professor of Chemistry in the University of Pennsylvania, wrote his objections to Faraday's theoretical opinions on Static Induction. At the end of Faraday's reply, he says:—"The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon their consideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts to hold my judgment as yet in suspense. It is indeed to me an annoying matter to find how many subjects there are in electrical science on which, if I were asked for an opinion, I should have to say I

cannot tell—I do not know; but, on the other hand, it is encouraging to think that these are they which, if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles. But on this question, as on many others, I have not yet made up my mind."

On the 1st of January the following year, Dr. Hare sent a reply. In Faraday's answer to this, he says:—"You must excuse me, however, for several reasons, from answering it at any length. The first is my distaste for controversy, which is so great that I would on no account our correspondence should acquire that character. I have often seen it do great harm, and yet remember few cases in natural knowledge where it has helped much either to pull down error or advance truth. Criticism, on the other hand, is of much value; and when criticism such as yours has done its duty, then it is for other minds than those either of the author or critic to decide upon and acknowledge the right."

This year he reported to the Trinity House on the necessity and method of examining lighthouse dioptric arrangements, and he had to examine the apparatus intended for Gibraltar. Between Purfleet and Blackwall he made a long comparison between English and French reflecting lamps and between English and French refracting prisms.

To Professor Auguste De la Rive, the son of his early friend, he wrote:—"Though a miserable correspondent I take up my pen to write to you, the moving feeling being a desire to congratulate you on your discernment, perseverance, faithfulness, and success in the cause of *Chemical Excitement of the current in the Voltaic Battery*. You will think it is rather late to do so; but not under the circumstances. For a long time I had not made up my mind; then the facts of definite electrochemical action made me take part with the supporters of the chemical theory, and since then Marianini's paper with reference to myself has made me read and experiment more generally on the point in question. In the reading, I was struck to see how soon, clearly, and constantly you had and have supported that theory, and think your proofs and reasons most excellent and convincing. The constancy of Marianini and of many others on the opposite side made me, however, think it not unnecessary to accumulate and record evidence of the truth, and I have therefore written two papers, which I shall send you when printed, in which I enter under your banners as regards the origin of electricity or of the current in the pile. My object in experimenting was, as I am sure yours has always been, not so much to support a given theory as to learn the natural truth; and having gone to the question unbiassed by any prejudices, I cannot imagine how any one whose mind is not preoccupied by a theory, or a strong bearing to a theory, can take part with that of contact against that of chemical action. How-

ever, I am perhaps wrong saying so much, for, as no one is infallible, and as the experience of past times may teach us to doubt a theory which seems to be most unchangeably established, so we cannot say what the future may bring forth in regard to these views."

He was made Member of the American Philosophical Society, Philadelphia, and Honorary Member of the Hunterian Medical Society, Edinburgh.

He was in the autumn of this year ordained Elder in the Sandemanian Church, and he held the office three years and a half.

Æt. 49 (1841).

On the 2nd of September Faraday went down to St. Catherine's lighthouse in the Isle of Wight, to remedy the condensation of moisture on the glass in the inside. On the 6th he returned home, "quite satisfied with the chimney, and have no doubt we shall have a lantern quite clear from sweat, and also much cleaner, both as to the mirrors and roof, from soot and blackness, than heretofore."

The 30th of June he left London for three months, with Mrs. Faraday and Mr. and Mrs. George Barnard, for Ostend and Switzerland. The journal which he kept contains many most beautiful descriptions. That of Brientz Lake and the Giessbach is perhaps one of the most striking:—"George and I crossed the lake in a boat to the Giessbach, he to draw and I to saunter. The day was fine, but the wind against the boat; and these boats are so cumbrous, and at the same time expose so much surface to the air, that we were about two hours doing the two miles, with two men and occasionally our own assistance at the oar. We broke the oar-band; we were blown back and sideways. We were drawn against the vertical rock in a place where the lake is nearly 1000 feet deep; and I might tell a true tale, which would sound very serious, yet after all there was nothing of any consequence but delay. But such is the fallacy of description. We reached the fall and found it in its grandeur; for, as much rain fell last night, there was perhaps half as much more water than yesterday. This most beautiful fall consists of a fine river, which passes by successive steps down a very deep precipice into the lake. In some of these steps there is a clear leap of water 100 feet or more; in others, most beautiful combinations of leap, cataract, and rapid—the finest rocks occurring at the sides and bed of the torrent. In one part a bridge passes over it; in another a cavern and path occur under it. To-day every fall was foaming from the abundance of water, and the current of wind brought down by it was in some parts almost too strong to stand against. The sun shone brightly, and the rainbows seen from various parts were very beautiful. One at the bottom of a fine but furious fall was very pleasant; there it remained motionless, whilst the gusts and clouds of spray swept furiously across its place and were dashed against the rock. It looked like a spirit strong in faith and steadfast in the midst of the storm of passions sweeping across it; and

though it might fade and revive, still it held on to the rock as in hope and giving hope, and the very drops which, in the whirlwind of their fury, seemed as if they would carry all away, were made to revive it and give it greater beauty. How often are the things we fear and esteem as troubles made to become blessings to those who are led to receive them with humility and patience! In one part of the fall the effect of the current of air was very curious. The great mass of water fell into a foaming basin, but some diverted portions struck the rock opposite the observer, and, collecting, left it at the various projecting parts; but, instead of descending, these hundred little streams rushed upwards into the air, as if urged by a force the reverse of gravity; and as there was little other spray in this part, it did not at first occur to the mind that this must be the effect of a powerful current of air, which, having been brought down by the water, was returning up that face of the rock."

Into the pages of this journal he has fixed, with the most extreme neatness, the different mountain-flowers that he gathered in his walks.

Mrs. Faraday wrote for him part of a letter to Mr. Magrath:—"I think Mr. Young would be quite satisfied with the way my husband employs his time. He certainly enjoys the country exceedingly; and though at first he lamented our absence from home and friends very much, he seems now to be reconciled to it as a means of improving his general health. His strength is, however, very good. He thinks nothing of walking thirty miles in a day, and one day he walked forty-five, which I protested against his doing again, though he was very little the worse for it. I think that is too much. What would Mr. Young say to that; but the grand thing is rest and relaxation of mind, which he is really taking." He finishes the letter himself:—"Though my wife's letter will tell you pretty well all about us, yet a few lines from an old friend (though somewhat worn out) will not be unpleasant to one who, like that friend, is a little the worse for time and hard wear. However, if you jog on as well as we do, you will have no cause for grumbling, by which I mean to say that I certainly have not; for the comforts that are given me, and, above all, the continued kindness, affection, and forbearance of friends towards me, are, I think, such as few experience. . . . . Remember me most kindly to Mr. Young. I will give no opinion at present as to the effect of his advice on my health and memory; but I can have only one feeling as to his kindness, and, whatever I may forget, I think I shall not forget that. . . . . Now, as to the main point of this trip, *i. e.* the mental idleness, you can scarcely imagine how well I take to it, and what a luxury it is. The only fear I have is that when I return friends will begin to think that I shall overshoot the mark; for feeling that any such exertion is a strain upon that faculty, which I cannot hide from myself is getting weaker, namely, memory, and feeling that the less exertion I make to use that the better I am in health and head, so my desire is to remain indolent, mentally speaking, and to retreat from a position which should only be held by one who has the *power* as

well as the will to be active. All this, however, may be left to clear itself up as the time proceeds."

Æt. 50 (1842).

He resumed the Friday evening lectures, and gave one on the Conduction of Electricity in Lightning-rods, and one on the Principles and Practice of Hullmandel's Lithotint. This year he made four reports to the Trinity House :—1, on comparison of the amount of Light cut off by French glass and by Newcastle glass ; 2, on a new Mode of suspending the Mirrors ; 3, its application to the Lundy Lighthouse, so as to save light ; 4, a Report on the Ventilation of the Tynemouth Light ; and he went to see the operation of the grinding-apparatus for lenses at Newcastle.

To Dr. T. M. Browne, who had asserted the isomerism of carbon and silicon, and who asked Faraday to witness his experiments and give him a written testimonial if they were satisfactory, he writes :—"That which made me inaccessible to you makes me so in a very great degree to all my friends—*ill health connected with my head* ; and I have been obliged, and I am still, to lay by nearly all my own pursuits, and to deny myself the pleasure of society, either in seeing myself in my friends' houses or them here. This alone would prevent me from acceding to your request. I should, if I assented, do it against the strict advice of my friends, medical and social.

"The matter of your request makes me add a word or two, which I hope you will excuse. Any one who does what you ask of me, *i. e.* certify if the experiment is successful, is bound, without escape, to certify and publish also *if it fail* ; and I think you may consider that very few persons would be willing to do this. I certainly would not put myself in such a most unpleasant condition."

This year he was made Chevalier of the Prussian Order of Merit (one of thirty), and Foreign Associate of the Royal Academy of Sciences, Berlin.

Æt. 51 (1843).

Early this year he sent the eighteenth series of his 'Researches' to the Royal Society. It was on the electricity evolved by the friction of water and steam against other bodies. This had been first observed by Sir W. Armstrong, and was attributed to evaporation, and was thought to be related to atmospheric electricity. He concluded, "the cause being, I believe, friction, has no effect in producing, and is not connected with, the general electricity of the atmosphere."

He read a paper at the Institution of Civil Engineers on the ventilation of lighthouse lamps, the points necessary to be observed, and the manner in which these have been, or may be, attained.

He gave three Friday discourses on some Phenomena of Electric Induction ; on the Ventilation of Lamp-burners, and on the Electricity of Steam.

For the Trinity House he went to the South Foreland lighthouses re-

garding their ventilation. He inspected the dioptric light of the first order, which had just been constructed in France and put up by French workmen, and compared its consumption of oil with the 15 Argand burners which were previously in use.

He sent to the Philosophical Magazine a paper on Static Electrical Inductive Action. Among his notes the following occurs :—"Propose to send to the Phil. Mag. for consideration the subject of a bar, or circular, or spherical magnet—first, in the strong magnetic field; then charged by it; and, finally, taken away and placed in space. Inquire the disposition of the dual force, the open or the related powers of the poles externally, and if they can exist unrelated. The difference between the state of the power, when related and when not, consistent with the conservation of force. Avoid any particular language. Should not pledge myself to answer any particular observations, or to any one, against open consideration of the subject. Want to direct the thoughts of all upon the subject, and to tie it there; and especially to gather for myself thought on the point of relation or non-relation of the antithetical force or polarities."

He was made Honorary Member of the Literary and Philosophical Society of Manchester, and Useful Knowledge Society, Aix la Chapelle.

Æt. 52 (1844).

He communicated to the Royal Society a paper on the Liquefaction and Solidification of Bodies generally existing as Gases. His object was to subject the gases to considerable pressure, with considerable depression of temperature. Though he did not condense oxygen, hydrogen, or nitrogen, the original objects of his pursuit, he added six substances, usually gaseous, to the list of those that could previously be shown in the liquid state, and he reduced seven, including ammonia, nitrous oxide, and sulphuretted hydrogen, into the solid form.

He sent to the Philosophical Magazine a speculation touching electric conduction and the nature of matter. Elsewhere he calls this "a speculation respecting that view of the nature of matter which considers its ultimate atoms as centres of force, and not as so many little bodies surrounded by forces, the bodies being considered in the abstract as independent of the forces, and capable of existing without them. In the latter view these little particles have a definite form and a certain limited size. In the former view such is not the case; for that which represents size may be considered as extending to any distance to which the lines of force of the particle extend. The particle, indeed, is supposed to exist only by these forces, and where they are it is."

This was the subject of his first Friday discourse. He also gave the last discourse on recent improvements in the Manufacture and Silvering of Mirrors.

For the Trinity House he only examined different cottons for the lamps.

In October he was sent by Sir James Graham with Mr. Lyell to attend the inquest on those who had died by the explosion in the Haswell colliery.

The following account is by Sir Charles :—

"Faraday undertook the charge with much reluctance, but no sooner had he accepted it than he seemed to be quite at home in his new vocation. He was seated near the coroner, and cross-examined the witnesses with as much talent, skill, and self-possession as if he had been an old practitioner at the bar. We spent eight hours, not without danger, in exploring the galleries where the chief loss of life had been incurred. Among other questions, Faraday asked in what way they measured the rate at which the current of air flowed in the mine. An inspector took a small pinch of gunpowder out of a box, as he might have taken a pinch of snuff, and allowed it to fall gradually through the flame of a candle which he held in the other hand. His companion, with a watch, marked the time the smoke took going a certain distance. Faraday admitted that this plan was sufficiently accurate for their purpose ; but, observing the somewhat careless manner in which they handled their powder, he asked where they kept it. They said they kept it in a bag, the neck of which was tied up tight. But where, said he, do you keep the bag? you are sitting on it was the reply ; for they had given this soft and yielding seat, as the most comfortable one at hand, to the Commissioner. He sprang up on his feet, and, in a most animated and expressive style, expostulated with them for their carelessness, which, as he said, was especially discreditable to those who should be setting an example of vigilance and caution to others who were hourly exposed to the danger of explosions. . . . . Hearing that a subscription had been opened for the widows and orphans of the men who had perished by the explosion, I found, on inquiry, that Faraday had already contributed largely. On speaking to him on the subject, he apologized for having done so without mentioning it to me, saying that he did not wish me to feel myself called upon to subscribe because he had done so."

To a lady of the highest talent, who proposed to become his disciple, to go through with him all his own experiments, he wrote :—"That I should rejoice to aid you in your purpose you cannot doubt, but nature is against you. You have all the confidence of unbaulked health and youth, both in body and mind. I am a labourer of many years' standing, made daily to feel my wearing out. You, with increasing acquisition of knowledge, enlarge your views and intentions. I, though I may gain from day to day some little maturity of thought, feel the decay of powers, and am constrained to a continual process of lessening my intentions and contracting my pursuits. Many a fair discovery stands before me in thought which I once intended, and even now desire, to work out; but I lose all hope respecting them when I turn my thoughts to that one which is in hand, and see how slowly, for want of time and physical power, it advances, and how likely it is to be not only a barrier between me and the many beyond in intellectual

view, but even the last upon the list of those practically wrought out. Understand me in this; I am not saying that my mind is wearing out, but those physico-mental faculties by which the mind and body are kept in conjunction and work together, and especially the memory, fail me, and hence a limitation of all I was once able to perform with a much smaller extent than heretofore. It is this which has had a great effect in moulding portions of my later life, has tended to withdraw me from the communion and pursuits of men of science my cotemporaries, has lessened the number of points of investigation (that might at some time have become discoveries) which I now pursue, and which, in conjunction with its effects, makes me say most unwillingly that I dare not undertake what you propose—to go with you through even my own experiments. You do not know, and should not now but that I have no concealment on this point from you, how often I have to go to my medical friend to speak of giddiness and aching of the head, and how often he has to bid me cease from restless thoughts and mental occupation and retire to the seaside to inaction. You speak of religion, and here you will be sadly disappointed in me. You will perhaps remember that I guessed, and not very far aside, your tendency in this respect. Your confidence in me claims in return mine to you, which, indeed, I have no hesitation to give on fitting occasions; but these I think are very few, for in my mind religious conversation is generally in vain. There is no philosophy in my religion. I am of a very small and despised sect of Christians, known, if known at all, as *Sandemanians*, and our hope is founded on the faith that is in Christ. But though the natural works of God can never by any possibility come in contradiction with the higher things which belong to our future existence, and must with everything concerning him ever glorify him, still I do not think it at all necessary to tie the study of the natural sciences and religion together; and in my intercourse with my fellow creatures that which is religious and that which is philosophical have ever been two distinct things."

In answer to Mr. Magrath, who sent him, from the 'Journal des Débats,' notice of his election as one of the eight foreign associates of the Academy of Sciences, Paris, he said:—"I received by this morning's post notice of the event in a letter from Dumas, who wrote from the Academy at the moment of the deciding the ballot, and, to make it more pleasant, Arago directed it on the outside."

He was also made Honorary Member of the Sheffield Scientific Society.

Æt. 53 (1845).

This year produced the nineteenth series of Researches on the Magnetization of Light and the Illumination of Magnetic Lines of Force:—1. Action of Magnets on Light; 2. Action of Electric Currents on Light; 3. General considerations. Also the twentieth series, on new Magnetic Actions, and on the Magnetic Conditions of all Matter:—1. Apparatus required; 2. Action of magnets on heavy glass; 3. Action of Magnets on other substances act-

ing magnetically on light; 4. Action of Magnets on the Metals generally. And the twenty-first series, on new Magnetic Actions, and on the Magnetic Condition of all Matter (continued): 5. Action of Magnets on the Magnetic Metals and their compounds; 6. Action of Magnets on Air and Gases; 7. General considerations.

For the Trinity House he made a long and exact comparison of the consumption and light of sperm and rape-oil. He gave a Friday discourse on the Condition and Ventilation of the Coal-mine Goaf, and another on the liquefaction and solidification of bodies usually gaseous; another on anastatic painting, and on the Artesian well in Trafalgar Square.

Early in the year he thus wrote to Prof. Auguste De la Rive:—"I have waited and waited for a result, intending to write off to you on the instant, and hoping by that to give a little value to my letter, until now, when the time being gone and the result not having arrived, I am in a worse condition than ever; and the only value my letter can have will be in the kindness with which you will receive it. The result I hoped for was the condensation of oxygen; but though I have squeezed him with a pressure of 60 atmospheres at the temperature of 140° F. below 0°, he would not settle down into the liquid or solid state; and now, being tired and ill and obliged to prepare for lectures, I must put the subject aside for a little while.

"Nitrogen is certainly a strange body. It encourages every sort of guess about its nature and will satisfy none. I have been trying to look at it in the condensed state, but as yet it escapes me.

"I thank you most truly, not only for the invitation (to the scientific meeting) you have sent me, but for all the favour you would willingly show me. Do you remember one hot day (I cannot tell how many years ago) when I was hot and thirsty in Geneva, and you took me to your house in the town and gave me a glass of water and raspberry vinegar? That glass of drink is refreshing to me still."

Late in the year he writes to M. De la Rive:—"I have had your last letter by me for several weeks intending to answer it, but absolutely I have not been able; for of late I have shut myself up in my laboratory and wrought to the exclusion of everything else..... I am still so involved in discovery that I have hardly time for my meals, and am here at Brighton both to refresh and work my head at once; and I feel that unless I had been here and been careful I could not have continued my labours. The consequence has been that last Monday I announced to our members at the Royal Institution another discovery, of which I will give you the pith.

"Many years ago I worked upon optical glass, and made a vitreous compound of silica, boracic acid, and lead, which I will now call heavy glass. It was this substance that enabled me first to act upon light by magnetic and electric forces. Now, if a square bar of this substance, about half an inch thick and two inches long, be very freely suspended between the poles of a powerful horseshoe electromagnet, immediately that the magnetic force is developed, the bar points, but it does not point from pole to pole, but equa-

torially or across the magnetic lines of force, *i. e.* east and west in respect of the north and south poles. If it be moved from this position it returns to it, and this continues as long as the magnetic force is in action. This effect is the result of a still simpler action of the magnet on the bar than what appears by the experiment, and which may be obtained at a single magnetic pole. For if a cubical or rounded piece of the glass be suspended by a fine thread 6 or 8 feet long, and allowed to hang very near a strong magneto-electric pole (not as yet made active), then, on rendering the pole magnetic, the glass will be repelled until the magnetism ceases. This effect and power I have worked out through a great number of its forms and strange consequences, and they will occupy two series of the 'Experimental Researches.' It belongs *to all matter* (not magnetic as iron) without exception; so that every substance belongs to one or the other class of magnetic or diamagnetic bodies. The law of action in its simplest form is that such matter tends to go from strong to weak points of magnetic force, and in doing this the substance will go in either direction along the magnetic curves, or in either direction across them. It is curious that amongst the metals are found bodies possessing this property in as high a degree as perhaps any other substance; in fact I do not know at present whether heavy glass, or bismuth, or phosphorus is the most striking in this respect."

In July he went with Mrs. Faraday and Mr. G. Barnard to France for three weeks, partly to inspect the lighthouses at Fecamp, Havre, Harfleur, and Cap de la Haye. His chief object was to be received into the Academy. At the same time he gained all the information he could regarding French lighthouses from M. H. Le Ponte and M. Fresnel. M. Dumas was his most constant companion in his visits to Chevreul, Milne-Edwards, Biot, Arago, the Well of Grenelle, and the water-works at Chaillot. On the 30th of July he went to the Institute. "Many of the members were gone out of town, but all that were there received me very kindly. I was glad to see Thenard, Dupuis, Flourens, Biot, Dumas of course, and Arago, Elie de Beaumont, Poinsot, Babinet, and a great many others whose names and faces sadly embarrassed my poor head and memory. Chatting together, Arago told me he was my senior, being born in 1786, and consequently 59 years of age."

He finishes his journal thus:—"We left George at the London Bridge Station; thanks be to him for all his kind care and attention on the journey, which is better worth remembering than anything else of all that which occurred in it."

He was made Corresponding Member of the National Institute, Washington, and of the Société d'Encouragement, Paris.

Æt. 54 (1846).

Early in the year he gave a Friday discourse on the relation of Magnetism and Light, and another on the Magnetic Condition of Matter, and, later in

the season, another on Wheatstone's Electro-magnetic Chronoscope, at the end of which he said he was induced to utter a speculation long on his mind, and constantly gaining strength, viz. that perhaps those vibrations by which radiant agencies, such as light, heat, actinic influence, &c., convey this force through space, are not vibrations of an ether, but of the lines of force which, in his view, equally connect the most distant masses together and make the smallest atoms or particles by their properties influential on each other and perceptible to us. A little later he sends these views to the Philosophical Magazine as thoughts on ray vibrations; "but, from first to last, understand that I merely throw out, as matter for speculation, the vague impressions of my mind; for I give nothing as the result of sufficient consideration or as the settled conviction, or even probable conclusion, at which I had arrived." His last Friday discourse was on the Cohesive Force of Water.

He reported to the Trinity House on drinking-water of the Smalls Lighthouse, and on a ventilation apparatus for rape-oil lamps.

To the Secretary of the Institution, who consulted him regarding evening lectures, he said, "I see no objection to evening lectures if you can find a fit man to give them. As to popular lectures (which at the same time are to be *respectable* and *sound*), none are more difficult to find. Lectures which *really teach* will never be popular; lectures which are popular will never *really teach*. They know little of the matter who think science is more easily to be taught or learned than A B C; and yet who ever learned his A B C without pain and trouble? Still lectures can (generally) inform the mind and show forth to the attentive man what he really has to learn, and in their way are very useful, especially to the public. I think they might be useful to us now, even if they only gave an answer to those who, judging by their own earnest desire to learn, think much of them. As to agricultural chemistry, it is no doubt an excellent and a popular subject; but I rather suspect that those who know least of it think that most is known about it."

He received both the Rumford and a Royal Medal, and was made Honorary Member of the Society of Sciences, Vaud.

Æt. 55 (1847).

He gave Friday discourses on the Combustion of Gunpowder; on Mr. Barry's mode of ventilating the New House of Lords; and on the Steam-jet chiefly as a means of procuring ventilation.

He reported to the Trinity House on the ventilation of the South Foreland lights, and on a proposal to light buoys by platinum wire ignited by electricity.

He writes to the First Lord of the Admiralty from Edinburgh:—"For years past my health has been more and more affected; and the place affected is my head. My medical advisers say it is from mental occupation. The result is loss of memory, confusion, and giddiness; the sole

remedy, cessation from such occupation and head rest. I have in consequence given up, for the last ten years or more, all professional occupation, and voluntarily resigned a large income that I might pursue in some degree my own objects of research. But in doing this I have always, as a good subject, held myself ready to assist the Government if still in my power—*not for pay*, for, except in one instance (and then only for the sake of the person joined with me), I refused to take it. I have had the honour and pleasure of applications, and that very recently, from the Admiralty, the Ordnance, the Home Office, the Woods and Forests, and other departments, all of which I have replied to, and will reply to as long as strength is left me; and now it is to the condition under which I am obliged to do this that I am anxious to call your Lordship's attention in the present case. I shall be most happy to give my advice and opinion in any case as may be at the time within my knowledge or power, but I may not undertake to enter into investigations or experiments. If I were in London I would wait upon your Lordship, and say all I could upon the subject of the disinfecting fluids, but I would not undertake the experimental investigation; and in saying this I am sure that I shall have your sympathy and approbation when I state that it is now more than three weeks since I left London to obtain the benefit of change of air, and yet my giddiness is so little alleviated that I don't feel in any degree confident that I shall ever be able to return to my recent occupations and duties."

To Professor Schönbein he writes, three months later:—"I shame to say that I have not yet repeated the experiments (on ozone), but my head has been so giddy that my doctors have absolutely forbidden me the privilege and pleasure of working or thinking for a while; and so I am constrained to go out of town, be a hermit, and take absolute rest. In thinking of my own case it makes me rejoice to know of your health and strength, and look on whilst you labour with a constancy so unremitting and so successful."

He was made Member of the Academy of Sciences, Bologna, Foreign Associate of the Royal Academy of Sciences, Belgium, Fellow of the Royal Bavarian Academy of Sciences, Munich, and Correspondent of the Academy of Natural Sciences, Philadelphia.

Æt. 56 (1848).

He this year communicated his twenty-second series of 'Researches' as the Bakerian lecture. It was on the Crystalline Polarity of Bismuth (and other bodies), and on its relation to the Magnetic form of Force. 1. Crystalline Polarity of Bismuth; 2. Crystalline Polarity of Antimony; 3. Crystalline Polarity of Arsenic. The second part of this series on the same subject was (4) on the Crystalline Condition of various bodies, and (5) Nature of the Magnecrystallic Force, and general observations.

"I cannot conclude this series of Researches," he says, "without remarking how rapidly the knowledge of molecular forces grows upon us, and

how strikingly every investigation tends to develope more and more their importance and their extreme attraction as an object of study. A few years ago magnetism was to us an occult power affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and, through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself."

He gave three Friday discourses on the Diamagnetic Condition of Flame and Gases; on two recent inventions of Artificial Stone; and on the Conversion of Diamond into Coke by the Electric Flame.

He was made Foreign Honorary Member (one of eight) of the Imperial Academy of Sciences, Vienna, and Doctor of Liberal Arts and Philosophy in the University of Prague.

Æt. 57 (1849).

He gave two Friday discourses, one on Plücker's repulsion of the Optic Axes of Crystals by the Magnetic Poles; and the other on De la Rue's Envelope Machinery.

He reported to the Trinity House on the ventilation of Flambro' Head, Dungeness, Needles, and Portland Lighthouses.

He was made Honorary Member, First Class, Institute Royale des Pays-Bas, and Foreign Correspondent of the Institute, Madrid.

Æt. 58 (1850).

The twenty-third series of Researches in Electricity appeared, on the Polar or other Condition of Diamagnetic Bodies. The twenty-fourth series was the Bakerian lecture, on the possible relation of Gravity to Electricity. He finishes this paper, saying, "Here end my trials for the present. The results are negative; they do not shake my strong feeling of the existence of a relation between gravity and electricity, though they give no proof that such a relation exists." The twenty-fifth series was on the Magnetic and Diamagnetic Condition of Bodies:—1. Non-expansion of Gaseous Bodies by Magnetic Force. 2. Differential Magnetic Action. 3. Magnetic characters of Oxygen, Nitrogen, and Space. The twenty-sixth series was on Magnetic Conducting-power:—1. Magnetic Conduction. 2. Conduction Polarity. 3. Magnecrystallic Conduction. Atmospheric Magnetism:—1. General principles. The twenty-seventh series was on Atmospheric Magnetism (continued):—2. Experimental inquiry into the Laws of Atmospheric Magnetic Action, and their application to particular cases.

He gave a Friday discourse on the Electricity of the Air, and another on certain conditions of Freezing Water.

He reported on the adulteration of whitelead for the Trinity House.

To Prof. Schönbein he writes:—"By-the-by, I have been working with the oxygen of the air also. You remember that three years ago I dis-

tinguished it as a magnetic gas in my paper on the diamagnetism of flame and gases, founded on Bancalari's experiment. Now I find in it the cause of all the annual and diurnal and many of the irregular variations of the terrestrial magnetism. The observations made at Hobarton, Toronto, Greenwich, St. Petersburg, Washington, St. Helena, the Cape of Good Hope, and Singapore, all appear to me to accord with and support my hypothesis. I will not pretend to give you an account of it here, for it would require some detail, and I really am weary of the subject." Later he writes:—"I think I told you in my last how that oxygen in the atmosphere, which I pointed out three years ago in my paper on flame and gases as so very magnetic compared with other gases, is now to me the source of all the periodical variations of terrestrial magnetism, and so I rejoice to think and talk at the same time of your results, which deal also with that same atmospheric oxygen. What a wonderful body it is!"

Miss Martineau had said, on the authority of the Annual Register, that he countenanced the Acarus Crossii. Faraday corrects her:—"I hope you will forgive me for writing to you about this matter. I feel it a great honour to be borne on your remembrance, but I would not willingly be there in an erroneous point of view."

In the summer he was asked by a friend to stay in the country. He writes, August 24, from Upper Norwood:—"I have kept your picture to look at for a day or two before I acknowledge your kindness in sending it. It gives the idea of a tempting place; but what can you say to such persons as we are who eschew all the ordinary temptations of society? There is one thing, however, society has which we do not eschew; perhaps it is not very ordinary, though I have found a great deal of it, and that is kindness, and we both join most heartily in thanking you for it, even when we do not accept that which it offers. I must tell you how we are situated. We have taken a little house here on the hill-top, where I have a small room to myself, and have, ever since we came here, been deeply immersed in magnetic cogitations. I write and write and write until nearly three papers for the Royal Society are nearly completed, and I hope that two of them will be good if they justify my hopes, for I have to criticize them again and again before I let them loose. You shall hear of them at some of the Friday evenings; at present I must not say more. After writing I walk out in the evening, hand-in-hand, with my dear wife to enjoy the sunset; for to me, who love scenery, of all that I have seen or can see, there is none surpasses that of Heaven: a glorious sunset brings with it a thousand thoughts that delight me."

Earlier the same friend asked him, for the first time, to dinner. He writes from Brighton:—"Your note is a very kind one, and very gratefully received; I wish on some accounts that nature had given me habits more fitted to thank you properly for it by acceptance than those which really belong to me. In the present case, however, you will perceive that our being here supplies an answer (something like a lawyer's objection)

without referring to the greater point of principle. I should have been very sorry in return for your kindness to say *no* to you on the other ground, and yet I fear I should have been constrained to do so."

At the end of the year he had another invitation from the Honourable Col. Grey. "If you could make it convenient to come down to Windsor any afternoon in the course of next week, it would give His Royal Highness great satisfaction to have the opportunity of having some conversation with you on this interesting subject (the magnetic properties of oxygen)."

He was made Corresponding Associate of the Accademia Pontificia, Rome, and Foreign Associate of the Academy of Sciences, Haarlem.

Æt. 59 (1851).

The twenty-eighth series of Researches were sent to the Royal Society on Lines of Magnetic Force, their definite character, and their distribution within a Magnet and through Space; also the twenty-ninth series, on the employment of the Induced Magneto-electric Current as a test and measure of Magnetic Forces.

He gave three Friday discourses on the Magnetic Characters and Relations of Oxygen and Nitrogen; on Atmospheric Magnetism; and on Schönbein's Ozone.

No work is recorded for the Trinity House.

He was made Member of the Royal Academy of Sciences at the Hague, Corresponding Member of the Batavian Society of Experimental Philosophy, Rotterdam; Fellow of the Royal Society of Sciences, Upsala; a Juror of the Great Exhibition.

This year closed the series of 'Experimental Researches in Electricity.' It began in 1831 with the induction of electric currents, and his greatest discovery, the evolution of electricity from magnetism; then it continued to terrestrial magneto-electric induction; then to the identities of electricity from different sources; then to conducting-power generally. Then came electrochemical decomposition; then the electricity of the voltaic pile; then the induction of a current on itself; then static induction. Then the nature of the electric force or forces, and the character of the electric force in the Gymnotus. Then the source of power in the voltaic pile; then the electricity evolved by friction of steam; then the magnetization of light and the illumination of magnetic lines of force; then new magnetic actions, and the magnetic condition of all matter; then the crystalline polarity of bismuth, and its relation to the magnetic form of force; then the possible relation of gravity to electricity; then the magnetic and diamagnetic condition of bodies, including oxygen and nitrogen; then atmospheric magnetism; then the lines of magnetic force, and the employment of induced magneto-electric currents as their test and measure.

The record of this work, which he has left in his manuscripts and republished in his three volumes from the papers in the Philosophical Transactions, will ever remain Faraday's noblest monument—full of genius in the

conception, full of finished and most accurate work in execution; n quantity so vast that it seems impossible one man could have done so much; and this will appear still more when it is remembered that Anderson's help may be summed up in two words, blind obedience.

The use of magneto-electricity in induction machines, in electrotyping, and in lighthouses are the most important practical applications of the 'Experimental Researches in Electricity;' but who can attempt to measure or imagine the stimulus and the assistance which these researches have given, and will give, to other investigators?

Lastly, if we look at the circumstances under which this work was done, we shall see that during the greater part of these twenty years the Royal Institution was kept alive by the innumerable Friday lectures which he gave at it. "We were living," as he once said to the managers, "on the parings of our own skin." He had no grant from the Royal Society, and during the whole of this time the fixed income which the Institution could afford to give him was £100 a year, to which the Fullerian professorship added nearly £100 more.

By the 'Experimental Researches in Electricity,' Faraday's scientific life may be divided into three parts. The first lasted to 1830, when he was thirty-eight; the second, or "research period," lasted to 1851; and the third and final period began in 1852, and continued to his last report to the Trinity House (in 1865) on the foci and descent of a beam of light 336 feet at St. Bees Lighthouse.

Æt. 60 (1852).

The first and last Friday discourses of the season were on Lines of Magnetic Force. In the Philosophical Magazine there was a long paper on the Physical Character of the Lines of Magnetic Force. He begins with a note:—"The following paper contains so much of a speculative and hypothetical nature that I have thought it more fitted for the pages of the Philosophical Magazine than for those of the Philosophical Transactions. . . . ." "The paper, as is evident, follows series xxviii. and xxix., and depends much for its experimental support on the more strict results and conclusions contained in them."

He made many reports to the Trinity House, among others:—on adulterated white-lead; on oil in iron tanks; on impure olive-oils; on the Caskets lighthouse. And the question of the use of Watson's electric light was first moved by a letter of Dr. Watson to the Trinity House.

In October he wrote a long letter to M. De la Rive. ". . . Do not for a moment suppose I am unhappy. I am occasionally dull in spirits, but not unhappy. There is a hope which is an abundantly sufficient remedy for that; and as that hope does not depend on ourselves, I am bold enough to rejoice in that I may have it.

"I do not talk to you about philosophy, for I forget it all too fast to make it easy to talk about. When I have a thought worth sending you, it is in

the shape of a paper before it is worth speaking of; and after that it is astonishing how fast I forget it again; so that I have to read up again and again my own recent communications, and may well fear that, as regards others, I do not do them justice. However, I try to avoid such subjects as other philosophers are working at, and for that reason have nothing important in hand just now. I have been working hard, but nothing of value has come of it."

Two months later he writes to Professor Schönbein from Brighton:—

"I am here sleeping, eating, and lying fallow, that I may have sufficient energy to give half a dozen juvenile Christmas lectures. The fact is, I have been working very hard for a long time to no satisfactory end. All the answers I have obtained from nature have been in the negative; and though they show the truth of nature as much as affirmative answers, yet they are not so encouraging; and so for the present I am quite worn out. I wish I possessed some of your points of character; I will not say which, for I do not know where the list might end, and you might think me simply absurd, and, besides that, ungrateful to providence."

Æt. 61 (1853).

Early in the year he gave a Friday discourse on observations on the Magnetic Force, and he gave the last lecture of the season on MM. Boussingault, Fremy, and Becquerel's experiments on oxygen.

He gave five reports to the Trinity House—on a comparison of the French lens and Chance's lens; on the lightning-rods at Eddystone and Bishop's Lighthouses; on the ventilation of St. Catherine and the Needles Lighthouses, and that at Cromer; and on fog-signals. A Company was formed to carry out Watson's electric light, but no trial of it took place.

In June he sent to the Athenæum an experimental investigation of table-moving. At the end he says, "I must bring this long description to a close. I am a little ashamed of it, for I think in the present age and in this part of the world it ought not to have been required. Nevertheless I hope it may be useful. There are many whom I do not expect to convince, but I may be allowed to say that I cannot undertake to answer such objections as may be made. I state my own convictions as an experimental philosopher, and find it no more necessary to enter into controversy on this point than on any other in science (as the nature of matter, or inertia, or the magnetization of light) on which I may differ from others. The world will decide sooner or later in all such cases, and I have no doubt very soon and correctly in the present instance."

A month later he writes to Professor Schönbein:—

"I have not been at work except in turning the tables upon the table-turners. Nor should I have done that, but that so many inquiries poured in upon me that I thought it better to stop the inpouring flood by letting all know at once what my views and thoughts were. What a weak, credulous, incredulous, unbelieving, superstitious, bold, frightened, what a

ridiculous world ours is as far as concerns the mind of man! How full of inconsistencies, contradictions, and absurdities it is! I declare that, taking the average of many minds that have recently come before me (and apart from that spirit which God has placed in each), and accepting for a moment that average as a standard, I should far prefer the obedience, affections, and instinct of a dog before it. Do not whisper this, however, to others. There is One above who worketh in all things, and who governs even in the midst of that misrule to which the tendencies and powers of men are so easily perverted."

After this year, as Director of the Laboratory and Superintendent of the House, he received £300 from the Royal Institution.

He was made Foreign Associate of the Royal Academy of Sciences, Turin, and Honorary Member of the Royal Society of Arts and Sciences, Mauritius.

Æt. 62 (1854).

At the end of this year he sent a long paper to the Philosophical Magazine on some points of magnetic philosophy. He begins saying:—"Within the last three years I have been bold enough, though only as an experimentalist, to put forth new views of magnetic action in papers having for titles, 'On Lines of Magnetic Force,' Phil. Trans. 1852; and 'On Physical Lines of Magnetic Force,' Phil. Mag. 1862. I propose to call the attention of experimenters in a somewhat desultory manner to the subject again, both as respects the deficiency of the present physical views and the possible existence of lines of physical force."

A course of lectures on education was given by different eminent men at the Royal Institution. Prince Albert came to Faraday's "Observations of Mental Education" on the 6th of May. In reprinting them, he said, "They are so immediately connected in their nature and origin with my own experimental life, considered either as cause or consequence, that I have thought the close of this volume (of Researches on Chemistry and Physics) not an unfit place for their reproduction." He ends his lecture by saying, "My thoughts would flow back amongst the events and reflections of my past life, until I found nothing present itself but an open declaration—almost a confession—as a means of performing the duty due to the subject and to you."

He gave two Friday discourses on Electric Induction, associated cases of Current and Static Effects; and on Magnetic Hypotheses.

The Parliamentary Committee of the British Association applied to him through Lord Wrottesley for his opinion whether any and what measures could be adopted by the Government or the Legislature to improve the position of science or of the cultivators of science in this country. He answers:—"I feel unfit to give a deliberate opinion. My course of life and the circumstances which make it a happy one for me are not those of persons who conform to the usages and habits of society. Through the kindness of all, from my Sovereign downwards, I have that which supplies all

my need; and in respect of honours, I have as a scientific man received from foreign countries and sovereigns those which, belonging to very limited and select classes, surpass in my opinion anything that it is in the power of my own to bestow.

"I cannot say that I have not valued such distinctions; on the contrary, I esteem them very highly, but I do not think I have ever worked for or sought them. Even were such to be now created here, the time is passed when these would possess any attraction for me, and you will see therefore how unfit I am, upon the strength of any personal motive or feeling, to judge of what might be influential upon the minds of others. Nevertheless I will make one or two remarks which have often occurred to my mind. . . . . A Government should, *for its own sake*, honour the men who do honour and service to the country. The aristocracy of the class should have distinctions which should be unattainable except to that of science. . . . . But, besides, the Government should, in the very many cases which come before it having a relation to scientific knowledge, employ men who pursue science, provided they are also men of business. This is perhaps now done to some extent, but to nothing like the degree which is practicable with advantage to all parties. The right means cannot have occurred to a Government which has not yet learned to approach and distinguish the class as a whole."

He sent five reports to the Trinity House, one of which, in two parts, was on Dr. Watson's electric light (voltaic), and on Prof. Holmes's electric light (magneto-electric). The conclusion was that he could not recommend the electric light, that it had better be tried for other than lighthouse uses first. To Dr. Watson he wrote that he "could not put up in a lighthouse what has not been perfectly established beforehand, and is only experimental."

He was made Corresponding Associate of the Royal Academy of Sciences, Naples.

Æt. 63 (1855).

His first Friday discourse was on some Points of Magnetic Philosophy and on Gravity. Later he gave a discourse on Electric Conduction; and another on Ruhmkorff's Induction-apparatus.

For the Trinity House he only went to Birmingham to examine some apparatus of Chance's.

This year, on the application of his friend M. Dumas, he was made Commander of the Legion of Honour, and received the Grand Medal of Honour of the French Exhibition for his discoveries.

He was made Honorary Member of the Imperial Society of Naturalists, Moscow, and Corresponding Associate of the Imperial Institute of Sciences of Lombardy.

Æt. 64 (1856).

This year he sent to the Royal Society his last paper, Experimental Relations of Gold (and other metals) to Light. It was read as the Bakerian lecture early the following year.

"At one time I had hoped that I had altered one coloured ray into another by means of gold, which would have been equivalent to a change in the number of undulations; and though I have not confirmed that result as yet, still those I have obtained seem to me to present a useful experimental entrance into certain physical investigations respecting the nature and action of a ray of light. I do not pretend that they are of great value in their present state, but they are very suggestive, and they may save much trouble to any experimentalist inclined to pursue and extend this line of investigation."

He gave two Friday discourses, the first on certain magnetic actions and affections; and the second on M. Petitjean's process for silvering glass, and some observations on divided gold.

He gave five reports to the Trinity House, and he entered into an engagement regarding the Board of Trade Lighthouses, and made four reports, two on Cape Race Lighthouse, and one on Dr. Normandy's distilled water-apparatus.

He was made Corresponding Member of the Netherland Society of Sciences, Batavia, and Member of the Imperial Royal Institute of Padua.

Æt. 65 (1857).

Two Friday discourses were given, the first on the Conservation of Force, and the second on the relations of Gold to Light.

"Various circumstances," he begins, "induce me at the present moment to put forth a consideration regarding the conservation of force. . . . . There is no question which lies closer to the root of all physical knowledge than that which inquires whether force can be destroyed or not. . . . . Agreeing with those who admit the conservation of force to be a principle in physics as large and sure as that of the indestructibility of matter, or the invariability of gravity, I think that no particular idea of force has a right to unlimited and unqualified acceptance that does not include assent to it. . . . Supposing the truth of the principle is assented to, I come to its uses. No hypothesis should be admitted nor any assertion of a fact credited that denies the principle. . . . The received idea of gravity appears to me to ignore entirely the principle of the conservation of force, and by the terms of its definition, if taken in an absolute sense, '*varying* inversely as the square of the distance,' to be in direct opposition to it."

To Mr. Barlow he writes:—

"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 result."

He gave six reports to the Trinity House. The most important was on Holmes's magneto-electric light, which was put up at Blackwall, and observed from Woolwich, and compared with a Fresnel lamp in the centre of Bishop's lens, and also in the focus of a parabolic reflector. He critically examined the cost of the apparatus, the price of the light, the suppositions regarding its intensity and advantages, and the proposition to put one up in a lighthouse. He agreed to its being tried at the South Foreland.

He was made Member of the Institute of Breslau, Corresponding Associate of Institute of Sciences, Venice, and Member of the Imperial Academy, Breslau.

Æt. 66 (1858).

He wrote a short paper on Regelation, which he sent with a letter to Dr. Tyndall on Ice of irregular fusibility. These were printed in Dr. Tyndall's paper on some Physical Properties of Ice in the Philosophical Transactions for this year.

He gave two Friday discourses. The first was remarks on Static Induction; and the other on Wheatstone's Electric Telegraph in relation to Science (being an argument in favour of the full recognition of science as a branch of education).

This year Prince Albert offered him a house on Hampton Court Green. It required repair, and he doubted whether he could afford to do it up.

He writes to a niece:—

"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. Phipps is most kind."

To Sir C. Phipps he writes:—

"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 would not bring myself to decline so honourable 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 favour 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."

To M. De la Rive he thus writes on the death of Mrs. Marcet:—

"Your subject interested me deeply every way, for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of 13 in the year

1804, remained there eight years, and during the chief part of the time bound books. Now it was in those books in the hours after work that I found the beginning of my philosophy. There were two that especially helped me, the 'Encyclopædia Britannica,' from which I gained my first notions of electricity, and Mrs. Marcet's 'Conversations on Chemistry,' which gave me my foundation in that science.

"Do not suppose that I was a very deep thinker, or was marked as a precocious person. I was a very lively, imaginative person, and could believe in the Arabian Nights as easily as in the Encyclopædia; but facts were important to me and saved me. I could trust a fact, and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet: first, as one who had conferred great personal good and pleasure on me, and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught, and inquiring mind.

"You may imagine my delight when I came to know Mrs. Marcet personally; how often I cast my thoughts backwards, delighting to connect the past and the present; how often, when sending a paper to her as a thank-offering, I thought of my first instructress; and such like thoughts will remain with me.

"I have some such thoughts even as regards your own father, who was, I may say, the first who personally, at Geneva, and afterwards by correspondence, encouraged, and by that sustained me."

He made twelve reports to the Trinity House. The most important was on the electric light at the South Foreland. He went there, with a Committee of the Trinity House, to see it from sea and land. The light was in the centre of the Fresnel apparatus, in the upper light, as a fixed light, and so comparable with the lower fixed light, which consisted of oil-lamps in reflectors. They went to the Varne light-ship. The upper was generally inferior to the lower light. Next morning they went to the light-house, and examined it by day and also at night.

He was made Corresponding Member of the Hungarian Academy of Sciences, Pesth.

Æt. 67 (1859).

He gave two Friday discourses on Schönbein's Ozone and Antozone; and on Phosphorescence, Fluorescence, &c. He sent eleven reports to the Trinity House, and one to the Board of Trade. On the 28th of March, the magneto-electric light was again exhibited at the South Foreland. On the 20th of April he went to sea to examine it. "The upper light," he says, "is far superior to the lower light; the electric light very fine." He visited the lighthouse; he found new lamps by Duboscq, and silvered reflectors behind. He writes:—"As a light unexceptionable; as electric light won-

derful." He had before drawn up instructions to lighthouse keepers and pilot cutters; and on the 29th of April he reports the sufficiency of the light as established.

He reported this year on Way's mercurial electric light; the one advantage it had was that the place of the light was unchangeable.

He was one of a Commission appointed to consider the subject of lighting public galleries by gas; and he reported favourably on the experimental attempt at the Sheepshanks Gallery.

To Mr. Barlow he writes from Hampton Court:—"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 and 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 year. It seems to give much satisfaction to all who have seen it, though the subject is beset with difficulties; for when the depth and 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 me 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, and there is so little that can serve as previous experience in the founding and arranging these degrees, that one must leave the whole endeavour to shape itself as the practice and experience accumulates."

Æt. 68 (1860).

He gave two Friday discourses on Lighthouse Illumination by the Electric Light; and on the Electric Silk-loom.

He gave eleven reports to the Trinity House, and he examined three Red-Sea lighthouses for the Board of Trade. On the 13th of February he went to Dover, but was prevented by snow from reaching the lighthouse; on the 17th he tried again, and on the 28th he gave his final report on the practicability and utility of the magneto-electric light. He says, "Hope it will be applied." On the 14th of March the magneto-electric light was proposed for Dungeness. On the 21st he gives his reply, and says there is no difficulty.

He was appointed with Sir Roderick Murchison to report upon the means of preserving the stonework of the new Palace at Westminster.

At Christmas he gave his last course of juvenile lectures on the chemical history of a candle.

He was made Foreign Associate of the Academy of Sciences, Pesth, and Honorary Member of the Philosophical Society of Glasgow.

He resumed the office of Elder in his Church in the autumn, and in little more than three years and a half he finally resigned it.

Æt. 69 (1861).

He gave Friday discourses on Platinum, and on Warren De La Rue's Photographic Eclipse results.

He gave ten reports to the Trinity House. The most important work was a visit on 31st of October to Dungeness, to see the new magneto-electric lamps, the machines, and the steam-engines. He drew up forms of observations to be made at Dungeness, at other lighthouses, and by the pilot cutters.

To Prof. Schönbein he writes:—"You really startle me with your independent antozone. . . . Surely you must hold it in your hand like a little struggler; for, if I understand you rightly, it must be a far more abundant body than cæsium. For the hold you have already obtained over it I congratulate you, as I would do if you had obtained a crown, and more than for a new metal. But surely these wonderful conditions of existence cannot be confined to oxygen alone. I am waiting to hear that you have discovered like parallel states with iodine, or bromine, or hydrogen, and nitrogen—what of nitrogen? is not its apparent quiet simplicity of action all a sham? not a sham, indeed; but still not the only state in which it can exist. If the compounds which a body can form show something of the state and powers it may have when isolated, then what should nitrogen be in its separate state? You see I do not work; I cannot; but I fancy, and stuff my letters with such fancies (not a fit return) to you."

In another letter he says, "I am still dull, stupefied, and forgetful. I wish a discovery would turn up with me, that I might answer you in a decent, respectable way; but it will not."

Still later he says:—"I look forward to your new results with great interest; but I am becoming more and more timid when I strive to collate hypotheses relating to the chemical constitution of matter. I cannot help thinking sometimes whether there is not some state or condition of which our present notions give us very little idea, and which yet would reveal to us a flood, a world of real knowledge,—a world of facts available both by practical application and their illustrations of first principles; and yet I cannot shape the idea into a definite form, or reach it by any trial facts that I can devise; and that being the case, I drop the attempt and imagine that all the preceding thought has just been a dreaminess and no more; and so there is an end of it."

In October he wrote to the Managers of the Institution:—"It is with the deepest feeling that I address you. I entered the Royal Institution in March 1813, nearly forty-nine years ago, and, with exception of a comparatively short period, during which I was abroad on the continent with Sir H. Davy, have been with you ever since. During that time I have been most happy in your kindness, and in the fostering care which

the Royal Institution has bestowed upon me. Thank God, first, for all his gifts. I have next to thank you and your predecessors for the unswerving encouragement and support which you have given me during that period. My life has been a happy one, and all I desired. During its progress I have tried to make a fitting return for it to the Royal Institution, and through it to science. But the progress of years (now amounting in number to threescore and ten) having brought forth first the period of development, and then that of maturity, have ultimately produced for me that of gentle decay. This has taken place in such a manner as to make the evening of life a blessing; for whilst increasing physical weakness occurs, a full share of health free from pain is granted with it, and whilst memory and certain other faculties of the mind diminish, my good spirits and cheerfulness do not diminish with them.

"Still I am not able to do as I have done. I am not competent to perform as I wish the delightful duty of teaching in the Theatre of the Royal Institution, and I now ask you (in consideration for me) to accept my resignation of the juvenile lectures. Being unwilling to give up what has always been so kindly received and so pleasant to myself, I have tried the faculties essential for their delivery, and I know that I ought to retreat; for the attempt to realize (in those trials) the necessary points brings with it weariness, giddiness, fear of failure, and the full conviction that it is time to retire; I desire therefore to lay down this duty. I may truly say that such has been the pleasure of the occupation to me, that my regret must be greater than yours need or can be.

"And this reminds me that I ought to place in your hands the whole of my occupation. It is no doubt true that the juvenile lectures, not being included in my engagement as professor, were when delivered by me undertaken as an extra duty, and remunerated by an extra payment. The duty of research, superintendence of the house, and of other services still remains; but I may well believe that the natural change which incapacitates me from lecturing, may also make me unfit for some of these. In such respects, however, I will leave you to judge, and to say whether it is your wish that I should still remain as part of the Royal Institution. I am, gentlemen, with all my heart, your faithful and devoted servant."

Shortly afterwards he wrote to the Secretary:—"You know my feelings, in regard to the exceedingly kind manner in which the Board of Managers received my letter, and *you* therefore can best convey to them my deep thanks on this occasion. Please do this for me. Nothing would make me happier in the things of this life than to make some scientific discovery or development, and by that to justify the Board in their desire to retain me in my position here."

Sir Emerson Tennant wished Mr. Faraday to witness the phenomena produced by Mr. Home. Mr. Faraday says, in his reply, "You will see that I consent to all this with much reserve and only for your sake." Three days afterwards Sir E. Tennant says, "As Mr. Home's wife is dying, the

probability is that the meeting, at which I wished you to be present, on the 24th may not take place. From the same cause I am unable to see Mr. Home previously, or to make the inquiries of himself necessary to satisfy the queries in your letter."

He was made Honorary Member of the Medical Society of Edinburgh.

Æt. 70 (1862).

On the 20th of June he gave his last Friday discourse, on Gas furnaces.

He gave seventeen reports to the Trinity House, and two to the Board of Trade. The most important of the Trinity House reports were still on the magneto-electric light. On the 12th of February he went to Dungeness, examined the engine-room, the machines, the lanthorn, the lamps, and the photometric effects. The keepers he examined, and found them not intelligent enough. At night he went to sea, testing at five miles off the effects of oil-lamp reflectors and the electric light, Prof. Holmes himself being in charge of the lamps for the trials. Then he went to the Varne floating-light, and compared Dungeness, Grisnez, and the South Foreland lights. In the morning he went to Dover to examine the upper South Foreland new hydrostatic lamp; and, in the course of the year, the different observations made at South Foreland, Varne, Dungeness, and the pilot-cutters had to be considered and reported on. The House of Commons this year called for copies of his reports on the magneto-electric light to be printed. At the International Exhibition he saw Berlio's magneto-electric machine and light, and he reported on the construction of it.

This year he was examined at great length by the Public School Commissioners. His most important answers were these:—"that the natural knowledge which had been given to the world in such abundance during the last fifty years, I may say, should remain untouched, and that no sufficient attempt should be made to convey it to the young mind, growing up and obtaining its first views of these things, is to me a matter so strange that I find it difficult to understand; though I think I see the opposition breaking away, it is yet a very hard one to be overcome. That it ought to be overcome I have not the least doubt in the world." In answer to the question at what age it might be serviceable to introduce the physical sciences, he says, "I think one can hardly tell that until after experience for some few years. All I can say is this, that at my Juvenile Lectures, at Christmas time, I have never found a child too young to understand intelligently what I told him: they came to me afterwards with questions which proved their capability."

Again he says, "I do think that the study of natural science is so glorious a school for the mind, that with the laws impressed on all created things by the Creator, and the wonderful unity and stability of matter and the forces of matter, there cannot be a better school for the education of the mind."

In September he wrote his last letter to Prof. Schönbein; he says,

"Again and again I tear up my letters, for I write nonsense. I cannot spell or write a line continuously. Whether I shall recover this confusion, do not know. I will not write any more. My love to you."

The Duke of Devonshire at his installation would have the University of Cambridge confer the degree of LL.D. on Faraday. He was also made Knight Commander of the Order of St. Maurice and Lazarus, Italy.

Æt. 71 (1863).

He made twelve reports to the Trinity House. In February he was again at Dungeness examining a new optic apparatus, and comparing the reflectors with the electric light, and new and old apparatus. He reported on the observations regarding the magneto-electric light, and on a French application to the Board of Trade about the magneto-electric light.

To the Registrar of the London University he wrote:—"Many of your recent summonses have brought so vividly to my mind the progress of time in taking from me the power of obeying their call, that I have at last resolved to ask you to lay before the Senate my desire to relinquish my station and render up that trust of duty which I can no longer perform with satisfaction either to myself or to others.

"The position of a Senator is one that should not be held by an inactive man to the exclusion of an active one. It has rejoiced my heart to see the progress of the University and of education under its influence and power; and that delight I hope to have so long as life shall be spared to me."

He was made Foreign Associate of the Imperial Academy of Medicine, Paris.

Æt. 72 (1864).

Twelve reports were made between January and October to the Trinity House. One was on a new magneto-electric machine; another on drawings, proposals, and estimates for the magneto-electric light at Portland. He made seven examinations of white and red leads, and two examinations of waters from Orfordness and the Fog-gun station, Lundy Island; and he reported on two 4th-order lights for the River Gambia.

He replied to an invitation of the Messrs. Davenport:—"I am obliged by your courteous invitation; but really I have been so disappointed by the manifestations to which my notice has at different times been called, that I am not encouraged to give any more attention to them, and therefore I leave these to which you refer in the hands of the Professors of Legerdemain. If spirit communications, not utterly worthless, should happen to start into activity, I will leave the spirits to find out for themselves how they can move my attention. I am tired of them."

A few weeks later he replied to another different invitation:—

"Whenever the spirits can counteract gravity or originate motion, or supply an action due to natural physical force, counteract any such action,—whenever they can pinch or prick me, or affect my sense of feeling or any other sense, or in any other way act on me without my waiting on them, or,

working in the light, can show me a hand, either writing or not, or in any way make themselves visibly manifest to me—whenever these things are done, or anything which a conjuror cannot do better, or, rising to higher proofs, whenever the spirits describe their own nature, and, like honest spirits, say what they can do, or pretending, as their supporters do, that they can act on ordinary matter whenever they initiate action, and so make *themselves* manifest,—whenever by such-like signs they come to me and ask my attention to them, I will give it. But until some of these things be done, I have no more time to spare for them or their believers, or for correspondence about them."

At the end of the year he was asked by Mr. Cole to be a Vice-President of the Albert Hall. He replied:—"I have just returned from Brighton, to which place my doctor had sent me under nursing care. Hence the delay in answering your letter, for I was unaware of it until my return. Now, as to my acceptance of the honour you propose to me. With my rapidly failing faculties, ought I to accept it? You shall decide. Remember that I was obliged to decline lecturing before Her Majesty and the Royal Family at Osborne; that I have declined and am declining the Presidency of the Royal Society, the Royal Institution, and other bodies; declaring myself unfit to undertake any reponsibility or duty even in the smallest degree. Would it not therefore be inconsistent to allow my name to appear amongst those of the effectual men who delight, as I should have done under other circumstances, to honour in every way the memory of our most gracious and regretted leader? These are my difficulties. It is only the name and the remembrance of His Royal Highness which would have moved me from a long-taken resolution."

Mr. Cole decided, "without a moment's doubt," that he was to be a Vice-President.

To a friend he writes:—"I find myself less and less fit for communication with society, even in a meeting of family—brothers and sisters. I cannot keep pace in recollection with the conversation, and so have to sit silent and taciturn. Feeling this condition of things, I keep myself out of the way of making an exposure of myself."

He was made Foreign Associate of the Royal Academy of Sciences, Naples.

Æt. 73 (1865).

He made his last report for the Trinity House in May this year on St. Bees Light.

He wrote to the Deputy Master:—"I write to put myself plainly before you in respect of the matter about which I called two days ago. At the request of the then Deputy Master I joined the Trinity House in February 1836, now near upon thirty years since. I find that time has had its usual effect upon me, and that I have lost the power of remembering and also of other sorts, and I desire to relieve my mind. Can this

be done without my retiring altogether, and can you help me in this matter?"

In looking back to his work for the Trinity House, going down to analyses of cottons, oils, paints, and waters, and recalling his words "that £200 a year is quite enough in itself, but not if it is to be the indicator of the character of the appointment," one is rejoiced to find that he received the highest reward which the scientific man can obtain. After himself testing the results by the most complete and searching trials, he was able to recommend that his own grandest discovery should be applied to "the great object of guiding the mariner across the dark and dreary waste of waters."

To the Managers of the Royal Institution he wrote, March 1:—

"Unless it be that as I get older I become more infirm in mind, and consequently more timid and unsteady, and so less confident in your warm expressions, I might, I think, trust more surely in your resolution of the 2nd of December, 1861, and in the reiterated verbal assurances of your kind Secretary than I do; but I become from year to year more shaken in mind, and feel less able to take any responsibility on me. I wish, therefore, to retire from the position of Superintendent of the house and laboratories. That which has in times past been my chiefest pleasure has now become a very great anxiety; and I feel a growing inability to advise on the policy of the Institution, or to be the one referred to on questions both great and small as to the management of the house.

"In a former letter, when laying down the juvenile lectures, I mentioned 'that other duties, such as research, superintendence of the house, and other services still remain;' but I then feared that I might be found unfit for them; I am now persuaded that this is the case. If under these circumstances you may think that with the resignation of the positions I have thus far filled the rooms I occupy should be at liberty, I trust that you will feel no difficulty in letting me leave them; for the good of the Institution is my chief desire in the whole of this action. Permit me to sign myself personally, your dear, indebted, and grateful friend."

"Resolved unanimously—

"That the Managers thank Professor Faraday for the scrupulous anxiety which he has now and ever shown to act in every respect for the good of the Royal Institution. They are most unwilling that he should feel that the cares of the laboratories and the house weigh upon him. They beg that he will undertake only so much of the care of the house as may be agreeable to himself, and that whilst relinquishing the duties of 'Director of the laboratory,' he will retain his home at the Royal Institution."

Sir David Brewster sent him a pamphlet on the Invention and Introduction of the Dioptric Lights, and asked him to give his opinion on the value and importance of these lights. He replied:—". . . . I would rather not enter as an arbitrator or judge into the matter, for I have of late been

resigning all my functions as one incompetent to take up such matters, and the Royal Institution as well as the Trinity House have so far accepted them as to set me free from all anxiety of thought in respect to them. In fact my memory is *gone*, and I am obliged to refrain from reading argumentative matter or from judging of it. I am very thankful for their tenderness in the matter; and if it please Providence to continue me a year or two in this life, I hope to bear the decree patiently. My time for contending for temporal honours is at an end, whether it be for myself or others."

In the fine summer at Hampton Court he sat in his window delighting in the clouds and the holiday-people on the green. A friend from London asked how he was. "Just waiting," he replied. This he more fully said in a note. "I bow before him who is Lord of all, and hope to keep waiting patiently for His time and mode of releasing me according to His divine word, and the great and precious promises whereby His people are made partakers of the divine nature."

To Sir James South, who wished to have some account of Anderson's services, Faraday wrote:—"Whilst endeavouring to fulfil your wishes in relation to my old companion, Mr. Anderson, I think I cannot do better than accompany some notes which he has himself drawn up and had printed, by some remarks of mine, which will show how and how long he has been engaged here.

"He came to assist in the glass house for the service of science in September 1827, where he remained working until about 1830. Then for a while he was retained by myself. In 1832 he was in the service of the Royal Institution, and paid by it. From that time to the present he has remained with that body, and has obtained their constant approbation. In January 1842 they raised his pay to £100 per annum with praise. In 1847 they raised it in like manner to £110. For the same reason in 1853 they raised it to £120; and in 1860, in a minute, of which I think Mr. Anderson has no copy, they say that, in consideration of his now lengthened services and the diligence exhibited by him, they are of opinion that his salary should be raised to £130.

"Mr. Anderson still remains with us, and is in character what he has ever been. He and I are companions in years and in work and in the Royal Institution. Mr. Brande's testimony when he left the Institution is to the same purport as the others. Mr. Anderson was 75 years of age on the 12th of last month (January). He is a widower, but has a daughter keeping his house for him. We wish him not to come to the Royal Institution, save when he is well enough to make it a pleasure; but he seems to be happy being so employed."

Æt. 74 (1866).

Early in January Anderson died. Sir James South wished some monument to be put up to him, and wrote to Faraday. He replied:—

"My dear old friend, I would fain write to you, but, indeed, write to no one, and have now a burn on the fingers of my right hand which adds to my trouble; so that I still use my dear J.'s hand as one better than my own, and fear I give her great work by so doing. She has, I understand, written to you this morning, and told you how averse I am to meddling with sepulchral honours in *any* case. I shall mention your good will to Anderson" [here Faraday took the pen, because his niece made some objection to the words "mention the good will to Anderson," who was dead]; "but I tell them what are my feelings. I have told several what may be my own desire; to have a plain simple funeral, attended by none but my own relatives, followed by a gravestone of the most ordinary kind, in the simplest earthly place.

"As death draws nigh to old men or people, this world disappears, or should become of little importance. It is so with me; but I cannot say it simply to others [here he stopped his writing, and his niece finished the note], for I cannot write it as I would. Yours, dear old friend, whilst permitted."

The Society of Arts this summer gave him a medal for his scientific discoveries.

During the winter he became very feeble in all muscular power. Almost the last interest he showed in scientific things was in a Holtz electric machine.

In the spring, for a short time, with decreasing power, there was at times wandering of mind. One day he fancied he had made some discovery somehow related to Pasteur's dextro- and lævo-racemic acid. He desired the traces of it to be carefully preserved, for "it might be a glorious discovery."

His loss of power became more and more plain during the summer and autumn and winter: all the actions of the body were carried on with difficulty; he was scarcely able to move; but his mind continually overflowed with the consciousness of the affectionate care of those dearest to him.

Æt. 75 (1867).

At times he could hardly speak a word, and with difficulty swallow a mouthful.

In the spring he went to Hampton Court. Gradually he became more and more torpid, and on the 25th of August he died there.

He said of himself, "In early life I was a very lively imaginative person, who could believe in the Arabian Nights as easily as in the Encyclopædia. But facts were important to me and saved me. I could trust a fact." And so afterwards this blacksmith's son from Jacob's Well Mews, full of inborn religion, and gentleness, genius, and energy, searched for and trusted to facts in his experimental researches, and thus left to science a monument of himself that may be compared even to that of Newton.

H. B. J.

On the 11th of December, 1781, at Jedburgh, was born DAVID BREWSTER, who, having made a telescope when only 10 years of age, and having entered on his university course at 12, devoted one of the longest of lives to discoveries in optics, and at last, laden with academic and scientific honours, sank peacefully to rest on the 10th of February, 1868.

He was one of four brothers, all educated for the Church of Scotland, and he advanced to the position of a licentiate; but a certain nervousness in speaking and delicacy of health, combined with an overpowering love for scientific pursuits, led him to decline a good presentation, and to abandon the clerical profession for that of an expounder of natural philosophy. Thus he entered on a career of investigation and literary work which for magnitude, as well as importance, has rarely been rivalled.

As an editor, he commenced in 1808 a work so large that it occupied him for twenty-two years—the Edinburgh Encyclopædia; and in the mean time he began with Professor Jameson the Edinburgh Philosophical Journal, and subsequently the Edinburgh Journal of Science; and from 1832 he was one of the editors of the Philosophical Magazine. Throughout his connexion with these periodicals he was a frequent contributor of original articles to their pages, and he continued to the last to write for the North British and other Reviews in a style so polished and so vigorous, that multitudes learnt from him the actual state of scientific questions who would never have read a merely learned dissertation.

But his fame rests not so much on this literary work as on his original researches, which were so numerous that the 'Catalogue of Scientific Papers' now being published by the Royal Society contains the titles of 299 papers by him, besides five in which his name is conjoined with those of other investigators. And these researches, though principally connected with the phenomena of light, spread over many other departments of human knowledge.

Nor were Brewster's labours for the advancement of science confined to the laboratory and the desk. In 1821 he founded the Scottish Society of Arts, and in 1831 he was one of the small party of friends who instituted the British Association, in the meetings of which he usually took a prominent part.

During this time honours steadily flowed in upon him. He was made an honorary M.A. of Edinburgh in 1800, and seven years afterwards an honorary LL.D. of Aberdeen. From 1838 to 1859 he was Principal of the United Colleges of St. Salvador and St. Leonard's at the University of St. Andrews; and for the last eight years of his life he held the same important office in the leading University of Scotland.

Having been chosen a Fellow of the Royal Society of Edinburgh in 1808, Sir David acted for a long time as its Secretary, and he was President at the time of his death. In 1815 he obtained both the Copley Medal and

the Fellowship of our Society; and this was followed three years afterwards by the Rumford Medal, and subsequently by one of the Royal Medals; and, singularly enough, in each case for discoveries concerning the Polarization of Light. In 1816 the French Institute awarded him a pecuniary prize, and nine years afterwards he became a Corresponding Member of that body; while in 1849 there was conferred upon him the distinguished honour of being chosen one of the eight Foreign Associates of the Academy of Sciences.

It would be tedious to enumerate his other honours from learned bodies at home and abroad; suffice it to add that he was made a Chevalier of the Prussian Order of Merit, and was knighted by his sovereign in 1832.

Sir David was twice married: first to the daughter of James Macpherson, M.P., of Belleville, the translator of Ossian, and afterwards to Jane Kirk, second daughter of the late Thomas Purnell, Esq., of Scarborough.

To give any adequate idea of the discoveries made known in those scientific papers which Sir David Brewster published every two or three months for sixty years, would be a task of gigantic magnitude. There seem to be thirty papers by him in our Transactions, principally in the earlier part of his career, and, with two exceptions, they are all on optical subjects. In 1813 he commenced with a communication "On some Properties of Light," and in the two succeeding years our Society published for him no less than nine papers—on the polarization of light by oblique transmission, by its passage through unannealed glass, by simple pressure, or by reflection, and on the optical properties of mother-o'-pearl, on calcareous spar. The phenomena of double refraction were indeed treated of in several subsequent papers; but there is a gap between 1819 and 1829, when he wrote on the periodical colours produced by grooved surfaces, investigated elliptic polarization by metals, and reverted to the optical nature of the crystalline lens. Two papers, one on the Diamond and the other on the Colours of Thin Plates, terminate this series in 1841; and the only paper he afterwards sent to our Transactions was one in conjunction with Dr. Gladstone on the Lines of the Solar Spectrum. But there seems never to have been any long intermission in his researches on light; for he was constantly sending communications on this subject to the Royal Society of Edinburgh or some other learned body, or to the various scientific serials with which he was connected. Thus in the first Number of the Edinburgh Philosophical Journal we find two papers from his pen, the first on new optical and mineralogical structure exhibited in certain specimens of Apophyllite and other minerals, the second on the Phosphorescence of Minerals.

It was as a laborious observer and ingenious experimenter that he excelled; he cared rather to collect a multitude of facts than to deduce from them general laws. Wonderful proofs of perseverance are his Tables of refractive indices, of dispersive powers, and of the polarizing angles of various reflecting bodies; and he seems to have submitted to optical exami-

nation every mineral that came in his way. Frequently one of these substances would form the subject of a monograph, as diamond, or amber, the double cyanide of platinum and magnesium, the felspar of Labrador with its changeable tints, or Glauberite with its one axis of double refraction for the violet, and two axes for the red ray. The prismatic spectrum arrested his attention, and in 1834 he announced the absorption of certain rays by the earth's atmosphere, and by nitrous gas; while eight years afterwards he pointed out the existence of luminous lines in certain flames corresponding to those defective in the light of the sun; but he missed the beautiful explanation of Kirchhoff. He also investigated the phenomena of diffraction and dichroism, and of late years exhibited to the British Association the tints of a soap-bubble, or of decomposing glass rendered still more lively by being viewed through a microscope. Indeed his last legacy to science was a paper on Film forms.

The best monument to his fame is perhaps his investigation of polarized light. Malus had first set foot on this domain, but his premature death left it open to the entrance of Brewster, and what wonderful regions did he explore! It not unfrequently happened that some other philosopher, with perhaps a profounder knowledge of mathematics, stepped in and deduced important laws; but sometimes he himself arrived at the higher generalizations; as, for instance, may be cited that of the refractive index of a substance being the tangent of its polarizing angle. But he was not always fortunate in his theories; thus his ingenious view of solar light, as composed of three primary colours (red, yellow, and blue) forming coincident spectra of equal length, has been shown to be completely fallacious. Yet he never abandoned his theory; a fact which we are disposed to attribute, not to a want of conscientious truthfulness, but rather to an inability to appreciate the real bearing of an argument, and to an over confidence in his own memory and the testimony of his senses.

During his optical investigations Sir David often turned from the phenomena seen to the organ of sight, and experimented on that wonderful eye which saw bands in the red rays less refrangible than Fraunhofer's A. Of late years especially he examined the functions of the retina, the *foramen centrale*, and the choroid coat of the eye of animals; he wrote several papers on the *muscæ volitantes*, and explained many peculiarities of single and binocular vision, and not a few optical illusions.

While pursuing these researches on light, he made frequent excursions into other regions of science; he discovered fluids in the cavities of some of the minerals he was examining, and these must be investigated; he wrote much on the mean temperature of the globe; his attention was attracted at one time to fossil bones from Ava, at another to the varnish-trees of India; while systems of double stars, and the pyro-electricity of minerals shared the notice of his comprehensive mind.

As an inventor of new apparatus Brewster also acquired no little renown. His first paper on this subject appears to have been "Some remarks on

Achromatic Eyepieces" in Nicholson's Journal for 1806; and seven years afterwards he published a separate "Treatise on new Philosophical Instruments for various purposes in the Arts and Sciences." In 1816, while repeating some experiments of Biot with a glass trough, he noticed that peculiar method of reflection which is the principle of the Kaleidoscope; and no sooner was this pretty instrument before the public than it became marvellously popular, and that not only as a toy for old and young, but large expectations were raised of its usefulness to the artist and designer of patterns. We are also indebted to him for many other ingenious contrivances for micrometers, burning-glasses, &c., and his writings frequently contained the germs of future inventions. Hence it is not easy to determine his precise share of merit in such appliances as the lenticular stereoscope, or the polyzonal lenses used in lighthouses. In regard to the latter, however, it may be safely maintained that while the chief credit of elaborating the dioptric system of illumination must be given to Fresnel, the persistent advocacy of Brewster materially contributed to its adoption on the shores of our own island.

In addition to the treatises already mentioned he wrote several distinct works of a biographical character:—the Memoirs of Sir Isaac Newton, Euler's Letters and Life, and the Martyrs of Science, viz. Galileo, Tycho Brahe, and Kepler. Nor must be omitted his letters on Natural Magic, and his 'More Worlds than One, the Creed of the Philosopher, and the Hope of the Christian.'

Sir David's anonymous writings were nearly as numerous as those to which his name was attached, and they spread over a wider range of subjects. The elaborate treatises on Optics in the Edinburgh Encyclopædia and in the recent editions of the Encyclopædia Britannica are both from his pen, and to each he contributed the articles on Hydrodynamics and Electricity. In the older work he also wrote on Astronomy, Mechanics, Microscopy, and Burning instruments, while in the later work he turned his attention among other subjects to that of photography.

To the Edinburgh Review he contributed twenty-eight articles, which are comprised between the Nos. LVII. and LXXXI. They include biographical notices of such men as Davy and Watt; reviews of such philosophical works as Whewell's 'History and Philosophy of the Inductive Sciences,' Mrs. Somerville's 'Connexion of the Physical Sciences,' Lord Brougham's 'Discourse on the Study of Natural Philosophy,' and even Compte's 'Philosophie Positive:' they pass from Buckland's Geology or Daguerre's photogenic drawings to the lighter subjects of deer-stalking or salmon-fishing; they follow Sir James Ross or Sir George Back in their arctic researches, and describe the British lighthouse system or the phenomena of thunder-storms.

To the Quarterly Review he seems to have contributed five articles, and in them he gives his estimate of works by Babbage, Herschel, and Abercrombie; while the subjects he treats are as wide apart as the production

of sound, and the analysis of the intellectual powers, the supposed decline of science in England, and the philosophy of apparitions.

'Meliora' and the Foreign Review each contain two articles from his pen, one in the latter being a notice of Dutrochet's 'Observations sur Endosmose et Exosmose.'

But it was in the North British Review that the longest series of articles appeared. We have a list before us of seventy-six in the first thirty-nine parts of that quarterly serial, and we doubt whether the enumeration is complete. This shows that, on an average, Sir David wrote two of these literary productions for each part, and suggests the idea that he must have reviewed every book of note that he read. The first Number of the North British commences with an article by him on Flourens's 'Eloge Historique de Cuvier;' and further on in the same part he discusses the 'Lettres Provinciales' and other writings of Blaise Pascal. In the second Number he describes the Earl of Rosse's great reflecting telescope; and shortly we find him engaged with such serious works as Humboldt's 'Cosmos' or Murchison's 'Siluria.' The rival claimants for the honour of having discovered Neptune divide his attention with Macaulay's 'History of England,' or the 'Vestiges of the Natural History of Creation.' With Layard he takes his readers to Nineveh, with Lyell he visits North America, and with Richardson he searches the Polar seas. The Exhibition of 1851, the Peace Congress, and the British Association come in turn under his descriptive notice; or, turning from large assemblies to individual philosophers, he sketches Arago, Young, or Dalton. In one Number we have "The Weather and its Prognostics," and "The Microscope and its Revelations;" elsewhere he describes the Atlantic telegraph, whilst in a single article he groups together "the life-boat, the lightning-conductor, and the light-house." He reviews in turn Mary Somerville's 'Physical Geography,' and Keith Johnston's 'Physical Atlas;' the History of Photography engages him at one time, and at another Weld's History of our Society. Under the guidance of Sir Henry Holland he investigates the curious mental phenomena of mesmerism and electro-biology, and under that of George Wilson he inquires into colour-blindness. He criticises Goethe's scientific works, expounds De la Rive's 'Treatise on Electricity,' and Arago's on Comets; or, turning from these severer studies, he allows Humboldt to exhibit the 'Aspects of Nature' in different lands to the multifarious readers of the Review.

In addition to all this Sir David issued some pamphlets of a personal nature—controversial writings which some objected to as unnecessarily persistent, though it should be recorded to his honour that he was ready to profit by friendly remonstrance.

Few of his living companions will remember this Nestor in science otherwise than as a venerable form full of vivacity and intelligence, keenly alive not to physical questions alone, but to the various social, politi-

cal, and ecclesiastical interests of his time, and giving frequent indications of that humble faith in God which was the foundation of his character, and which brightened his declining years and the closing scenes of his earthly life. His many personal friends will retain his memory in their warm affection. Posterity will know him mainly for having opened up new regions in our knowledge of optical phenomena, and for having given a mighty impulse to science during two-thirds of the nineteenth century.—J. H. G.

Charles Giles Bridle Daubeny* was born February 11, 1795, at Stratton in Gloucestershire, third son of the Rev. James Daubeny, entered Winchester School in 1808, and was elected to a demyship in Magdalen College, Oxford, in 1810. In 1814, at the age of 19, he took the degree of B.A. in the second class, according to the old style of the Oxford Examinations. In 1815 he won the Chancellor's Prize for the Latin Essay, the prize for the English Essay in the same year being gained by Arnold.

Destined for the profession of medicine, he proceeded to London and Edinburgh as a medical student (1815–18). The lectures of Professor Jameson in Edinburgh on Geology and Mineralogy attracted his earnest attention, and strengthened the desire to cultivate natural science which had been awakened by the teaching of Dr. Kidd at Oxford. In Dr. Kidd's class-room the future historian of volcanoes had frequently met Buckland and the Conybeares, Whateley and the Duncans—men of vigorous minds and various knowledge. The change from thoughtful Oxford to active Edinburgh was the crisis in Daubeny's career. The fight was then raging in the modern Athens between Plutonists and Neptunists, Huttonians and Wernerians, and the possession of Arthur's Seat and Salisbury Craig was sternly debated by the rival worshippers of fire and water. Daubeny entered keenly into this discussion, and, after quitting the University of Edinburgh, proceeded, in 1819, on a leisurely tour through France, everywhere collecting evidence on the geological and chemical history of the globe, and sent to Professor Jameson from Auvergne the earliest notices which had appeared in England of that remarkable volcanic region†. Some of the views afterwards advanced by the young physicist touching the elevation of the hills and the geological age of the valleys of Auvergne‡ have been reexamined and discussed by later eminent writers, such as Scrope, Murchison, Lyell—not always in agreement with him, or, indeed, with one another; while the prehistoric antiquity of the volcanoes them-

* Extracted from a more extended Obituary Notice of Dr. Daubeny, read to the Ashmolean Society of Oxford, by Professor John Phillips, F.R.S., February 17, 1868.

† Letters on the Volcanoes of Auvergne, in Jameson's Edinburgh Journal, 1820-21.

‡ Transactions of the Royal Society of Edinburgh, 1831.

selves has been questioned even within a few years, and defended by none more effectually than by Dr. Daubeny*.

From the beginning to the end of his scientific career, volcanic phenomena occupied the attention of Dr. Daubeny; and he strove by frequent journeys through Italy, Sicily, France and Germany, Hungary and Transylvania, to extend his knowledge of that interesting subject. In 1823–25, he had by this means prepared the basis of his great work on volcanoes, which appeared in 1826, and contained careful descriptions of all the regions known to be visited by igneous eruptions, and a consistent hypothesis of the cause of the thermic disturbance, in accordance with the view first proposed by Gay-Lussac and Davy. Water admitted to the uncombined bases of the earths and alkalies existing below the oxidized crust of the globe, was shown to be an efficient cause of local high temperature, and a real antecedent to the earthquake movements, the flowing lava, and the expelled gas and steam. In later years† Dr. Daubeny freely accepted, as at least very probable, a high interior temperature of the earth; but he did not allow that the admission of water to a heated interior oxidized mass would account for the chemical effects which accompany and follow an eruption. On this point there are still data to be gathered and inferences to be examined.

Four years previously to the publication of the 'Description of Volcanoes,' Dr. Daubeny was appointed to succeed Dr. Kidd as Aldrichian Professor of Chemistry, and took up his abode in, or rather below, the time-honoured Museum founded by Ashmole. In these rather gloomy apartments nearly all the scientific teaching of Oxford had been accomplished since the days of Robert Plot; in them were still collected, as late as 1855, by gas-light and furnace-fires, the most zealous students of Practical Chemistry; but now they are filled with Greek sculpture, and Chemistry has flitted to the magnificent laboratories of the University Museum, directed by Sir Benjamin Brodie.

In 1834 he was appointed Professor of Botany, and migrated to the "Physic Garden," as it was called, which had been founded in the early part of the reign of Charles I.

Under his diligent and generous management, with liberal aid from the University, Dr. Daubeny lived to see the old Garden entirely arranged, enriched with extensive houses, extended in area, and made both attractive and beautiful.

In the pleasant residence at the Botanic Garden, Dr. Daubeny passed the remainder of his life—the third of a century. Here, incessantly active, he instituted many experiments on vegetation under different conditions of soil, on the effects of light on plants, and of plants on light, on the distribution of potash and phosphates in leaves and fruits, on

* Quarterly Journal of Science, 1866.

† "Memoir on the Thermal Waters of Bath," British Association Reports for 1864.

the conservability of seeds, on the ozonic element of the atmosphere, and on the effect of varied proportions of carbonic acid on plants analogous to those of the coal-measures*. These last-mentioned experiments are among the very few which can be referred to as throwing light on the curious question whether the amazing abundance of vegetable life in the carboniferous ages of the world may not have been specially favoured by the presence, in the palæozoic atmosphere, of a larger proportion of carbonic acid gas than is found at present.

A favourite subject of research with Dr. Daubeny, naturally springing from his volcanic explorations, was the chemical history of mineral waters. The presence of iodine and bromine in some of these formed the subject of a paper in the Philosophical Transactions for 1830; and a Report to the British Association in 1836 included a general survey of mineral and thermal waters. This subject was not neglected in his 'North-American Tour' (1837–38), which contains a great number of interesting observations on the character of the country which he traversed, as well as the educational institutions, where he was heartily welcomed.

Dr. Daubeny was a great traveller, almost an annual visitor to the continent, usually, at least in his later years, accompanied by some scientific or literary friend, some member of his family, or some young Oxonian of cultivated taste, to whom the sight of Auvergne and the Tyrol in the company of such a guide was a gift of priceless value.

In one of his journeys to Spain in 1843, for the purpose of studying the geological relations and agricultural value of the great phosphatic deposit of Estremadura, he was accompanied by Captain Widdringtoa, R.N. It was a journey prompted by benevolence and attended by hardship. No doubt, in some future day, railways will carry heavy loads of this valuable substance to enrich the agriculture of Spain†. In another year he might be found in Norway, or musing in the Garden at Geneva, where he was always welcomed by the great botanist whose friendship he gained in early life, and to whose memory he has devoted a careful critical essay, which was read to the Ashmolean Society in 1842‡.

It was at Geneva that he "began to estimate at their true weight the pretensions of Botany to be regarded as a science, and to comprehend the principle on which it might be inculcated as constituting an essential part of a liberal education." Here he first pursued his botanical studies under the guidance of Decandolle in 1830, and thus qualified himself for the Professorship to which, as already observed, he was appointed in 1834.

Chemistry, however, was the thread which bound together all the researches of Dr. Daubeny; not that he was personally a dexterous

* Miscellaneous Memoirs and Essays, 1867. British Association Reports, 1837–57.

† Memoir read to the Geological Society in 1844.

‡ Daubeny's Miscellanies, vol. ii. "On the Life and Writings of A. P. Decandolle."

manipulator of chemical instruments, though a diligent practical analyst. He was rich in chemical knowledge, profound and varied in his acquired views of chemical relations, always prompt and sagacious in fixing upon the main argument and the right plan for following up successful experiment or retrieving occasional failure. In 1831 appeared his 'Sketch of the Atomic Theory,' a work which well sustained the reputation of the author as a master of language and a conscientious teacher of science.

So soon as the arrangements were made for the location of Chemistry in its new abode Dr. Daubeny took the occasion of resigning the Chair of Chemistry, and used all his influence to increase the efficieney of the office and secure the services of the present eminent Professor.

In his position as a teacher of Botany, he took pleasure in drawing attention to the historical aspects of his subject, and specially, as a part of his duty, treated of Rural Economy both in its literary and its practical bearings. Hence arose the "Lectures on Roman Husbandry" (1857), written in a style very creditable to the classical training of his early years, and containing a full account of the most important passages of Latin authors bearing on crops and culture, the treatment of domestic animals, and horticulture. To this is added an interesting Catalogue of the Plants noticed by Dioscorides, arranged in the modern natural orders. This was followed, after a few years, by a valuable Essay on the Trees and Shrubs of the Ancients, and a Catalogue of Trees and Shrubs indigenous in Greece and Italy (1865).

To facilitate his researches in Experimental Botany, Dr. Daubeny had obtained possession of a piece of land lying some half a mile or so from Oxford; but of late years symptoms of ill-health interfered both with his enjoyment of the recreation of his little farm, and the experiments for which it was destined.

During a few late winters Dr. Daubeny found it desirable to exchange his residence in Oxford for the milder climate of Torquay. Here his activity of mind was equally manifested by public lectures on the temperature and other atmospheric conditions of that salubrious resort, and by experiments on ozone and the usual meteorological elements, in comparison with another series in Oxford. By this connexion with Devonshire he was induced to join the Association in that county for the Advancement of Science, Literature, and Art; and one of his latest public addresses was delivered to that body, as President, in 1865.

In his whole career Dr. Daubeny was full of that practical public spirit which delights in cooperation, and feeds upon the hope of benefiting humanity by associations of men. When the British Association came into being at York, in 1831, Daubeny alone stood for the Universities of England.

In 1856 he was its President, at Cheltenham, in his native county,

amidst numerous friends, who caused a medal to be struck in his honour—the only occurrence of this kind in the annals of the Association.

The same earnest spirit was manifested in all his academic life. No project of change, no scheme of improvement in University Examinations, no modification in the system of his own college, ever found him indifferent, prejudiced, or unprepared. On almost every such question his opinion was formed with rare impartiality, and expressed with as rare intrepidity. Firm and gentle, prudent and generous, cheerful and sympathetic, pursuing no private ends, calm amid jarring creeds and contending parties—the personal influence of such a man on his contemporaries for half a century of active and thoughtful life fully matched the effect of his published works. His latest labour was to gather his 'Miscellaneous Essays' into two very interesting volumes, and then, after patiently enduring severe illness for a few weeks, he sank to that rest which, often in his thoughts, had ever been expected, with the calmness of the philosopher and the hopefulness of the Christian. He died at five minutes past twelve A.M., December 13, 1867, in his 73rd year.

His remains were laid in a vault adjoining the walls of Magdalen College Chapel, in accordance with his own expressed wish "that he might not be separated in death from a society with which he had been connected for the greater part of his life, and to which he was so deeply indebted, not only for the kind countenance and support ever afforded him, but also for supplying him with the means of indulging in a career of life at once so congenial to his taste and the best calculated to render him a useful member of the community."

In the preceding brief notices no mention has been made of Dr. Daubeny's short career as a medical man, for which he had prepared himself by professional study in Edinburgh and London. In Oxford he justified his title of M.D. and his Fellowship with the College of Physicians by attaching himself to the Radcliffe Infirmary. In this capacity, however, he did not long remain; nor did he continue his medical practice, though during all his life the progress of medical science was much at his heart, as may be seen in the Harveian Oration which he delivered before the College of Physicians in 1845. In that elegant address he speaks of himself as "... quem, a medicinæ castris tanquam profugum, Physicarum Scientiarum amor, aut Otii Literati dulcedo, ad aliam vitæ normam jam tot per annos transtulit, ut ne inter commilitones vestros recenseri merear."

In these words we have the key to the valuable life which was passed so busily and so gracefully among his academic brethren, and to the works of scientific and literary interest which are all that now remain to us of Charles Daubeny. What he has said of these works is perhaps the truest and most modest comment that will ever be made on them and on the circumstances under which they were produced. For they are "some of the fruits of a

life chiefly spent in tranquil intellectual occupation, under the fostering wing of one of those great semimonastic establishments which are peculiar to this country; and however slight their intrinsic value, considered as contributions to the stock of human knowledge, may be, they will serve at least to show, by their number and variety, what might be accomplished by persons gifted with greater energy and more profound attainments, through the aid of foundations in which an exemption from domestic cares, and a liberal provision for all the reasonable wants of a celibate life, afford such facilities for the indulgence of either literary or scientific tastes."

Under the influence of the traditions of former scientific culture in Oxford, and

"Not mindless of those mighty times"

when the leading spirits of remote antiquity committed to posterity the priceless records of early philosophy, was Charles Daubeny conducted to the School of Chemistry, and the School of Geology. In them, but especially in the former, he imbibed sound and various knowledge. From them he passed at once to researches and publications which have contributed as much as those of any physicist of this century to sustain the credit of the University and guide the progress of useful knowledge. And the influence of these publications was in no slight measure due to the pure classical taste, and the sure employment of appropriate language, which were the gift of the foundations of William of Wykeham and William of Waynflete.

The same accuracy appeared in the frequent addresses which he was called on to make on social or public occasions. He affected no grace or oratory;

"His words succinct, yet full, without a fault,
He said no more than just the thing he ought;"

but the calm and reasonable views which he might be trusted to present on all subjects of scientific interest or administrative reform, never failed to have their due influence even over the agitations of controversy—from which he never shrank if his sense of justice and love of truth called for vindication. Any one accustomed to a considerable degree of intimacy with Dr. Daubeny would be able to declare that he never met with any man more entirely truthful and just-minded. You might absolutely rely upon him, in regard of deeds, thoughts, and motives. To convince his judgment was to enlist his sympathy and secure his active help; to be censured with overmuch strictness was a passport to such protection as he could honestly give. In defence of a friend whose Essay was unpopular, in opposition to a course of University mutation which he did not think was reform, in advocating what he believed to be desirable changes, his arms were ever ready; nor did he throw a pointless dart.

With reference to the influence of Dr. Daubeny in scientific discussions,

one may venture to say that it would have been greater had his early studies been more turned in the direction of mathematics, especially as applied to physical research. In the beginning of his career, indeed, Chemistry was only acquiring numerical exactness, and Geology was quite unprovided with mechanical laws of earth-movement. But no one knew better than Dr. Daubeny that right geometrical conceptions are always necessary to a student of science, and laws of proportion indispensable elements of sound philosophy.

The published writings of Dr. Daubeny are very numerous. Besides what have appeared as independent works, the list of his Memoirs in Transactions and Journals up to 1863, as given in the Royal Society's "Catalogue of Scientific Papers," amounts to seventy-too. Many of these, scattered through various periodicals and not conveniently accessible, were collected and arranged by their author in two volumes of Miscellanies. In this collection appeared twelve Experimental Essays, ten Geological Memoirs, eight Essays on Scientific Subjects, and twelve on Literary Subjects. Besides these were several papers of interest, some published separately, which, having been composed after the first edition of the 'Description of Volcanoes,' were employed in the preparation of the second edition, or noticed in supplements to that work.

By these arrangements Dr. Daubeny has rendered it unnecessary, for those who desire to know his views on the various subjects which occupied his mind, to refer to such publications as the Edinburgh Philosophical Journal, Edinburgh New Philosophical Journal, or Journal of the Geological Society, or even to the Linnean Transactions, Royal Society's Transactions, or Reports of the British Association, except from a desire to learn his first thoughts from his first words. The following is a list of the works which contain the principal results of Dr. Daubeny's scientific and literary labours:—

1. Description of Active and Extinct Volcanoes. 8vo, London, 1826. Second Edition, 1848. Several Supplements.
2. Tabular View of Volcanic Phenomena. Folio, thick, 1828.
3. Notes of a Tour in North America (privately printed). 8vo, 1838.
4. Introduction to the Atomic Theory. 8vo, 1852.
5. Lectures on Roman Husbandry. 8vo, 1857.
6. Lectures on Climate. 8vo, 1863.
7. Trees and Shrubs of the Ancients. 8vo, 1865.
8. Miscellanies on Scientific and Literary Subjects. 2 vols. 8vo, 1867.

JULIUS PLÜCKER, Foreign Member of the Royal Society, was born on the 16th of July 1801, at Elberfeld. After studying in the Gymnasium of Düsseldorf, and in the Universities of Bonn, Berlin, and Heidelberg, he passed some years in Paris. In 1825 he became a Privatdocent of Mathematics in Bonn, and in October 1828 was appointed Professor extraordinarius in that University. In 1833 he went to Berlin in the same capacity, and lectured also in the Friedrich-Wilhelm's Gymnasium. In 1834 he obtained the Professorship of Mathematics in the University of Halle, and in 1836 he was appointed Professor of Mathematics in the University of Bonn. The treatises and memoirs on Analytical Geometry written by him during the twenty years that followed his return from Paris secured for him a place among the first mathematicians of his time. He now entered upon a new career; for the superintendence of the Physical Museum having been entrusted to his care, he turned his attention to experimental research, and was appointed to the Professorship of Physics in 1847. A series of brilliant discoveries soon placed him among the foremost labourers in this department of science. These researches occupied him till 1856.

In repeating some of Faraday's experiments, he was led to the discovery of magnecrystallic action,—that is, that a crystallized body behaves differently in the magnetic field according to the orientation of certain directions in the crystal. These researches occupied him till 1856, when he turned his attention to the action of powerful magnets on the luminous electric discharge in glass tubes containing highly rarefied gas. In a wide tube the light of such a gas is too faint to permit a satisfactory observation of its spectrum; he found, however, that by employing tubes which were capillary in one part, brilliant light and definite spectra were obtained in the narrow part. These spectra were found to be characteristic of the several gases and to indicate their chemical nature, though the gases might be present in such minute quantity as utterly to elude chemical research.

In continuing these researches he next made the remarkable discovery of the two totally different spectra of each of the elementary substances, nitrogen, sulphur, selenium, hydrogen, iodine, lead, manganese, and copper, according as it is submitted to the instantaneous discharge of a Leyden jar charged by an induction coil, or rendered incandescent by the simple discharge of the coil, or else, in some cases, by ordinary flames. The two spectra were found to exhibit a difference in character, and are not merely different in the number and position of the lines which they show. This difference he attributed, with the greatest probability, to a difference in the temperature of the gas when the two are respectively produced. These results were made known in a memoir by himself conjointly with Dr. S. W. Hittorf, printed in the Philosophical Transactions for 1865. About this

time he resumed his geometrical investigations, but only lived to see the publication of the first part of the treatise upon which he was engaged.

He took an active part in the management of the University, having been twice Rector, frequently Dean of the Faculty of Philosophy, for many years Member of the Academic Senate and the Examination Commission. He was a Member of the Academies of Munich, Haarlem, Rotterdam, Lund, and Upsala, of the Société royale de Liège, of the Société des Sciences Naturelles de Cherbourg, of the Société Philomathique of Paris, Honorary Member of the Cambridge Philosophical Society, Corresponding Member of the Institute, of the Academies of Vienna, Göttingen, and the Physikalische Verein of Frankfort; his election as Foreign Member of the Royal Society was in 1855. The Copley Medal for the year 1866 was awarded to him for his researches in Analytical Geometry, Magnetism, and Spectral Analysis.

His separate works are:—

Analyseos applicatio ad geometriam altiorem et mechanicam (Bonnæ, 1824).

Analytisch-geometrische Entwickelungen (Essen, 1831).

System der analytischen Geometrie (Berlin, 1835).

Theorie der algebraischen Curven (Bonn, 1839).

System der Geometrie des Raumes in neuer analytischer Behandlungsweise (Düsseldorf, 1846, second edition, 1852).

Enumeratio novorum phenomenorum in doctrina de magnetismo inventorum (Bonnæ, 1849).

De crystallorum et gazorum conditione magnetica (Bonnæ, 1850).

Neue Geometrie des Raumes, gegründet auf die Betrachtung der geraden Linie als Raumelement (Leipzig, 1868, Erste Abtheilung).

He also edited a work by his former pupil, Professor August Beer, entitled "Einleitung in die Electrostatic, die Lehre vom Magnetismus und die Electrodynamik," left in manuscript by the latter at his death.

His papers in the 'Transactions' of the Royal Society are:—

On the Magnetic Induction of Crystals, March 26, 1857.

On the Spectra of Ignited Gases and Vapours, with especial regard to the different Spectra of the same elementary gaseous substance, conjointly with Dr. S. W. Hittorf, February 23, 1864.

On a New Geometry of Space, December 22, 1864.

Fundamental Views regarding Mechanics, May 29, 1866.

He is also the author of numerous papers on analysis, geometry, electricity, magnetism, physical optics, and spectral analysis, in Crelle's 'Journal,' Gergonne's 'Annalen,' Liouville's 'Journal,' Poggendorff's 'Annalen,' the Abbé Moigno's 'Les Mondes,' the 'Philosophical Magazine,' the 'Annali di Matematica.'

He died at Bonn on the 22nd of May, 1868.

JEAN BERNARD LÉON FOUCAULT, Foreign Member of the Royal Society, was born in Paris on the 18th of September 1819. He began the study of medicine, but soon gave the preference to physics and the sciences of observation. At the age of twenty he employed himself in improving the processes of photography. For three years he assisted M. Donné in preparing the illustrations of his lectures on microscopic anatomy, and was associated with M. Fizeau in conducting a variety of original researches. They investigated the comparative intensities of the light of the sun, of the voltaic arc between carbon poles, and of lime heated before the oxyhydrogen blowpipe. They read memoirs on the interference of calorific rays, on the interference of two rays of light in the case of a large difference in the lengths of their routes, and on the chromatic polarization of light. In December of 1849 Foucault described an electromagnetic regulator of the electric light. Conjointly with Regnault he was the author of a paper on binocular vision. He contributed besides several memoirs on colour, on voltaic and frictional electricity, and on the employment of the conical pendulum as a time-keeper.

M. Arago had suggested the employment of Wheatstone's revolving mirror, in a manner resembling its use in measuring the propagation of the electric current in a wire, to decide whether the velocity of light within a refractive medium is greater or less than its velocity in air. The former result implies the truth of the emission theory, the latter that of the undulatory theory. The experiment, as devised by M. Arago, was nearly (perhaps quite) impracticable, inasmuch as it depended upon the observation of an image of momentary duration formed in an unknown part of the field of view. By the happy introduction of a concave mirror having its centre in the axis of the revolving mirror, a fixed image was obtained; and the experiment thus rendered possible proved that the velocity of light is greater in air than in water. This experiment was made in 1850, not long after M. Fizeau had approximately determined the velocity of light in air by measuring the time it occupied in travelling from the place of the observer to a station 8633 metres distant, and back again. Foucault also suggested the means of measuring the velocity of propagation of radiant heat.

In February 1851 he communicated to the Academy the results of his observations on the rotation of the plane of oscillation of a freely suspended pendulum in the direction east-south-west, and thus supplied an ocular demonstration of the diurnal motion of the earth. By the construction of the gyroscope, in September 1852, he gave a second demonstration of the same phenomenon. For these discoveries the Copley Medal for the year 1855 was awarded to him. About this time he was appointed Physical Assistant to the Imperial Observatory. In September of the same year he exhibited a new instance of the conversion of work into heat. A copper

disk being made to revolve rapidly in its own plane, on bringing a horse-shoe magnet into such a position that the disk revolved with its rim between the poles of the magnet, the moving force required to maintain the velocity of rotation increased, and the temperature of the disk was raised.

On the 16th of February 1857 he described a reflecting telescope, having a speculum formed of glass coated with chemically reduced silver and afterwards polished, of 10 centims. aperture and 50 centims. focal length, without being aware that a telescope on the same principle and nearly of the same dimensions had been described by Steinheil in the Allgemeine Zeitung of the 24th of March 1856. In the following year Foucault succeeded in giving the speculum the form of a spheroid or of a paraboloid of revolution, and described a new process for finding out the configuration of optical surfaces. A reflector of this description, having an aperture of 40 centims. and 2·5 metres focal length, was mounted in the Imperial Observatory of Paris in June 1859. Another of these reflectors, having an aperture of 78 centims. and a focal length of 4·5 metres, was constructed for the Observatory in 1862. The polarizer known as his was invented in 1857.

The project of determining the absolute velocity of light in air with the aid of Wheatstone's revolving mirror, conceived in 1850, was carried out in 1862. The value Foucault obtained for it was 298,000 kilometres in a second of time, instead of 308,000 kilometres, the previously received value. Combining the newly found velocity with the constant of aberration, 20·445, the sun's equatorial parallax is found to be 8″·86, the value deduced by Mr. Stone in his recent discussion of the transit of Venus in 1769 being 8″·91, and the value adopted in the 'Nautical Almanac' for 1870 being 8″·95. In this year Foucault was elected a Member of the Bureau des Longitudes.

In the years 1863, 1864, 1865 he appears to have been occupied with the task of investigating the conditions of isochronism of Watt's governor, and modifying its construction so as to render the time of revolution invariable. These improved governors are applied to the transit-recorders constructed for the use of the Indian Survey. In January 1865 he was elected a Member of the Mechanical Section of the Institute. In 1866 he invented a new and improved regulator for the electric light, and a telescope for viewing the sun, in which the light is rendered endurable to the eye by coating the outer surface of the object-glass with a film of chemically reduced silver so thin as to be transparent. This process was applied with complete success to a refractor having an aperture of 25 centims.

In July 1867 he was attacked by paralysis, and died on the 11th of February, 1868. The date of his election as Foreign Member of the Royal Society is June 9, 1864.

Antoine François Jean Claudet was born at Lyons in 1797. He received a good commercial and classical education in his own country, and at the age of 21 he entered the office of his uncle, M. Vital Roux, an eminent banker, who a few years after placed him at the glass-works of Choisy-le-Roi, as director, in conjunction with M. G. Bontemps, the well-known glass-manufacturer. Eventually M. Claudet came to London to introduce the productions of Choisy. In 1833 he invented the machine now generally used for cutting cylindrical glass. For this invention he received the medal of the Society of Arts in 1853. But all this while he was a student of science training and waiting for the object to which his true life was to be devoted. The path was opened to him by the discovery of M. Daguerre.

In January 1839 that discovery was first *announced* to the world, and specimens of the results were exhibited, the *modus operandi* being still preserved secret. The French Government at once entertained the project of rewarding the discoverer, and in the following June assigned to M. Daguerre a pension of 6000 francs annually, and to M. Nièpce, jun., a pension of 4000 francs annually, that the new art might be presented *a gift to the world.*

In the month of August 1839 the new discovery was *published* to the world. It was received with enthusiasm, and rapidly adopted as a means of delineation, portraiture being its most early and extensive application. England alone failed to partake freely of this "gift to the world," M. Daguerre having entered into negotiations which secured a patent in this country whilst the question of his claims was under the attention of the French Government. M. Claudet became the possessor of a part of this patent, and commenced in 1840 the practice of portraiture in the Adelaide Gallery, where his studio remained for many years. There, as a zealous worker, he devoted himself to the improvement and development of photography, perfecting known processes and inventing new ones. His earliest contribution to the art was a mode of obtaining vastly increased sensitiveness by using chloride of iodine instead of iodine alone. His paper on this subject was read before the Royal Society in June 1841 ; and, by a curious coincidence, it followed Mr. Fox Talbot's description of his own photographic process, the calotype. From this period till his death his contributions to photographic literature were copious and interesting, the idiomatic excellence and elegance of his English being remarkable.

In 1847, discussing the properties of solar radiation modified by coloured glass media, he made a bold attempt to lay the foundation of a more complete theory of the photographic phenomena, and he was rewarded by the publication of his paper in the Philosophical Transactions, and by his subsequent election (in 1853) as a Fellow of the Royal Society. At this time the collodion process had supplanted the method of Daguerre ; and Claudet was one of the first to appreciate and adopt it.

The marvellous phenomenon of objects in relief was now brought before him in the stereoscope, and seemed to him a greater charm than the ex-

quisite detail of the Daguerreotype. He assisted Sir Charles Wheatstone in the early application of the stereoscope to photography; and in his admirable treatise on the stereoscope he gives the history of the art and the theory of the principles of binocular vision. His great aim was the elevation of photography by rendering her work scientifically true; and the Reports of the British Association during a period of twenty years bear ample testimony to the ingenuity and originality of his inventions. His dynactinometer, his photographometer, his focimeter, his stereomonoscope, his system of unity of measure for focusing enlargements, his system of photosculpture, and other results of his experimental researches are familiar to most photographers.

In the later years of his life he became convinced that one of the greatest deficiencies of photography, in the representation of solid figures, is the incapability of obtaining an equally well-defined image of all the various parts situated on different planes. Hence it became his object to remove from photographic portraiture the mechanical harshness which marked and marred the plane situated in the exact focus of the lens, and so to produce, as in the best works of art, a uniformly soft and harmonious treatment. His success in the first instance was partial, inasmuch as the adopted motion of the posterior lens only of the optical combination slightly altered the size of the superimposed images, and thus introduced a theoretical, though hardly visible, amount of blurring. Dr. Sommer, M. Voigtlander's stepson, supplied a series of formulæ showing that, although for all practical purposes in photography the movement of one lens attained the object in view, yet the simultaneous motion of the two lenses, receding from or approaching a fixed point between them, was the only legitimate mode of reconciling practice with theory, and of securing in every plane an exact uniformity of image. To fulfil this condition was a difficult problem, the solution of which was most perplexing. But, says Claudet, with a determination which commands success, "I did not like that it should be said my plan was not entirely in accordance with the mathematical laws of optics, and I set to work to find a mechanical means by which I could avail myself of the calculations of Dr. Sommer. I have found such means; and it proves that the differential movement can be effected, not only as readily, but with a greater command and steadiness than by moving only one lens." His ingenious automatic arrangement is described in his last paper read before the Royal Society, in 1867, and published in the Proceedings, entitled "Optics of Photography: on a Self-acting Focus-Equalizer, or the means of producing the Differential Movement of the two Lenses of a Photographic Optical Combination, which is capable, during the exposure, of bringing consecutively all the Planes of a Solid Figure into Focus, without altering the size of the various images superposed."

After this, and in the same year, he had an interesting correspondence with his veteran collaborateur Sir David Brewster, who held that the most perfect photographic instrument is a single lens of least dispersion, and

least aberration, and least thickness. Claudet realized these views in his portraiture with a small topaz lens, which reached with equal distinctness every plane of the figure. He then communicated the nature and result of his experiments to the British Association at Dundee; and his work was done. His last illness, in December 1867, was of very brief duration. He suddenly passed away from us, in the 70th year of his age, while his mental powers retained the vigour and freshness of youth; and by his death photography lost a father, and very many photographers a friend.

The scientific life of Claudet is given at length in a "Memoir" published in the 'Scientific Review,' and reprinted for distribution at the Meeting of the British Association at Norwich in August 1868. In an Appendix there is a list of forty papers communicated from 1841 to 1867 to the Royal and other Philosophical Societies and to photographic and philosophical publications in England and France. Here also we have a striking portrait of this zealous photographer, obtained with his Focus-Equalizer, and printed from the only negative preserved when his "Temple to Photography" in Regent Street was destroyed by fire, "a few weeks after its chief priest had quitted it for ever."

In recognition of his merits M. Claudet received awards of eleven medals, including the Council Medal of the Universal Exhibition, 1851, besides that, being on juries, on other great occasions he was excluded from the awards. He was elected a Fellow of the Royal Society in 1853, and in 1865 he was made a Chevalier of the Legion of Honour.—J. B. R.

Charles James Beverly, F.R.S., F.L.S., was born in August 1788, at Fort Augustus in the Highlands, where his father's regiment was then quartered. He entered the Navy in 1810 as Assistant Surgeon, and was employed in that capacity during four years on the Baltic and Mediterranean stations, but chiefly the latter, in H. M. SS. 'Pyramus,' 'Resistance,' and 'Caledonia,' during which period he was frequently sent in boats on cutting-out expeditions, and was present at the capture of Porto d'Anzo in 1813. He was then placed on Lord Exmouth's list for promotion, but, falling into bad health, was sent to England in charge of sick and wounded from the fleet.

On his recovery he was appointed to the 'Tiber' as Assistant Surgeon, and served in that ship till 1818, when, upon a strong recommendation, he was selected by the Admiralty to be Assistant Surgeon in the 'Isabella,' then about to proceed to the polar regions under the command of Sir John Ross. In 1819 and 1820 he served in Sir Edward Parry's first expedition, and passed the winter at Melville Island, discovered in that well-known voyage. On his return he was promoted to the rank of Full Surgeon, having seen more than ten years' service in sea-going ships as Assistant Surgeon, and being highly commended for his skill and care in his attendance on the sick. He subsequently suffered from an affection of his eyes, and immediately on his recovery was nominated most unexpectedly

to the Flagship on the Barbadoes Station as Supernumerary Surgeon. The risk of changing from an arctic to a tropical climate while in weak health forced him to decline the appointment, and he was removed from the list of surgeons. He served in 1827 as a volunteer under Sir Edward Parry in the capacity of Surgeon and naturalist in the long and perilous ice-journey on the Spitzbergen seas. He was elected a Fellow of the Royal Society in May 1831.

After retiring from the Navy, Mr. Beverly entered into private practice in London. He died on the 16th of September, 1868, a short time after attaining the age of 80.