Ever truly Yours
Tho. Graham

THE LIFE AND WORKS

OF

THOMAS GRAHAM, D.C.L., F.R.S.

ILLUSTRATED BY 64 UNPUBLISHED LETTERS.

PREPARED FOR THE GRAHAM LECTURE COMMITTEE OF THE GLASGOW PHILOSOPHICAL SOCIETY

BY

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EDITOR'S PREFACE.

THE Graham Medal and Lecture Fund was established in 1878 by the leading manufacturing chemists of Glasgow subscribing a sufficient amount of money, the annual interest of which was to be employed by the Chemical Section of the Philosophical Society of Glasgow in the encouragement of research, the name of Graham being coupled with this fund on account of his being a Glasgow man and a great chemist, whose aims and methods are worthy of imitation.

The first Graham Lecture was accordingly devoted to the life of Graham as an investigator, and was delivered in 1879 by Mr. Chandler Roberts, F.R.S.

The Committee did not think it then necessary to devote further lectures exclusively to the life and work of Graham, but it was pointed out to them by the late Dr. James Young of Kelly that a full life of Thomas Graham had never been written, and that a large collection of his private letters were in the hands of Dr. R. Angus Smith, awaiting a convenient opportunity for publication. Communication was then made with Dr. R. Angus Smith, who expressed an opinion that extracts from these letters would be of sufficient interest to justify the Committee in adopting them as the basis of the second Graham Lecture, and that he would be pleased to deliver this lecture for the Committee.

A letter was received on the 2nd February, 1884, in which Dr. R. Angus Smith says:—"As to the Graham Lecture, I shall endeavour to describe it. It will consist of—*First,* Preface about Glasgow — fragmentary, very fragmentary. *Second,* An introduction, trying to describe Graham's surroundings in Glasgow, also very fragmentary. *Third*, A long series of extracts from letters by Graham, from his earliest college days to his later years. This will constitute the pith and novelty of the paper, and it is this which I desire to have published in connection with his writings as abridged by me. *Fourth*, The abridgment of Graham's writings. It will not be in the artistic form of a lecture, but it will contain matter to be preserved, and it will be as it stands a life of Graham, fragmentary, it is true, but capable of being added to, and the additions I may be able to make as it is passing through the press."

The lecture was fixed for the 20th February, 1884, but the hand of death was already upon Dr. R. Angus Smith, and, amid much feebleness and effort, the MS. was transmitted to Glasgow a few days before, and read to the Society by Professor Ferguson. Dr. R. Angus Smith was born twelve years after Graham, and died 11th April, 1884, and some of the last moments of his busy life were employed in putting together these memorials of his early friend, who may be called appropriately the "Scotch Faraday," and who was certainly one of the most distinguished workers in chemical science of the present century—Dr. R. Angus Smith himself being not the least notable.

Dr. Smith's manuscript has required a few editorial remarks, which are enclosed within brackets; and it may be mentioned that the Editor has not had the opportunity of comparing the proofs with Graham's original letters, but only with Dr. R. Angus Smith's transcripts of them.

J. J. C.

LIFE AND WORKS

OF

THOMAS GRAHAM.

WHEN I think of Thomas Graham I go back to a period in which, it seems to me, Glasgow was by no means a remarkably scientific spot; and yet there were individuals there with great scientific insight, and who have from time to time in twelve hundred years shone brightly in it.

Fame has made the sap of this community the spirit of Kentigern; but there is always a beginning before the one that becomes the most prominent, and St. Ninian was earlier. It is from the community at the High Church, once a Cathedral, that historians, and notably Skene, have drawn the line of active life that has become the Glasgow that we see. It is interesting to me to think whether the teaching of those early men have really influenced the community to a great extent. Days of savage darkness came over the country soon after Kentigern's death, but there still seems to have been a memory of the good deeds of him and his unknown successors five hundred years after his death, when the thought came into life in 1150 that Glasgow had once been a centre of civilization, and ought still to be able to act as one. So remarkably do the laws of thought act upon man, so fully are we influenced by tradition, so ready are we to seek the smallest and most illegible sign-post, that the savagery and confusion of half a millenium of misery were forgotten, and the faint tracing of blessed life was enough, at the end, to re-establish the city with even more of its original importance. Even in the next better-known three hundred years, amid difficulties which nature and man seem to consider essential to the development of civilization, there was such an advance that it was thought advisable, even by the pope of the time, that a university should be established in Glasgow, being a place well suited and adapted to that purpose on account of the healthiness of the climate and the abundance of of food, as well as being an Episcopal city. The university was to be a place "adorned with the gifts of science, that it may produce men distinguished for ripeness of judgment, adorned with virtue, and skilled in different sciences; and that it may be a fountain of knowledge from the fulness of which all desirous of being instructed may drink liberally."

The University at the beginning of this century had Dr. Thomas Thomson, one of the most enlightened chemists in Europe—I do not say the most original, but active and industrious he certainly was. He was far behind his contemporary, John Dalton, in vigour of thought, but he had a more brilliant vision and a greater power of expression; and chemists owe him much for seeing so closely those laws which have made chemistry into a science. He was Graham's teacher.

Thomas Graham was born in Glasgow, 21st December, 1805—Faraday being then fourteen years old—and went first to a preparatory school in 1811, thence to the Glasgow High School in 1814, where he studied classics under Dr. Dymock and Dr. Chrystal. He subsequently studied at the University from 1819 to 1826, taking his degree as M.A. in 1826.

His taste for chemistry was very early developed, even before he left the nursery; his amusements when 6 or 7 years old were making gas in tobacco pipes, making eggs move with quicksilver, and other little tricks of the kind.

[Graham's father was a merchant of good position in Glasgow, and the boy was originally intended for the church. His strong predilections for a scientific career, however, caused him to shape out a path for himself, much to the grief of his father, who, probably not understanding the aims and scope of science, was prejudiced in his judgment. That such was the case was proved by the sequel. Although Graham eventually became possessor of his father's property and a comparatively rich man, in these early days he was poor; but his letters at this time to his mother and sister Margaret show a gentle and courageous spirit, sustained by lofty ambition, which make them as literary productions peculiarly interesting.]

Immediately after obtaining his degree at Glasgow in 1826 he went to Edinburgh, where he pursued his scientific studies under Dr. Hope, and enjoyed the friendship of Professor Leslie.

One of his earliest letters from Edinburgh, dated June 10, 1826, contains the following curious sentences :—

"TO MRS. GRAHAM.

"From Edinburgh, 10th June, 1826.

"—— The Professors here are all at logger-heads with each other. Leslie calls Hope in his class-room 'the showman in the other corner,' while Dr. Hamilton has just received £500 from Hope for defamation."

[Graham's letters dated from Edinburgh are here printed in the order arranged by Dr. Angus Smith, with a few connecting remarks.]

Edinburgh, 22nd June, 1826.

MY DEAR MOTHER,—I must request Henry to deliver the enclosed letters. The only one which he is not acquainted with is Muir's. Let it be handed in to Muir's warehouse, which is I believe under Rennie's school. It is requesting Muir to send down to Henry the Chemical Society Minute Book, as I have at last succeeded in establishing a similar institution in Edinburgh. If Henry gets the Minute Book sent down to him, which I am rather afraid he will not, as Steel has it probably away with him, he will be so good as to send it to me by coach before the end of next week. It will be too bulky to go into the office parcel. I hope you will be merciful to me in not answering your letters quickly, as I singly have such a number of correspondents, and have before me a vast deal of writing work. I opened the new Chemical Society last Saturday with an essay, and am appointed to deliver soon in the Hunterian. I am quite well-known here now. My best respects to Grandmother and Aunt. Will you make my correspondents be more particular in telling how grandmother is getting on.

I remain,

DEAR MOTHER,

Yours sincerely,

THO. GRAHAM.

Edinburgh, 7th July, 1826.

My Dear Margaret,—I am much obliged to you for your long and elaborate epistle. Till this moment I have not had the smallest leisure to answer it. My first and introductory essay in the Hunterian went off with great *eclat.* On the motion of Dr. Brown (the poet of Stirling), a vote of thanks was given for it. This must be a very good thing, if we are to judge of it from its rarity, as it is the first time which it has occurred in the Hunterian. The best part of the vote was "and that Mr. G—— be solicited to make his essay public"! However, I have no intention of publishing till I accumulate matter, not for an article, not for a pamphlet, but for a book—God knows when that will be.

As for Edinburgh news, I can send you very little. Hamilton, the bookseller, says that Constable's affairs are very bad—that he has really been in an insolvent state for the last six or seven years. For several years he has been paying at the rate of £5,000 a year for mere discounts—a circumstance which of itself was enough soon to have ruined him. The editors of the *Edinburgh Medical Journal* and *Jameson's Scientific Journal* have deserted him, and taken to other publishers. His *Edinburgh Monthly Magazine* has been given up. A composition of 3s. 6d. in the £ is talked of. Sir Walter Scott will get on; he has been made King's printer—a situation worth £3,000 a year, chiefly in salary. Sir Walter must have been rather a calculating genius. It turns out that he was almost sole proprietor in the printing establishment of James Ballantyne & Co., who have failed; so that he intended not only to profit by writing the novels, but also to have a good share in the profits of printing them. Ballantyne was little more than his servant. I have not had time to read "Woodstock" yet. Sir Walter has in some measure satisfied the cravings of his creditors by binding over to them "Napoleon," and several forthcoming novels. The estate of Abbotsford is really not Sir Walter's, it is his son's; it was bought with Lady Scott's money, who had £70,000, and settled upon *her* heirs, so that it is in no danger. Lady Scott was a Frenchwoman.

Bentress is going to Paris in April next; he confesses that his estate is worth £1,500 a year. He and I were down at the glass-house getting instruments constructed. He has been so often there about his *Gravimeter* that he is as well known (he says) as a pickpocket. We had glorious fun the night he brought out his instrument in the Hunterian—he was so afraid of a public appearance. I followed, and eulogised his instrument. In return, when he heard that I was to read on "Liquidity," he got out all books possible on the subject, and was telling MacDowal that he wished to God that he would be able to make a speech for Gra-ham (two syllables). . . . You may assure Bell that the washerwomen are such blockheads here that they did not put an atom of starch into my shirts—horrible! horrible! . . . We have had several showers here, and even a wet day or two, but the rain has scarcely made a sensible impression.

How are the Miss Campbells? How are all the Glasgow young ladies? Alas, the want of them is felt most sensibly here; as Miss Brown (Mitchell's landlady) says, the gentlemen turn savages when they go out to the West Indies for want of them. Alas, that I should become

Your most loving *Indian Savage,*

THO. GRAHAM.

Edinburgh, 1st August, 1826.

MY DEAR MOTHER,—I had not seen the *Annals of Philosophy* since I came to Edinburgh, so that I was not aware, till congratulated upon the circumstance by him (Johnston) that a certain paper of mine had been republished in the number of that work for July. I have sent a copy for you. The *Annals of Philosophy* is a London publication, and is the leading scientific journal of the country. It is published monthly, and is 2/6 a number. You will find in the number enclosed articles by Dr. Christison, a Professor in the College here, by Mr. Christie, a Professor at Woolwich, by Dr. Hugh Colquhoun, Professor Thomson's nephew, an ingenious young chemist, by Sir H. Davy, page 62, etc; it is therefore a good introduction. I also trouble you with the inaugural discourse of a young M.D., a friend of mine. You need not care although his lairdship [Graham's father] see the foregoing.

Edinburgh, 21st September, 1826.

MY DEAR MARGARET,—I have been writing a good deal of late and experimenting too—although from Langstaff's absence and other circumstances, not in the laboratory. In the investigations, however, in which I have been been latterly engaged, a laboratory is by no means necessary. My essay in the *Annals* seems to have had some effect, as it will appear from a quotation which I am about to make from the No. of that Periodical for the present month, p. 100—the author is a Mr. Bailey, and he is criticising Dr. Hugh Colquhoun's paper "The accurate reasoning of Mr. Graham, I may also remark, in his observations on the absorption of gases by liquids (quoted in the same number of the *Annals* in which Dr. Colquhoun's paper appears), as well as the experimental evidence he cites, is entirely favourable to this view, etc."

It may be proper to mention that that essay forms a part, and only a small part, of a much greater investigation in which I am engaged, which I hope will all be equally successful. Perhaps in a month or two I will have something additional in the *Annals*. It is this circumstance which makes me less caring about being baulked at the Mechanics' Institution. Had I got that situation, the overwhelming labour which would have accompanied the proper and successful discharge of its duties, would have been amply sufficient of itself—a labour, too, which would be of very little use to myself—whereas the labour in which I am at present engaged in following out my theoretic speculations, may eventually be even more productive and profitable—certainly more honourable and productive of scientific distinction. Clark, my antagonist, has never got the length of the *Annals*.

Edinburgh, 11th Oct., 1826.

My Dear Mother,—The basket has been delayed from day to day in the daily expectation of the journal which accompanies it, and which contains another article, as you will see by the contents. The journal was printed in London ten or eleven days ago, but owing to a passage, per smack, of nine days, did not arrive here till to-day. The next number will probably contain a notice of a curious discovery of Miss C. and me in Baking. The present article, although short, is one of considerable importance. There is nothing that I wish more than that somebody should oppose the theory which it contains, as a discussion of that kind is the very thing to make one known.

Before the laird's burst of eloquence arrived—conceiving that I was fairly set upon my own pins—I had commenced a little chemical work, for which I think there is a call. I can give you here no more than its title page.

THE LANGUAGE OF CHEMISTRY:

OR A

Definition of the Term of the Science,

Systematically Arranged, Developing its Principles

and Object.

Edinburgh, Oct. 15, 1826.

My Dear Mother,—My operations have been interrupted for two or three days. I find a room, which Miss Cameron occupies at night as a sleeping room, and which is otherwise unoccupied, forms a very good laboratory for the experiments in which I am engaged, and likely to be engaged for a considerable time. They are doing very well.

Edinburgh, Nov. 7, 1826.

My Dear Mother,—I send you, by Mr. Stirling, another number of the *Annals* having a short notice of mine in it. I am glad to hear by him that you are all well. You could not be better than we are here. Did Mr. A. Mitchell call with my letter and Miss C.'s, which was worth half a dozen of mine?

The students are flocking in here in great numbers, but no classes have opened as yet, except the medical. My laboratory is getting on quite to my liking.

"The Language of Chemistry" has been laid on the shelf for some time. We are tiring to death here for want of letters from Cloverbank. At the beginning of next week be so good as to send Bell down to Howie's, the carriers, for a tin box, carriage paid, which I hope will contain a more faithful and full representation of matters than this scrawl.—I am, with much respect,

Yours affectionately,

THO. GRAHAM.

Edinburgh, 20th November, 1826.

My Dear Mother,—Although I have not achieved a great-coat, yet I am rather comfortably accoutred in a pair of "gentleman's lamb's-wool pantaloons," *alias* long-legged worsted drawers. My particular studies are going on very favourably, although I was obliged to lay out a little in furnishing my home laboratory, yet it will be easy to make it all up.

The town is filling very quickly. All the classes, nearly, having opened, and the Courts having sat down for the winter. As for myself, I neither see much of the bustle nor care for it. The more secluded I keep myself from company till I have acquired some standing in science the better. The lassie here is very convenient for such interruptions, for whenever she knows that I am engaged she takes it upon herself to say that I am from home —very much to my satisfaction in general.

Edinburgh, 20th November, 1826.

My Dear Margaret,—Mr. Dunn, a philosophical instrument maker, cheated me of the opportunity of having an article in the next number of the *Annals*, by making me wait for ten days or a fortnight for some very minute weights which he was making for me and which were necessary. Professor Brande has done me the honour of giving a very favourable account of my first article in his *Quarterly Journal of Science* for October, but which I did not see till a few days ago accidentally upon the table of the Writers to the Signet Library. The *Annals* were lying there too. I have several articles on the stocks—but more of this anon.

Dr. Hope intends to give another lady's class this winter. I have heard him lecture once or twice, and set him down for a complete puppy.

Edinburgh, Dec. 20, 1826.

My Dear Mother,—Dr. Ritchie, the Professor of Divinity here, has been doing all he can to prevent Dr. Chalmers from succeeding him. With this purpose he continued to lecture till a few days ago, although he was not heard in the first bench. One day a gentleman, who passes under the name of Dr. Syntax, and attends all the classes, came in. He was applauded upon his entrance, but considering this as meant to insult him, he made a long speech to the students during the lecture, which Dr. Ritchie is so deaf that he did not notice, so that the students were enlightened by two lectures at once. At the conclusion of the lecture Dr. Syntax stepped up to the professor and advised him in an audible voice to resign his situation, for no one heard a word that he said. Principal Baird has latterly been reading his lectures.

The Hunterian Society had an anniversary dinner at which several of the professors were present. I had been pitched upon to toast Professor Leslie, who was to be there, but was awfully cheated of my speech. For just as I was about to rise and deliver my most eloquent speech, Professor Leslie was obliged to leave the room upon business. Mortifying, indeed!

[During this year he was occupied with three papers—(1) "On Absorption of Gases by Liquids;" (2) "On the Heat of Friction;" (3) "Alcohol Derived from Fermentation of Bread."]

Edinburgh, January 29th, 1827.

MY DEAR MOTHER,—Johnston brought me a letter from Steel, and two letters of introduction from Dr. Thomson to Professor Leslie and to Dr. Turner, Lecturer on Chemistry here. Both of them I have delivered, and been very kindly received indeed.

I found Dr. Turner in his house, Pitt Street, New Town. He is a very gentlemanly young man, apparently not above thirty. He is chemical editor of Dr. Brewster's *Scientific Journal*, and has written a good deal on chemistry. We were conversing together for more than an hour. He showed me a work which he has very nearly finished, and which will be published in a very short time —"Elements of Chemistry"—in one thick volume. He intends to have a ladies' class for chemistry this winter in the Assembly Rooms, instead of Dr. Hope. During my stay his sisters came in, and were jesting about the method in which they had been carrying round the hand-bills about these lectures in their muffs, and leaving them at music shops, and wherever they called. Dr. Turner promised to return my call, and hoped that we would become well acquainted.

Professor Leslie resides in Queen Street, and there I found him sporting a dressing-gown. There was even a longer confab here than in the former case. He is a bachelor, and his drawing-room has a good deal the appearance of a laboratory, so that there were plenty of instruments to show and discuss. As I suggested to him a variation and improvement in a favourite process of his, he was so good as to offer me the use of his instruments to perform it. He also requested me to prepare some chemical things for him, for I find that although an excellent mathematician and natural philosopher, he is no great chemist. He also invited me to step into his class and his laboratory whenever I chose, and to call at his house. He took my address. As Dr. Thomson's letters were sealed, I did not see what they contained, but I should think that they were very favourable, as the first question that both Professor Leslie and Dr. Turner asked was, if I was intending to give a course of lectures. I am much obliged to Dr. Thomson for these letters, as they give one an excellent introduction to the scientific society of Edinburgh.

Steel, in his letter, without any commands from me, had enclosed 15s. 6d., my share of the Chemical Society's funds. It came very opportunely, although I would rather that it had never come out of Glasgow.

Edinburgh, January 29, 1827.

My Dear Margaret,—The ginger-bread was excellent. Mr. Johnston got away the last of it as a supply for his Durham journey, thinking greatly of it from the scientific principles upon which it had been baked.

I was surprised the other day by the receipt of the card which I have enclosed for your profitable edification—from Chalmers the bookseller, the Doctor's brother. I have been led to understand that it arose from something which Mr. Dunn, the optician, had said to Mr. C., of and concerning your humble servant. Upon calling upon Mr. C. as requested, I found that he was in a great dilemma for material for the new popular journal of science which he is setting about, and the plan of which he borrowed from our Chem. Society, and that he wished me to give him some papers—offering six guineas a sheet of 24 pages, 12mo; the intended magazine being of small size, containing 60 pages for one shilling, to be published monthly. As I might publish anonymously in it, I told him that I would think of it.

By the Powers! a very good method of adding two or three guineas a month to one's income. What he wants is quite different from what would go to the Annals, so that they do not interfere, and, as I wont put my name to them, the L. can know nothing about it. Although I have been otherwise very throng, I have sent him two or three short things, and will send him some more for the first No., which will not appear for near a month yet. I suppose he pays when the articles are printed.

Another great advantage of it is that, for the purposes of the Magazine, Chalmers receives all the scientific journals, British and Foreign, and submits them to his contributors. The obtaining a reading of them the moment they come out is what I would have given anything to compass.

Notwithstanding my denial, some of my friends maintain here that I am the Editor of the Magazine. Where they got the information I know not, as I never told them anything about it.

Hoping that you will be satisfied for a season with the above news, allow me to subscribe myself on this side of the sheet,

Your affectionate brother,

THO. GRAHAM.

Edinburgh, Feb. 23rd, 1827.

MY DEAR MOTHER,—I was surprised yesterday by a visit from J. Renwick, with parcel of provisions and packet of letters. He called when I was out looking after a situation for Mr. M. Miller. Mr. Miller is rather troublesome with his situations, and I am determined not to put myself to much trouble in regard to them; for, after having, with a good deal of manœuvering, secured this situation for him, he declined it. It was a private classical academy in Newcastle, like Johnston's, and which was either worth, or might easily be made worth £150, while he is canvasing away here for a Lancastrian school not worth £80, and not half so respectable.

The publication of *Chalmers' Miscellany* has been rather hindered for a few days from his absence, occasioned by the death of his mother, but it will be out by 1st March. During his absence Mrs. Chalmers had the charge of affairs, and I was prime minister. Mrs. C. very much regretted her mother-in-law. She was upwards of 90, and was perfectly sensible. Young Mrs. C. had a letter from her within six weeks of her death, with a present of stockings for the children, of her own knitting. The doctor was her fourth child, and our Mr. Charles C. among the youngest. Her oldest son has been all along in London. The doctor left his class, and was with her at Anster, in Fifeshire, during her last illness, which was not severe, and only ten days. Mrs. C. said that she had a great care for all her sons' wives, and liked to bring them all together about her.

Mr. Renwick stayed here last night. I find upon looking at the box of instruments that they are in a very bad state. I have never opened them since I came here. Perhaps some of the deficient instruments may be lying about the house.

Edinburgh, Feb. 27th, 1827.

My Dear Henry,—You will see that Chalmers' concern is printed. The woodcut on the title page was rather got up in a hurry, but it is meant for a head of Dr. Franklin. Some say that it is very like Chalmers himself, and that will probably be the prevalent theory among the knowing ones at Glasgow. It is rather thin, but he intends after he has printed a volume to add a sheet and half more to it, and charge 1s. 6d. My signature is G.

There is one of my suggestions which you will find under the head of "A Valuable Appendage to Kitchen Fire-places," which has wondrously caught the fancy of our *Ebony*. He says that it is calculated to produce a complete revolution in the sublime science of cookery—that all the blue-stocking ladies of Edinburgh to whom he has mentioned the improvement are captivated therewith, and that a thousand and one sand baths are forthwith to start into being. There must, he says, be another and a long article upon it in the next number, and Mrs. Chalmers has been so kind as to furnish me with Dr. Kitchener's "Cook's Oracle," in order that the article may be garnished with apt quotations and "modern instances." I shall probably devote a couple of hours to it some time this week.

These, however, are merely subordinate pursuits, and neither they nor their fruits are what I aim at. Eminence purely scientific is more philosophical, more honourable, and more desirable in my eyes. But leave it to time.

Edinburgh, March 15th, 1827.

MY DEAR MOTHER,—The other day I got £5 from Chalmers for contributions to his magazine. It was quite a Godsend. It certainly came very seasonably, as I do not expect a remittance from the L. for a fortnight, if at all. I have had no letter from him. I have great pleasure in sending the little presents which accompany this, as the first-fruits of Authorship. The expense of them, I can assure you, was a mere trifle. They would have been better, had it not been for the temptation of adding some conveniences to my laboratory. The ring which I request you to accept of, you will perceive is for hair. Just start out the glass front, by putting the point of a penknife into one of the ends; and the hair is put where the silk is at present. It is quite easily done, and there is no danger.

Do not make yourself uneasy about the L. sending money, for, if I was applying myself to it, Mr. Chalmers is quite willing to give me as much of the magazine to write, as would yield me five guineas a month, which is more than I get from the L.

Edinburgh, March 15th, 1827.

MY DEAR MARGARET,—I saw Prof. Leslie yesterday, and he says that his ladies' class has succeeded beyond expectation, and keeps him tremendously busy, and he introduced a good deal of chemistry for their amusement, in which he is by no means an adept.

I intend to have Dr. Turner at breakfast some of these days, and to introduce Bentress to him, as Dr. T. studied at Gottingen under the celebrated Stromeyer, and Mr. B. intends eventually to land there.

Edinburgh, April 16th, 1827.

MY DEAR MOTHER,—I have had a good deal of intercourse with Dr. B. [Brewster], chiefly, if I recollect, since my last letter to Henry by Mitchell's parcel; indeed, it was by a mere accident that Miss Cameron's lodgings escaped a visit from this doctor of encyclopedic renown. Bentress and I were promenading Princes Street upon some business or other, when we met with, and were hailed by, Dr. B. He was on his way with the tickets of admission to Sir W. Hamilton's phrenological lecture for us. We had then a walk, arm-in-arm, the whole length of Princes Street, the doctor in the centre. Bentress had nothing to say, so that I had the whole conversation to keep up with the doctor; but he is a person with whom one feels perfectly at ease. He was anxious for a paper—proposing as an inducement fellowship admission to the Edinburgh Royal Society.

Talking of Dr. Brewster, I do not recollect whether I mentioned that he had sent me a present of some specimens of carbon, similar to what Colquhoun describes, and which Margaret will recollect. He also sent me a liquid to examine, obtained from a portable oil gaswork in London, which is rather curious. He mentioned that upon being poured upon hot water it took fire. For, one night, when drinking toddy with some others in Sir Walter Scott's, having accidentally had it in his pocket when the servant came in with more hot water, he poured some of the liquid upon the hot water, and it took fire. I suppose that they had all been rather groggy, for when I told him that I thought it impossible that it could have taken fire of itself, he confessed that perhaps some of them had set fire to it.

By the bye, the sand bath, although it cost me very little thought, is making some noise here. Mrs. Chalmers, of St. Andrews, and many ladies here, are getting them erected. Do you think I write popularly? Mr. Chalmers thinks as much of my new article on "A Marine Amulet" as he did of the sand bath.

Edinburgh, May 27th, 1827.

My Dear Mother,—I think that I will be home for certain within a week. Indeed, I would have come off with Mr. Mitchell had it not been that the whole of my pecuniary interest in this sublunary world amounts to three sixpences, so that it is necessary for me to remain a few days and write to raise the wind.

May 30th, 1827.

I look forward with great pleasure to my visit to Glasgow, as notwithstanding that I have now been a whole year in Edinburgh, I feel as much as ever all the solitude and desolateness of being in a land of strangers, which, from my retiring habits, will be always the case wherever I go.

Edinburgh, July 20th, 1827.

My Dear Mother,—I have seen Chalmers twice, although neither of the times did I enjoy a private *tête-a-tête*, as other people were present. He seemed, however, to say that the sale of his journal had latterly been considerably injured in England by the "Library of Useful Knowledge." It was detained last month in expectation of a plate from Dr. Fleming of Flisk, which, however, did not come forward. Wilson, the painter, and Browne, of the Hunterian, were up last night.

I have not yet returned to my labours, but am drawing up a plan for them. Everything is sufficiently encouraging, although I shall never know rest and peace till I get them completed. But perseverance, I doubt not, will overcome every difficulty.

Edinburgh, August 1, 1827.

My Dear Mother,—I have not seen Chalmers for near a fortnight, so that I do not know what his present plans are. I am busy at present with writing a treatise of a chemical nature on "Steam as a Moving Power," and performing some experiments which the subject gives rise to. It may be profitable in two ways—first, as a book, for it would make a small one, and there is at present a great demand for such a book, for all the previous writers of such books were engineers, and confined themselves to the machinery of the steam engine, neglecting the chemical properties of steam, upon which everything depends, and of which they are in general wofully ignorant. Secondly, my investigations into this subject have led to a slight improvement, which may be applied with the greatest ease to such engines, and increase the power of the steam by one-fourth, or more—but of this anon. I may add that I had been applying myself to this subject a considerable time before I was West with you.

Edinburgh, August 6th, 1827.

My Dear Margaret,—I had occasion to see Blackwood the other day with the business of *another* Miss M., although a less attractive and less lovely object. It was towards the end of last week. Chalmers had seen him that forenoon, informed him of my errand, and prepared him for my reception. Upon entering the shop, I requested one of the young men to inform Mr. Blackwood that Mr. Graham waits for him. Ebony immediately made his appearance—a business bustling like man; said that Mr. C. had informed him of my business, and requested me to walk on with him. He led the way through a saloon, with a large circular table in the centre, piled with new works, magazines, newspapers, and other ambrosial sweets, around which there sat authors, blessed in their labours, happy spirits! Beyond, at the little door of a deep recess—a most holy sanctum—Ebony drew up in position second, and beckoned my entrance. It was a little cell, and the scanty light from above played upon the busts of Scott and Byron, supported on a marble table. These, with two chairs, constituted all the furniture of the den. It soon appeared in our conference that Ebony had determined to fight Shy. "So many excellent novels have now been published, that nothing except what is very superior would sell, &c." He enquired what was the nature of the story and if the authoress had ever published before, and he concluded by saying that if I sent it down, he would look over it, although he might not be able to say anything about it for a week or ten days. I sent him the manuscript that evening, so that it is now in the hands of the Philistines. I suspect much, from what little more I read of it before sending it away, that it will not suit Blackwood. The first volume is historical, well written, and interesting enough for a *first* volume. But in the second volume long conversations are given, which have certainly the merit of being true to life, but to me at least feel exceedingly dull and vapid. Instead of leaving the heroine Juliet's loveliness as much as possible to the conception of the reader, so that every one may mould her to their own standard of beauty, and none be shocked, she is described most needlessly at sixteen, as "tall and overgrown." Certain young ladies are introduced in "yellow bonnets fitting the head." Now such might have been the fashion at the period referred too, but the reader of the present age cannot help associating something ridiculous with characters so described. A bad impression is produced which tells against such characters, till the reader has forgot the circumstance. It was quite unnecessary to mention it.

I wish that I had seen the second volume before I left Glasgow, as it would have prevented me from being so sanguine of its success. I depended also upon Miss M., who told me that the latter volumes were *much better* than the first.

Edinburgh, 16th September, 1827.

My Dear Mother,—I was very anxious to hear from Clover-Bank till I received Margaret's letter yesterday morning. You see I send in another statement—as it is certainly the best policy to do so as often as possible—sufficiently imaginative like the rest. We are well here and exceedingly comfortable. The weather has been rather broken here for a week or two, so that the laboratory, with its little furnace, is the most comfortable room in the house. I have *scanned* off my allowance two very fine thermometers of great accuracy and beauty made by Dunn, and some other things, so that I now go on pretty smoothly.

Robert Logan of Eastwood has been here for a few days, he promised to call up immediately and to convey this parcel, so that I am, as usual, pressed for time. By way of ballast I send you a couple of letters, and the scrawl of one of mine. Johnston had sent up a paper, that I should deliver for him to the R. Society, upon which he had been working away for two years, for he has never published anything. I at once detected a serious oversight which pervades the whole of it, and will expose him to a severe basting if he publish it. I accordingly wrote him as you have it in the scrawl. His letter is an answer to mine—a good deal nettled, you will see, and talking nothing to the purpose. I have also answered that, and shown him to be wrong by experiment. The other letter is a curiosity, in so far as it has been in the penny post, and trying all the Grahams in Edinburgh since June. It is from Dr. Thomson's nephew, Steel. The whole address originally was "Mr. Thomas Graham," till at last it had fallen into the hands of some person who knew me and completed the address. The postman delivered it last night with that account.

Edinburgh, 1 W. Richmond Street, 4th October, 1827.

My Dear Johnston,—Your paper I received last night, after it had been opened by a person of the same name who resides at 1 W. Nicholson Street. To-day (Thursday), with the intention of delivering it, I have called twice on Dr. Turner without finding him. Nevertheless, trusting to your friendly indulgence, I have at last concluded, after a severe struggle, to retain the paper and take no further step till I have communicated with you. I employed the greater part of last night in perusing the paper; the result was the discovery (which the clear manner in which your results are stated made the more easy) of a most important oversight—not, however, in what you candidly allow to be the debateable part of the essay. You make Gmelin's salt, or your chloro-ferro-cyanide of potassium, to consist of

½ atom chlorine.
3 ,, cyanogen.
1 ,, iron.
2 ,, potassium.

and of these elements (mark) *alone,* no water, no hydrogen, or oxygen present. Now this constitution is totally subverted in a passage which occurs in page 21 of your paper, and is as follows:—"Heated in a tube they (crystals of chloro-ferro-cyanide of potassium) give off no water. As the heat approaches to redness it is decomposed, giving off *hydrocyanic* acid and *ammonia.* At a bright red heat it blackens and melts, and is at last totally decomposed, leaving *potash and peroxide of iron.*"

Here we have oxygen and hydrogen in abundance in a body which professedly contains not an atom of either. An analagous result of the igneous decomposition of the old ferro-cyanide of potassium did not disturb Porrett or Berzelius [and perhaps that misled you]; but why? Because they allowed 3 atoms water in their salt, the *decomposition* of which acccunted for the disengaged oxygen and hydrogen which appeared. Now, unfortunately, your theory is expressly founded on the supposition that these 3 atoms of water are got rid of. The decomposition of your chloro-ferro-cyanide should have strongly resembled that of the cyanide of mercury; and a very important body it would have been, enabling us to procure potassium at once. I had been attempting in vain to form a true cyanide of potassium for that purpose. You must therefore either sacrifice your experiments on the composition of Gmelin's salt or your theory. I mean not here by your "theory" the particular theory of a chloro-ferro-cyanic acid, to which you give preference—but every theory according to which you can arrange the same elements, of which you give two if not three (pp. 12 and 13). To save your *experiments* you may deny the correctness of the common analysis of prussiate of potash thus yielding up your theory, as it is founded on that analysis. To save your *theory* on the other hand you must retract your experiment on the decomposition of the salt by heat. But the fundamental synthesis of your salt given at the beginning of your paper applies strictly not to the ruby crystals obtained on solution, but to the "yellow substance" formed although the ruby crystals *are not* a chloro-ferro-cyanide of potassium or compound of such elements, the yellow substance might be, although I do not believe it. Expose this latter substance properly dried to a red heat, and see.

Repetition of your experiments would probably show that the whole three atoms water in the prussiate of potash are not displaced by the chlorine. It is there you should look for the error. At present your theory is contradictory.

As your character as an accurate experimenter might be injured by the publication of your paper in its present shape, although perhaps in no department of chemistry are such mistakes more allowable, I hope you will forgive the extraordinary procedure I have followed. I am afraid I have presumed too much. But still your paper is valuable—you have ascertained the existence of chlorine in Gmelin's salt, and from an individual salt you have raised the class. These results might be published without the synthesis which alone are defective. I fear that from your extreme susceptibility you will feel this partial failure more keenly than you should. Your paper might be remodelled, so as to be unexceptionable, although with diminished pretensions. Anxiously awaiting your commands.

I remain,

My Dear Johnston,

Your affectionate friend,

THOS. GRAHAM.

I strongly suspect (although at present I have not room for my reasons) that the common analysis of Prus. Pot. on which you found is inaccurate, and that it really contains *more* water and *less* cyanogen. Should this be the case, you may get out of the dilemma with your experiments safe—although not with your theory. I hope it may. T.G.

Durham, 7th Oct., 1827.

My Dear Graham,—I have this morning received yours dated the 4th. I don't know how it has been so long on the way. The fellow that got the packet must have kept it a long time. However, &c.

What a desperately acute fellow you are, and with what a vengeance you write. Your objection is certainly valid, but even were it sustained it would have no such overturning effect as you suppose. You will observe that I allow 3 atoms water to the salt of soda, and yet it contains the same acid and is a cyanide (if you like)—the water being that of crystallization. It is in this light that Berzelius views the ferro-cyanide. Nor does *hyd.* enter into the composition of the ferro-cyanide of Porrett—neither, therefore, need it enter into that of the chloro-ferro-cyanide—so that the whole question consists in *whether the salt of potash contains water or not.* It is true that I have founded my reasoning upon the constitution of that salt, but this was merely because it was the first formed—the most easily formed—and that to which my attention had been chiefly directed. Any other salt would have done as well, only if it contained water it might have been said that the *hyd.* of it formed a part of the acid. But that the chlorine at least of none of the salts, even of those containing water—that of Barytes, for instance—is not combined with *hyd.* is manifest by their giving off uncombined chlorine when treated with concentrated sulph. acid. When you mention the *yellow powder* as probably anhydrous, and as the only thing to which my remarks apply, you forget the experiment stated in which the salt dissolved and carefully crystallized gave exactly an equal weight of anhydrous crystals. I hope you see, then, that, allowing your objection, your conclusions are unsupported, for even if the salt of potash did contain water I had only to say with Berzelius—Yes, but it is water of crystallization. And then you will observe that I have stated the quantity of water in all the salts as very difficult to determine, and in no one case, so far as I can recollect, have I spoken upon it with absolute certainty.

Thus far, for argument's sake, have I discussed the theory of the matter with you. But still the salt of potash is *anhydrous.* The passage which has given rise to your speculations has apparently been carelessly written, whether or not I read it over again. I am not certain, but I am sorry that I have not a copy of it. I take it for granted, however, that the extract you have given me is a literal copy, and I have to beg of you to make one or two alterations to prevent any such mistakes in future. 1st. For peroxide of iron, read *carburet* of iron. 2nd. The second clause refers to the application of heat in contact with, and correct the whole as follows (I copy from your letter, and make as few alterations as possible to save blotting):—Heated in a glass tube they give off no water. *In a platinum crucible*, as the heat approaches to redness, *the salt is partially* decomposed, giving off hydrocyanic acid and ammonia. At a bright red heat it blackens and melts, and is at last totally decomposed, leaving Potash and *carburet* of iron. Here the underlined parts are the corrections. The substitution of *peroxide* for carburet has, I suppose, arisen from thinking at the moment of the *green sediment*, which, when heated in an open crucible, gives peroxide of iron. Now, you will see at once the source of the hyd. Potassium, when heated in contact with air, burns and becomes potash; hydrogen therefore will be evolved in proportion to the quantity of the oxygen it obtains which is due to the decomposition of moisture. You say its decomposition if anhydrous should be analogous to that of cyanide of mercury; and so it is—heat cyanide of mercury in the open air—it blackens, melts, and gives off *white* fumes of hydrocyanate of ammonia. Whence comes the *hyd.*—is it from water in the cyanide? As to the absence of water in this salt, Gmelin and I agree; it is not, however, beyond dispute, but the contrary is unsupported evidence.

I copy the following experiment from my note-book. I wish it could be inserted in a note; it would show how difficult it is to decompose the salt entirely, and that the iron is in the metallic state:—"5 grns. heated to redness in a platinum crucible over a spirit lamp first decrepitated, giving off hydrocyanic acid; ammonia was then evolved in considerable quantity, and this continuing, the crucible was covered and exposed to a strong red heat. Removed from the fire, a *greyish black* powder remains, which appeared to have been in fusion. With dilute muriatic acid it effervesces strongly, giving off the smell of hydrocyanic acid. The solution obtained gave a precipitate of iron with benzoate of ammonia, the previous filtration having left a slight blue sediment on the filter. There remained undissolved a shining black matter at the bottom of the crucible, which, by repeated digestion with nitric and muriatic acid, was partially decomposed, and in solution gave the precipitate characteristic of iron. It was, therefore, carburet of iron, and the metal taken up by the dilute muriatic acid was probably in the state of carbonate. The blue sediment shows a portion to have been still undecomposed."

This experiment is dated 18th December, 1826, and was made in Thomson's laboratory. If you think it is of any service, I will be obliged by your writing it on a bit of paper and introducing it as a note to the word *Carburet.*

If I have not succeeded in satisfying you, I have so in assuring myself that you have been hasty in supposing this *lapsus* to overturn all my reasoning. As to repeating the experiments you mention, I am satisfied myself upon the point. I have made hosts of experiments—many of which are not stated in the paper. Any more made are from blunders of the kind now corrected. I have stated nothing which I have not verified or considered as already settled. I conceive that upon reconsideration you will find reason to be satisfied with the presently received composition of the prussiate of potash. It is very difficult to determine the water certainly, but I cannot see how you can increase it at the expense of the cyanogen without also altering the amount of bases. I have tried to estimate it, but certainly could not depend on my own results if different from those of Thomson, Berzelius, and Philips. I rely on Dr. Thomson the more as he has stated that he had been obliged to come to that conclusion though anxious to obtain a different result.

In my last letter I did injustice to Mr. Faraday in relation to the disinfecting liquor. I have since had occasion to alter my opinion in regard to his correctness, and I, therefore, hasten to make him amends by saying I believe him to be right.

I had the pleasure of seeing a handsome notice of your paper on *solution* in the *Quarterly Journal* for this month. Unfortunately I have no opportunity of seeing the *Annals*, and, therefore, I got there the first notice of the publication of your paper. What is given in the *Quarterly* (Brande's) I agree with perfectly. There is only about a page: I should like to see the whole paper.

I hope that after making the corrections stated you will give in the paper as early as possible. I fear it will now be too late to have any chance of insertion in the *Transactions.*

Be kind enough to give me as early intelligence as possible, and believe me, my dear Graham,

Yours very sincerely,
(Signed) JAMES F. W. JOHNSTON.

Edinburgh, 12th Nov., 1827.

My Dear Mother,—I have been exceedingly negligent in my Cloverbank correspondence, notwithstanding the highly exemplary conduct of my correspondents. I can scarcely hope to be excused, but the truth is that I was never so busily engaged as during the last five or six weeks (anxiously engaged, too). Dr. Brewster, who manages the Royal Society here, is out of town, and intends to be so during the winter. In the course of a few weeks the Society intends to publish a volume of transactions in which I am very anxious to have two papers. One of these papers is already in the hands of Dr. Turner, and is highly approved of by him and John Robinson, being, according to the doctor (modesty avaunt!), "a beautiful and complete thing." The other paper is just finishing, and is a horribly long one, containing an account of 40 or 50 new compounds, and such is the burdensome richness of the mine I have opened, that I could easily add as many more. Dr. Turner, whom I have told of the nature of the discoveries, says that it will form a capital paper also. There is, therefore, no difficulty whatever in getting the papers read, and eventually printed by the Royal Society, but the point is to get them read at the very first meeting of the Society, otherwise they will be too late to appear in the forthcoming volume of their transactions, and perhaps would not be printed for a year. Now, so little confidence have Turner and Robinson in themselves that, protesting they will do everything in their power to meet my wishes, they are not sure whether they will be able to have both the papers read the first night, in fact they are quite confused without Dr. Brewster to direct them, and there is really no difficulty in the case. If they do not pick up courage, I will write to Dr. Brewster, who will dissipate all their imaginary difficulties in a moment. If they cannot be got read and printed immediately by the Society here, I will take the advice Leslie gave me the other day, and send them to the Royal Society of London.

Notwithstanding my hurry, I can assure you I have been in excellent spirits, and as for health, strong as a thousand elephants. Miss C. is also well, and everything comfortable. Never were things in a more favourable state should I choose to set up in a chemical line here, than they will be this winter or spring. Of the two lecturers, Dr. Fyfe is setting out to travel on the continent for his health, and Dr. Turner expects every day to be appointed to the Chemical Chair in the University of London. Should he go, I can depend upon getting his fine new laboratory moderately, with his practical classes, without any competition, in the absence of Fyfe. Dr. Turner is exceedingly confident that he will get the London chair. He told me that the candidates beside himself are Dr. Fyfe, Dr. Ure, of Glasgow, and Mr. Phillips, the editor of the *Annals*—none of them great hands, although Phillips is certainly the most deserving. Turner thinks, however, that Phillips will have no chance, because he has never lectured, as if there was any mystery in lecturing. I think, however, that Turner has a very fair chance, particularly from his acquaintance with Leonard Horner, now the Warden or Principal of the University, who is an Edinburgh man. It is curious how little distinguished any of the candidates are; and, indeed, the circumstance that the election has been put off so long is not complimentary to them. Indeed, perhaps in no other department of literature or science is there so little competition in this country at the present day as in chemistry. The chief reason of this is that the most of chemists are medical men; their reputation soon procures them great practice, which is too tempting to be rejected, and chemistry becomes neglected amidst their extensive and highly-profitable professional engagements. This has been the case with almost all the great London chemists who should have been competitors for the chair, such as Dr. Prout, Dr. Wollaston, Dr. Bostock, &c. In fact, they were no longer chemists. Turner expects to hear of the issue of the election every day, as it was to be decided in the beginning of November. I confess I would feel rather reluctant at setting up as chemist in Edinburgh, preferring London, but all will depend upon circumstances. Meanwhile everything is encouraging. Should Dr. Turner succeed in getting to London, I will have a friend at court, and there is another chemical professorship—"On the Application of Chemistry to the Arts"—which probably will not be filled up for a year or two. In all these competitions everything depends upon sheer impudence, of which I confess I have too little, confining it to my letters entirely. When Turner first told me of his being a candidate, and of his confident expectation, it was only his presence that prevented me from laughing, for he rests his whole merit upon his having lectured for a few years, and having compiled "Elements of Chemistry," while his original discoveries amount to nothing, or are exceedingly trivial. Bentress would never give Turner any credit, and even told Dr. Brewster that he thought he rated Turner too highly. But Dr. Turner is very courteous, and really an amiable character, so that people are charitable in their opinion of his talents; and, for my part, I will be happy in his success. You recollect of Johnston's faulty paper, as he insisted I was obliged to give it to Turner, but at the same time told him that I did not warrant its accuracy. I was somewhat amused at Turner and his paper afterwards. He had examined it, and saw there must be something wrong in it; but being like Johnston, not overly clear-headed, he could not see precisely where the error lay. The next time I met him I soon found he was fishing me to find where I placed the error. Of course this must have been disagreeable to him, so I spontaneously explained the whole matter. It was then "as clear as day, an error that overturned the whole theory," &c., and I was requested to intimate to Johnston that, while his paper contained so palpable and manifest an inconsistency, it would not be recommended to the Society. This I did as mildly as possible, for I am really sorry for poor Johnston. Johnston, however, has given me provocation which, from any other person, would have been unpardonable—in a second letter some time ago—but he has now virtually surrendered, and must feel the excess of regret for his nonsense. I have scarcely room to subscribe myself

Your affectionate Son,

THOMAS GRAHAM.

Edinburgh, 19th Nov., 1827.

My Dear Mother,—Dr. Turner has been appointed to the London University, and I am to succeed him here, so far as succession is practicable. I am to take up his experimental or laboratory classes in three months hence, or in February, and to commence a summer course of lectures of three months in May. During that time I am to have the free use of his laboratory, apparatus, and everything, which is an unspeakable advantage. But next winter I shall be obliged to get a class-room, &c., of my own, as Dr. T.'s devolves by an old compact on Dr. Cullen, the lecturer on the Practice of Physic. But other favourable circumstances may arise before that time, and at any rate the present advantage is not to be rejected, while the accident of appearing as successor to the Professor of Chemistry in the University of London is itself of some value.

These experimental classes to which I refer consist of half a dozen students and upwards who come to the laboratory for an hour, and get half lecture, half experiment, assisting in performing the experiments. The fee is three guineas a quarter; and one may have more than a single class of this kind in a day. They are what you would call private classes.

Dr. T. is going up to see how things are doing in London at Christmas, and it is probable if his stay is protracted that I may lecture for him at that time, for a few days, both in his own public lectures and in the School of Arts, where he is at present lecturing in the place of Dr. Fyfe.

He has promised, while at London, to recommend me as a fit person for writing certain Treatises on the Chemical Arts "in the Library of Useful Knowledge" to Leonard Horner and Mr. Brougham.

He informs me that it is agreed that Dr. Hugh Colquhoun and I are to be proposed as members of the Royal Society at an early meeting. . . . . .

[During 1827 Graham was occupied with three papers—(1) "On the Finite Extent of the Atmosphere;" (2) "Longchamp's Theory of Nitrification;" (3) "On Exceptions to the Law that Salts are more Soluble in Hot than in Cold Water."]

Edinburgh, 11th Jan., 1828.

My Dear Mother,—Dr. Turner resumed the class yesterday. He was up here, but for so short a time that I got scarcely any news from him. Sir H. Davy had a paralytic stroke, but is getting better. Mr. Phillips, the editor of the *Annals*, has left London, having got the superintendence of some great work about Birmingham. Dr. T. was highly pleased with his reception among the dons. He breakfasted last Sunday with Mr. Brougham. The arrangements about the class and laboratory are quite agreeable to him. The class-room holds 500 seated. He must go up sooner than he expected—in July—and open his class in Oct. In the lecturing I was more at my ease this week. One night I was upon phosphorus, had the room darkened, and was exceedingly flashy. I was much amused by the fellows becoming rather uproarious in the dark, just as we used to be in the Pig's. James says that there was always a number of strangers present. I was slightly annoyed with two things—a touch of sore throat, which I got from drinking a glass of water after finishing, and by inhaling a little of chlorine gas when making it, owing to James's fault in remaining away too long one day—I had permitted him to be operator to Mr. Ritchie, of Tain, who delivered a lecture on Oil and Coal-gas in the Assembly Rooms.

Edinburgh, Feb. 21, 1828.

MY DEAR MOTHER,—My silence has been so long as to be quite inexcusable, but I have been always deferring and deferring until I had something definite to tell you. Shortly after I wrote last I had two offers, both of which deserved attention. The first was through a Dr. Gardiner, from Constable Junior, to write a volume of his Miscellany (which you have perhaps seen) upon Heat and Electricity. This I was very much inclined to do, as I would be obliged to write lectures on the first of these subjects at anyrate. But I have long had it in view to translate and get published in a compressed form, like Ure's Dictionary, Thenard's French System of Chemistry, which in the original is in five volumes. I had employed Mr. Chalmers to sound some of the booksellers, and Oliver & Boyd embraced the proposal at once, and, after I was introduced to them, and had several meetings, offered to give me half the profits of the publication, without any of the risk, if I would undertake it. This would be considered a good offer as such things go, but at the advice of both Turner and Chalmers, particularly of Turner, I preferred another plan, for by the former one is quite in the power of the booksellers, who can estimate the profits at what they choose. This is just to require a certain percentage on the retail price of the book, say 10 or 12 per cent. upon the price of every copy printed, which would probably be sold about 25s. Oliver & Boyd have not expressed an opinion as yet upon this plan, and the whole arrangement is postponed till a copy of the last edition of the original (lately out) can be procured from London, and I believe it will not arrive before the commencement of next month.

It would take about a year to translate Thenard and write notes to it, and the profits of the first edition might be about £120; but then there would be as much on every subsequent edition. It would also be a great thing to have one's name attached to a book of such weight and importance. Turner thinks that if he were in my situation he would adopt the plan at once. Dr. Thomson first brought himself into notice by translating Fourcray's System of Chemistry. In the meantime, however, I have been doing some thing to Constable's business, as it would require more than two or three months—the return about £30—and as the materials will do for lectures. On all these accounts I have deferred the experimental course, of which the success would be rather problematical, but am still ready for the lectures, should things yet turn that way. But I am somewhat unwilling to set up as a regular lecturer in Edinburgh! It is so medical. . . . .

Edinburgh, 8th April, 1828.

My Dear Mother,— . . . . . . I was admitted into the Royal Society last night. I don't care although the L. should know, but should he think that there is anything to pay it will take away all the merit of it.

Friday, April 25, 1828.

My Dear Mother,— . . . . The business with Boyd, of Oliver & Boyd, is not yet settled, although I have delayed from day to day to write you, expecting that it would be settled. He seems determined to make a good bargain, and to-day I received a note from the firm, declining my proposal of £130 sure for the first edition, and half the profits on every subsequent edition, which might be estimated at £100 on each subsequent edition. It is not my charge for the first edition which he quarrels with, but he seems to think that if I am made sure of so much on the first edition that I have no title to so much as half the profits upon subsequent editions. I am quite aware that my proposal as to subsequent editions is high, but Boyd is so wary that it is difficult to guess what proposal as to that matter he would be disposed to fall in with. He won't suggest anything. . . . . . At the Royal Society last Monday I had a paper, and made my *debut* as a member. Johnston, of Durham, was rejected in the Council as a member, where he was proposed by Dr. Turner, at his own urgent request. I had a great deal of talk with Dr. Hope as to Johnston's merits, and kept him up as well as I could, but to no purpose. It was Dr. Hope who occasioned his rejection in the Council, he being, as Miss Cameron says, "no poor man's friend." . . . . Hope told me that he had repeated my alcohol process, "which he fancied," before his class with success—it takes a week, so that it is no bad advertisement—and added, with a pomposity which was almost too much for my gravity, "and may I be allowed to ask, Sir, was this beautiful discovery the result of accident or of research?" Humbug.

[Graham was occupied with three papers in 1828, viz.:—(1) "Experiments on the Absorption of Vapours by Liquids;" (2) "On the Influence of Air in determining the Crystallization of Saline Solution;" (3) "A Short Account of Experimental Researches on the Diffusion of Gases."]

The poverty of Graham at this period is illustrated by the following curious anecdote communicated by his sister, Mrs. Reid:—"I dare say you know of his taking a very poor little "room at Portobello, and laying wires in the sea, mostly after it "was dark, but moonlight perhaps, for his experiments. His "landlady found some of his things below the bed in his room, and "told him she did not like to see such things, she doubted he was "after no good, and he had better take his leave. She put his "wires, etc., out at the door and bid him walk, she would rather "want a week's rent than let him in again; so poor Tom had to "hide his things and walk back to Edinburgh that night. The "rent of the room was 3s. per week, without fire, which was a "great deal more than he could spare at that time." Graham then returned to Glasgow, where he taught Mathematics for some time at the suggestion and under the patronage of the late Dr. Meikleham, Professor of Natural Philosophy in the University of Glasgow. Subsequently he had a Chemical Laboratory in Portland Street, where he also delivered lectures, and for the year 1829-30 did similar work in the Mechanics' Institution. In 1830 he was appointed Professor of Chemistry in the Andersonian University.

[During the next six years Graham worked hard in Glasgow—lecturing, teaching, and sending out workers from the Andersonian University.]

Glasgow, August 2, 1831.

My Dear Margaret,—I had occasion yesterday to address, for the first time, that most learned body—the Faculty, for the purpose to get attendance on a course of Practical Chemistry made imperative, which should long ago have been the case. Mr. Harry Rainy seconded my motion, and our proposal was graciously received, although, as all dignified bodies move slowly, the bill must be read a second and a third time at successive meetings before it is passed into a law.

I do not know what to say for news. Some mad doctor at Port-Glasgow, it appears, lately alarmed the Government on the fancied appearance of the Russian cholera in that quarter, and occasioned a meeting of the Privy Council and other unnecessary measures. The Faculty replied yesterday to a letter of inquiry from Mr. Dixon, that the town was never more healthy at this season of the year than at the present moment.

Mülhausen, Alsace (Hotel de la Couronne),
1st September, 1836.

My Dear Margaret,—In his laboratory (Daniel Köchlin) we found the excellent chemist Schlomberger, he is an *elève* of Mr. Köchlin's, and who has written the best papers on Madder. The chemist was throng at work in his blouse-coat and wooden clogs. Köchlin is himself a most interesting person. By the way, C. Thomson is studying with him, and he has a high opinion of Walter Crum, and pronounces him, now that James Thomson is getting old, the most accomplished printer we have in England. The afternoon we devoted to Mr. Hoffer's own establishment; John finds them exceedingly communicative, and they seem to show us everything without reserve. He thinks that he has already attained the most important objects of his mission, and that he will be able, on returning, to produce the beautiful Madder rose-reds for which Alsace is famed, so that the journey will not be lost . . . . . .

I was amused by the feeling which seems to prevail here in regard to the French Institute. They have never published Schlomberger's valuable papers on Madder, because they impugn the results of a brother member, M. Robiquet; although Mr. Köchlin spoke on the subject to Gay-Lussac, Chevreul, and others.

The fact, I believe, is that this exclusive dealing results from no bad feeling, but the Institute is a coterie, all the members are familiar with each other, and unwilling to give each other cause of offence. Gay-Lussac likewise, who is chemical editor of the *Annales de Chimie,* is an exceedingly mild man, and is certainly carried too far by the fear of offending his brother academicians. There is one of my papers which he has promised to publish (on Phosphuretted hydrogen), but which I would not be surprised to find him restrained from doing when he finds that I give more credit to Henry Rose than will be agreeable to M. Dumas. Dumas surprised me much the first day I met him by a hasty and unfair criticism on Rose, whom I defended. But afterwards, on reflection, he was at pains to undo the impression which his remarks might have produced, and finally transmits through me sentiments of respect to Rose, so that I hope to be the means of reconciling them.

Hotel de Rome, Dresden, September 27, 1836.

My Dear John,—I have this morning arrived here, having travelled from Jena by way of Leipsic in company with Dr. Daubeny, of Oxford. After you left me at Frankfort I went down to Giessen and spent two days with Liebig. He is a most delightful fellow, and I was glad to find him in very fair health. Returned to Frankfort, and took the diligence to Weimar, near Jena, which I reached this day week. Between 300 and 400 "*Naturforscher*" had arrived, and the business of the meeting had fairly commenced. The arrangements were satisfactory, although there was not the same ostentation and amount of treating as at the British meetings. In the mornings sectional meetings—the Chemical and Physical Sections meeting together; a general meeting from 11 to 1, at which memoirs, or rather essays, of general interest were read. Dinner from 2 till 4, and sectional meetings again at ½ past 4 to 6 for those who had not taken too much champagne; and the evenings concluded very rationally with a ball at which there was a good turn-out of Jena beauties. As for the communications, they were interesting enough, but nothing strikingly important. Of the savans who had assembled Humboldt was the great star, but to me Mitscherlich was the most interesting.

I had a good deal of Mitscherlich's society, and found him very communicative. He has a most insinuating address, but there is a cold propriety in everything he says and does. I was sorry also to notice (and I made the same observation of M. Dumas) that when I stated any little discovery of my own he was fretted and could not conceal a little disappointment *because it had escaped him.* This was not like the hearty good feeling of Liebig or Henry Rose. But I have no reason to complain of Mitscherlich. My opinion of him as a chemical philosopher, high as it was before, has been elevated by my intercourse with him. I was particularly struck by the beautiful simplicity of some hints which he diffidently threw out as to the effect of a base, such as water or potash, on the corpuscular arrangement of particles which cannot subsist without a base. The base, which may be water, has here manifestly an effect upon the atomic constitution of the acid, if we could figure it to ourselves. Water may have a similar effect on the metaphosphate of water, which contains one atom water. The influence of the water on the arrangement of the particles composing phosphoric acid may be *different* from what it was in the other case. This idea of Mitscherlich is, of course, altogether hypothetic, and he by no means pressed it, but it is certainly a *very little* assumption, and we can observe in it the great secret of correct philosophising, namely, to advance by the shortest possible steps. In these corpuscular speculations it was interesting to observe how naturally he fell into the simple but philosophic views of Dalton in regard to atomic arrangements.

You know Clark's letter to Mitscherlich on a difficulty in isomorphism, which is unquestionably a most profound speculation. Mitscherlich has never noticed it, which in courtesy he was bound to do, even although he has had a second letter from Dr. Clark. I more than once started the subject, but he talked of having been so busy overhauling the arrangements of his laboratory, of being "tormented from morning till night," I suppose to palliate his neglect; and the speculation was evidently not to his mind—too striding for him. He alluded to other cases of similarity or identity of form besides that on which Clark founds, such as that of sulphur and bisulphate of potash, where the constitution of the two bodies is totally different. Mitscherlich's law is that two bodies which have the same constitution should have the same crystalline form, but it does not necessarily follow that bodies which possess the same crystalline form should likewise have the same constitution. Mitscherlich disposed of the question by observing that because two and two make four that is no reason why three and one may not make four also. . . . .

Berlin (Hotel de Brandenbourg), October 10, 1836.

My Dear Margaret— . . . . This morning Magnus and I visited Prof. Enke by appointment at the Royal Observatory, where we spent several hours. The Professor explained all the admirable arrangements of a working observatory to us in detail. We saw the spots on the sun, and also the two stars which compose the double star Alpha Herculis, although it is only of the third or fourth magnitude, which, considering that it was bright daylight, was a very severe trial of his splendid Frauenhofer. I have not seen anything for a long time that pleased me so much as this far-famed observatory. . . . .

[About this time Graham sought the appointment of Professor at the University College, London, having during the preceding seven years been at work on the following papers, viz.:—(1) "Observations on the Oxidation of Phosphorus;" (2) "Notice of the singular Inflation of a Bladder;" (3) "On the Application of Spongy Platinum to Eudiometry;" (4) "Effect of Animal Charcoal on Solutions;" (5) "An Account of the Formation of Alcohol;" (6) "Researches on Arseniates, Phosphates, &c.;" (7) "On Hydrated Salts;" (8) "On Phosphuretted Hydrogen;" (9) "On Water as a Constituent of Salts."]

Sablonieris Hotel, Leicester Square,
London, Saturday, April 29, 1837.

My Dear James,—By the letter which I wrote (to Margt.) on Tuesday you would learn that I had started fairly in my canvass. I have seen nothing since contradictory to my first impressions. In whatever way it goes, the appointment will be made by the medical professors, who will take care to be unanimous, or nearly so. Of the nine or ten virtual electors I have good reason for believing that four, at least, are not only favourable but *anxious* for my appointment, while the others, so far as I can discover, are not as yet committed to any other candidate. It is also a little fortunate for me that my supporters belong to both of the two parties into which the professors occasionally separate, although there is no serious division among them. But, as the whole force of the recommendation to be made to the Council by the professors depends upon their unanimity, I am not to expect that my four friends are to stand out in my favour if there should be a majority in favour of another candidate. It would be perilling their influence with the Council, without securing my return.

Johnston of Durham turns out, after all, to be my most formidable opponent. Both Mr. Richard Phillips and Dr. D. B. Reid would have some support in the Council—particularly the former, from his true respectability of character—but they are both given up for different, although sufficient reasons, by the professors.

Dr. Apjohn, of Dublin, from whom I feared most, has been rather inactive, and I think his chance of success inferior to Johnston's or my own, although he follows us.

I remain, Dear James,

Yours most faithfully,

(Signed) THO. GRAHAM.

Leicester Square, London, May 6, 1837.

Dear William,—At this moment I am in considerable doubt as to my movements. I had intended to start by the steamer for Leith to-night, and may very likely do so still, but the applications, &c., of the candidates having been lodged yesterday, and a meeting of the Council taking place to-day at 4, after the distribution of the prizes in University College, I am advised by Dr. Carsewell not to decide till I hear whether the Council takes up the applications at that meeting and refers them to the Senate, in which case a meeting of the professors would be called for Tuesday or Wednesday, and a committee then appointed to report upon the applications. Dr. C. thinks it might be worth while postponing my departure for a week to give that committee an opportunity of communicating with me personally if they desire it. The Council will sit till six, and I can scarcely hear the result before seven, which will be too late for the post at this distance.

I shall, however, retain this letter till the last moment. In case I remain here, would you make a point of seeing Mr. Young and request him to begin the Practical Course, and get on with it till the end of the week, assuring the students of my return at the beginning of the week following. . . . .

I remain, MY DEAR WILLIAM,

Yours affectionately,

(Signed) THO. GRAHAM.

William Graham, Esq.

[Graham was elected Professor of Chemistry at University College, London, in 1837—an appointment he held for eighteen years. He lived in the neighbourhood of this College to the end of his life, 16th September, 1869—first in Torrington Square and afterwards in Gordon Square.]

London, Saturday, Oct. 21st, 1837.

MY DEAR MARGARET,—I have to-day completed the third week of my course, and am as well and comfortable as at the beginning of it. My number to-day is 221, which I have no doubt is the largest class on chemistry in the United Kingdom. I have got 70 pages of my first number in type, it requires 93 or 96. It is likely to be ready in time so far as I myself am concerned, but there is a risk of the woodcuts, about 30 in number, not being all ready, as this is the busy season for the annuals, and so forth. It is not a matter of much consequence, however, if it appears in the first week of November, as to the exact day.

We are all doing well at University College this season. I am very much pleased with the attention and respectful behaviour of my students, more than 120 of them have put down their names for examination.

London, Oct. 26th, 1837.

MY DEAR MARGARET,—At Sharpey's advice I have thrown away the papers, and for a few days past have been extemporizing. I am satisfied that it is the true system, and will come to be much the easiest in the end. After I get my first No. out (which by the way is all in types), I shall not allow it to interfere with my other engagements, but only take it up when perfectly convenient. The extemporizing requires one to be both in good health and in good spirits, and I am determined not to risk myself for any consideration; so keep yourself easy.

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36 Keppel St., Russell Sq.,
Nov. 3, 1837.

My Dear Margaret,—Already five weeks over. My No. in the class is 232, and a third student has joined the laboratory, a Mr. Fenton, of Rochdale.

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36 Keppel St., Russell Square,
Nov. 24, 1837.

My Dear Margaret,—I have after all left myself very little time to write to you. I had a very long and agreeable letter from Liebig a few days ago, conveying the information that his University have made me a Dr. of Philosophy *causa honoris*, but this is a profound secret, and to be kept like *all* such, as I am doubtful whether I shall make any immediate use of it, although it is very flattering. He has been reconciled to Dumas at Paris, and has a grand project of publishing a chemical journal in three languages, to be edited by Liebig, Dumas, and myself, but not before summer.

London, Dec. 2, 1837.

My Dear Margaret,— . . . My present number is 238, and we cannot look for any addition till January, when we may be joined by few new perpetuals, for we do not give the second term by itself. But you must not think that all my 238 are payees. This arises from the system of perpetual tickets.

Somerset House, 24th March, 1838.

Sir,—I am directed by his Royal Highness, the ——, President of the Royal Society, to communicate His Royal Highness's thanks to you for having exhibited your very interesting experiment of the solidification of carbonic acid gas at Kensington Palace, and also to express to you the very great gratification which that exhibition afforded to His Royal Highness's visitors. I am further directed to suggest to you whether you might not present to the Royal Society an account of the experiment, which would be very acceptable to this body.

In thus conveying to you the sentiments of His Royal Highness, allow me to express to you my individual gratification in witnessing your truly scientific experiment on that occasion. I felt, I assure you, at a loss which most to admire—the highly important result, the philosophical beauty of the principles on which it was obtained, or its immediate scientific application.

I have the honour to be,

SIR,

Your most obedient Servant,

(Signed) S. HUNTER CHRISTIE.

Professor Graham, F.R.S.,
&c., &c., &c.

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9 Torrington Sq., Monday, March 11, 1839.

MY DEAR MARGARET,— . . . . I attended last night the first of the Marquis of Northampton's *soirées*. He has one of the large houses in Piccadilly, facing the Green-park, beyond Park Lane. There was a great turn-out. The heliographic drawings of Niepce, Daguerre's old partner (referred to in the *Athenæum* of this week), were on the table. They are on metal—extremely delicate and beautiful when seen in a proper light, suggesting a mezzotinto drawing. My idea is that the metallic plate, wetted with nitrate of silver, has been placed in a small camera obscura *containing hydrogen gas*, and then the prospect reflected in upon it. Where the light fell the white silver was deposited in the metallic state on the plate, and makes the lights on a darker metallic ground, while the shade can be etched afterwards by an acid from which the lights will be protected by the silver. But we shall see. Mr. Talbot's plan for prepared paper, in which the lights and shades are reversed, is curious but not at all new.

9 Torrington Sq., Monday, April 29, 1839.

My Dear Margaret,—My compliments to Mr. Reid, and tell him that he may have the cabinet for £7 10s., neither less nor more (the sum for which Howard procured me the bookcase I have here), but not before his house is built and he finds that he has room for it. If not a very handsome, I believe it is a substantial piece of carpentery, as Mr. Young [Dr. James Young] used to pride himself on it, particularly on the hinges of the lower compartments, which are of his own invention.

[During the years 1837-9 Graham was busy with three elaborate and important papers, viz.:—(1) "On the Constitution of Salts;" (2) "On the Law of the Diffusion of Gases;" (3) "On the Theory of the Voltaic Circle."]

From York, Sept. 29, 1844.

To Mrs. J. Reid,—I write you in the middle of our meeting here, which has proved an excellent one. We have the best men in all departments and a pretty numerous attendance besides. My own Section is particularly favoured as we have the two stars of the meeting, Professor Matteucci from Italy, and Liebig. The latter has attracted crowds of country gentlemen to the Section, which is quite a new feature in it. Liebig is residing with the Archbishop, whose son, the Rev. W. Vernon Harcourt, is highly chemical. I drove out with Mr. Harcourt to dinner on Thursday at his father's, and arriving early had half-an-hour of the Bishop's company while the rest were dressing. He is a hale old man of eighty four, of strong sense and highly popular. He was using a stick from a lameness occasioned by his breaking one of the branches of the tendon achilles of the heel at the late Royal christening, of which accident he gave me a familiar account. He suffered so much that he could not have read the service, but having been a parish Priest before he was a Bishop he knew it by heart; and the Queen and Albert were afraid all the time that he would let the child fall out of his arms. His daughter Miss H. does the honours; and the young ladies of the family (her nieces) are fine girls, young Miss H. in particular, who has had a week of desperate flirtation with Rev. Deans and Professors who flock about the palace.

I return to London on Wednesday evening and will therefore miss the Liebig dinner at Glasgow.

Oct. 29, 1844.

To John Graham, Esq.—I have had a bustling week owing to Liebig's return to London, and second visit. He saw no company except private friends, and did not visit out last week (except once) till we left for Oxford together on Friday after my lecture. This was a long promised visit for me to Dr. Daubeny, and I remained there till Monday, occupying Felton's chambers in the Dr.'s college (Magdalen) for three nights. This being term time, I saw Oxford, as you may suppose, to great advantage. The Doctor had his sister and two nieces on a visit to him in his official residence in the Botanic Garden, one of the said nieces is not quite a beauty, but everything else, and very dangerous.

Liebig returned before me on Saturday evening, accompanied by Dr. Buckland, who consented to remain with him at No. 9, and did the honours of the house till the return of the usual occupant. I wish Margaret had been here to have met the Doctor, his good nature and humour or rather classical drollery are quite overpowering.

Liebig had a very satisfactory interview with Sir Robert [Peel] after church on Sunday—Buckland introducing. Among other things, the Professor was asked his opinion of the new (proposed) College of Chemistry. He said that its character and success would depend entirely upon the chemist who should undertake its management, and that not being settled he could not give any opinion, although he thought favourably of the principle. It is, Liebig really believes, at present a job of certain parties, but I would not be surprised to see it taken out of their hands. Sir Robert enquired why not attach it to one of the existing institutions—University College or King's College. This Liebig thought the proper plan.

Dr. Buckland has much of the premier's confidence, and is, I can easily see, his principal scientific adviser. I have no wish to be encumbered with the new college, and believe that the means of teaching chemical analysis in London are sufficiently ample and cheap, more than the public will take advantage of. The best *locale* for a Giessen school is not London, but some country town, and it might be well connected with an agricultural college. I have no wish to meddle with such a speculation or be any way responsible for it; foreseeing, as I do, great perils and chances of failure. Dr. Buckland, however, having extracted my views of the subject, adopts them, and I have no doubt they will go further. Sir R., I should mention, had expressed his willingness to go as far as to accommodate such a college of chemistry with house-room, provided the necessary expenses were otherwise guaranteed.

Dr. Daubeny was taken by surprise by the announcement in the *Standard* of Saturday that Dr. Dalton had left £4000 to found a new professorship of chemistry in Oxford (!!!) One of the executors (Mr. Nield, I believe) also informed Dr. Daubeny at York that he had a legacy of £100, but of neither of these circumstances has he received any official notice or confirmation. I wish you would let me have what you know on this matter. The will, having been sworn in Doctors' Commons, is, I suppose, published. Also, how your own proposed professorship in Manchester is likely to be received. Say how much I should subscribe. Dr. Daubeny proposes to give ten guineas, but perhaps you would expect twenty from one who fraternizes with the League, and has so much to interest him in Manchester.

[Between the years 1839 and 1844 Graham was busy with three papers, viz. :—(1) "On Preparation of Chlorate of Potash;" (2) "On the Constitution of the Sulphates;" (3) "On the Heat Disengaged in Combination," in two parts.]

Aug. 31st, 1850.

To Miss Graham,—I started from London on Tuesday night, and in twenty hours reached Cologne very much cut by sea-sickness and fatigue. But after a night at the Hotel Hollande I was quite recovered, and enjoyed the sail up the Rhine in a very fair day, the voyage to Biberich near Mayence lasting from 6 to 7 evening and reached Frankfort by railway about ten: when I got to a good hotel, "the Roman Emperor," the town being crowded from the fair. The peace people had left. Cobden had been most admired, and the people here had been gratified by the discursive style of English addresses which they take as a lesson in public speaking. Next morning I went on by railway and diligence to Giessen, from 9 to 2 p.m.; and after dining at the Rappen called on the Professor. He appeared to me in better health than the reports, indeed I would imagine quite as well as when I first saw him thirteen years ago. He had many inquiries for you, and has made up his mind that you are to come and spend four or five months with them. Madame Liebig and part of the family are at Ostend, he had waited a day or two on my account, and then Dr. Will and he proceed to Aix-la-Chapelle to analyse the waters—a professional engagement. I found the leading young chemists of the place still here, Dr. Strecker whom I think very highly of, Flutmann, and Will. Kopp had already left, but Knapp and Madame Knapp were at home; also an Austrian young professor Dr. —we had a very merry supper party. Dr. Hoffman I also found at his mother's, and was introduced to the old lady and his two sisters, a very pleasant family circle. They know you very well through Mrs. H.; she had left again for Darmstadt after a visit here; she, I understand, is very well. The doctor however has not made so much ground as he should have done. We shall be occupied all this day (Saturday) about Giessen, then Dr. W. and I go to Marburg to-morrow (by rail) to see Bunsen. On Monday we propose starting with his married sister for Frankfort, next day to Darmstadt, and Wednesday to proceed direct to Switzerland. If you should write a line, it would be best to address me to "*Post Restante*" Berne.

The season in Germany has hitherto I think been scarcely equal to England. This, however, is a magnificent day, and they hope for a warmer Autumn.

I am living with the Professor, and have the world of news and information. You will be glad to hear that he is entirely reconciled to Dumas. They had met and spent a few days together, at Lille I believe. The minister of commerce had in command from the president to present the decoration of the Legion of Honour to the Professor. Liebig will be over for ten days at Easter to see the great Exposition, with every body in Germany, and intends to bring Madame Liebig.

[Between 1844 and 1855 Graham was at work on ten papers —eight on small matters, but two of great importance, being his celebrated papers on the "Motion of Gases," and the "Diffusion of Liquids."]

In 1855 Graham succeeded Sir John Herschel as Master of the Mint. On his appointment to the Mint, he laboured assiduously and successfully in acquiring a thorough knowledge of the technical work and financial relations of his office, and discharged his duties with much energy and judgment. It is known that he brought about various reforms and economies in the working of the establishment; but the service for which he will be chiefly remembered was the introduction of the new bronze coinage, which, besides substituting a more convenient medium of circulation than that in previous use, was attended with a pecuniary profit to the State of very large amount.

The Mint, Nov. 7th, 1855.

To Miss Graham, Nice,— . . . I have not happened to remark anything of late respecting the state of Rome and other Italian cities as regards cholera, but if they are suffering from an autumn attack that is likely to be on the wane. I shall send, however, a few daily papers to Nice, and if matters are really bad you are likely to find some notice of them.

We are keeping up our high rate of work (£420,000) weekly at the Mint, although it requires constant vigilance on my part, particularly in the absence of Mr. Barton, whom I wish to make out his month's holiday undisturbed. He returns in a day or two. . . . .

London, Dec. 1st, 1855.

To Miss Graham, Rome,— . . . . I hope you will find a healthy, comfortable apartment, and would recommend you not to be too parsimonious on that head.

Here things have been going on much as usual. But I have one surprise for you. Dr. Lee, of Hartwell, has just married a lady of twenty-five. The first news of the happy event, coming through Admiral Smyth, was set down as a joke, but it has been published since in the *Times.* At the Royal Society Anniversary Dinner yesterday, where I first heard this, not a bad joke was current respecting the new Lord Mayor's dinner, Salomons, being, you know, a Jew. One of the city waiters was addressing the visitors—"Gents., would you have fish," and when Baron Platt remarked that it was too bad to address judges in that manner, his neighbour at table asked if the Baron did not know that "Gents." was short for Gentiles!

Faraday called at the Mint, looking uncommonly well after a sojourn of three months at Brighton, and in first-rate spirits. He had many enquiries about you. Sharpey also appears to be somewhat better in health.

Playfair says that Lord Palmerston does not look favourably upon the decoration of the English jurors by the Emperor, and that the crosses will be somewhat contraband on this side of the channel; it is the aristocratic feeling coming out.

I should mention that Lord Wrottesley, notwithstanding his omissions of the *soirées*, is giving much satisfaction as President of the R. S. The two Royal medals were given one to Hind, Mr. Bishop's Astronomer, and the other to Westwood the Naturalist. The Copley medal to M. Foucault (pendulum movement, showing rotation of the earth, &c.) I dine to-day with Dr. and Mrs. Arnott to meet the latter. He made a pretty French speech at the Anniversary Dinner, where, by the way, I was myself called up, Sabine giving me the alternatives of proposing the health of the President or returning thanks for "Her Majesty's Ministers," of which I preferred the former. . . . .

Dec. 13th, 1855.

To Miss Graham, Rome,— . . . Dr. Hoffman had an accident to his eye which was at first a little alarming, and got, I believe, into the newspapers. The Rev. Mr. Barlow had caused an explosion of sulphuric acid, a small drop of which found its way past the glasses into the Dr.'s eye. He was confined to a dark room for a week or ten days in consequence of the inflammation, but is now going about again without having suffered any permanent injury. You may imagine what a vexation the accident was for poor Mr. Barlow, who attended at Fitzroy Square like a nurse. The Dr. promises a musical party as an opening of the house; the furnishing goes on gradually. At the Mint we have continued to keep up to our six deliveries a week (£430,000), although unusually troubled with unworkable gold, of which I was obliged to complain to the bank. The mischief I have since traced to a small quantity of iron in the gold, which is that of "Ballarat" and the "ovens" simply melted down, whereas it ought to be refined before being sold to the bank and sent to us. The bank melters, Brown and Wingrove, promise to look after this in future. I am gradually and quickly suppressing the abuses of the coining department, and Mr. Brande now submits to my innovations with resignation if not with a particularly good grace. They were chiefly ridiculous extra charges which the men had been allowed to make, the passing of which every week was a recurring vexation which I now escape. . .

Jan. 8, 1856.

To Miss Graham, Rome.—I had your long and amusing letter giving your visits to the Cardinals and French Commandant, for which many thanks. It has since been forwarded to Cooper at Southport. On calling on Mrs. Wilkinson, a few days ago, to deliver your message, I received an emphatic contradiction from her of the report that Dr. Wiseman was going to Rome. She rather opened her heart to me on the occasion, telling me that she is the only Catholic convert in the family, her husband having lived and died—"poor man"—a Protestant clergyman. Her brother Mr. Porrett, is High Protestant also, and a little intolerant, so that the good lady holds to her adopted faith under what she considers no small persecution. She was present at Oscot College when Mr. Talbot was first received; he gave up an English living. When she was fifteen or sixteen, the father of Mr. Hemans was devotedly attached to her, but afterwards married Felicia. She saw a young man once at Oscot Church, in whom she recognized the form and features of her old admirer, although the young man was shorter and not so good looking; this is your Mr. Hemans. She is very well and sends her compliments.

I got tolerably through the holidays all things considered. First—I went to a German tea, with which No. 9 Fitzroy Square was inagurated, but here made two mistakes—dining beforehand, for the repast was of a most substantial character, recalling the tea-dinners of primeval times. The other, for which in some degree I must blame the Doctor, who had put at the bottom of his invitation, "evening dress with decorations," was appearing on the occasion in white choker, black coat, buttoned up to the throat, and the Rosette and Star of the Legion of Honour blazing on the left breast. Coming late and proving to be alone in my glory, the effect upon the company, and especially the female portion of it, was magnificent, perfectly stunning. The Dr. himself had not mustered courage for such an exhibition, and there was no other *decoré* present. Miss Hoffmann is evidently more comfortable in their own house than she was formerly in her brother's lodgings.

Secondly—I ate my Christmas dinner at W. Tookes, Russell Square, who has lots of grandchildren, and had the honour to take out Mrs. Arthur Tooke, an old Pau friend of yours. . . . . Also, old Thomas Tooke, the distinguished political economist, Dr. Gray of the British Museum, and a remarkable lady whom I must not forget, Mrs. Grant, who has been to Pau and everywhere; she has a daughter a Ward in Chancery, whose property Mr. Tooke manages. Mrs. Grant has been presented to all the Queens of her time I believe, and discussed the royal bearing of each. She is not altogether satisfied with the Empress of the French, who is a nice gentleman's wife, but wants *fierté.* The Queen of Spain disgusted her. She pronounces our own Queen, little as she is, to have dignity, and to look the Queen more than any of her compeers.

It is a little curious to find myself taken into council by the Government on financial questions, and to be talking over affairs of state with the Chancellor of the Exchequer and Secretary of the Treasury, over a parlour fire in Downing Street. The fact is, that finance at present offers a magnificent career if one could give up Chemistry; but no, I must remain true to my first love.

Paris, 3rd Oct., 1856.

To Miss Graham,—Pelouze was at his post, and from him and the contractor at the Mint I have every information that I can desire. We were to-day at the manufactory of MM. Rousseaux, and saw the production of both sodium and aluminum on a grand scale.

I shall have scarcely occasion to go beyond Paris, and will probably remain over the meeting of the Institute on Monday. I propose to take Brussels on my way back.

30th Dec., 1857.

To Mrs. James Reid,—The five-pound gold piece was struck expressly for you as a *souvenir* of the Mint. It is considered one of Wm. Wyon's best works. "Una and the Lion," and such pieces are so scarce that they bring a good premium. I am only afraid that the gold will burn in the pocket of such a thrifty person, and that you will be disposed to realize.

[From 1855 to 1860 Graham published nothing of importance, but in 1861 appeared his celebrated paper on "Liquid Diffusion applied to Analysis."]

16th May, 1861.

To MISS GRAHAM,—The issue of the new bronze is going on satisfactorily, all that is manufactured being out or bespoke.

Last week has been a week of *soirées.* I could not avoid being out every evening. Among the rest Lord Ashburton's, as President of the Geographical. I had not seen Lord A. since his marriage. He introduced me very kindly to Lady A. Wild, you will be surprised to hear, has given up his office at the Royal Society. There is some difference between him and the other (superior) officers, which has not been explained. He certainly was not generally liked.

I was able to send in a paper to the R. S. last week, which contains two or three years' work, and has cost no small effort to get up amid my other distractions. I need only describe it as 98 folio pages in length. I expect also to hand in two short ones, all but ready, before the recess. This is an unspeakable relief to me, for nothing depresses me so much as the falling behind in my scientific career, consequent upon my official engagements; but I hope to recover in some considerable degree my place in the race by these papers.

17th.—I had a visit to-day at the Mint from Prince Arthur, a fine bright boy. He certainly promises well.

7th June, 1861, from London.

To MISS GRAHAM, Pau,— . . I forget if I told you of the last move of the officers of the R. S.—it was to place me as Treasurer and Vice-President in succession to Sabine, the latter being elevated to the chair. This would have suited me better, as the duty would have been light; but, after some hesitation, I have felt it necessary to decline this also. W. Crum and Dr. Hoffman agreed with me in the propriety of this resolution. Sharpey is, however, much disappointed, as he had set his mind upon such an arrangement, which he says would have made Sabine's presidency go down better with the body of the Society. But the overwork of last winter and spring has made me nervously afraid of encountering engagements merely for honour's sake. My chemical work, on the other hand, is going on very nicely. . . .

4th Nov., 1861.

My Dear Master,—I am very glad to be able to inform you that Lord Palmerston has sanctioned the appointment of your brother to the office of senior clerk and assistant coiner in the Mint.

This may be attributed not only to the testimonials of superior fitness for the office which your brother possesses, but I hope that you will also consider it a mark of the estimation in which, for your services and the economy you have introduced into the department over which you preside, you are held.

Believe me,

My Dear Master,

Very truly yours,

The Master of the Mint.

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7 Nov., 1861, from London.

To Miss Graham, Pau,—I am enjoying my holidays, after a way. The change in the weather, wind and snow, sent me up from Hastings after a few days. The business at the Mint has been brought up so that things can go on for a few weeks without my intervention. Mr. Barton and John have it all their own way, and I believe both will be improved by the responsibility. In the meantime I fully enjoy my temporary release.

The election of President of the Royal Society comes on upon the 30th inst. Dr. Roget is still urgent for Lord Brougham, and he will be proposed. The election might, I believe, be easily secured by a little organization among his supporters. It would be an appropriate close to the career of the old hero. In the meantime, from necessity as well as choice, I keep myself entirely out of the melée.

In making holiday I looked in at the National Gallery yesterday and found Mr. Wornam the Director. The change is surprising.

That Turner room is really a National treasure. I wandered out afterwards to the Zoological where I met Lewis with "George Eliot" on his arm. I had never seen the lady before, she has a long face, and her attractions do not reside in her looks—so far for first impressions. Last week I had a very agreeable communication to make to Mr. Aitkinson and University College. My old friend Mr. Evans of Worcester (Hill and Evans, the vinegar makers) gave me the first intimation to the College, of a small legacy to U. C. under a will of which he is one of the Executors. It is about £60 per annum, without any restriction. The testator was an old gentleman of the name of Webb, 90 years of age.

9th Nov., 1861, from London.

To Miss Graham, Pau,—The issue of the new bronze pieces goes on successfully, and we have the public with us. The change from the old to the new will, I believe, be roughly completed within the Metropolitan district in the course of a few weeks more.

11th Dec., 1861, from London.

To Miss Graham, Pau.—I have obtained another senior clerk, a kind of deputy to John, principally for the outdoor work. He is a Mr. C. F. Macdonald, of Manchester, who was recommended to me by George Wilson, and of whom Mr. Gladstone had also heard from other sources.

In this business I have allowed an extremely simple system to develope itself, which has kept the work falling on the department at a minimum, and procured us an amount of gratuitous assistance all over the country which is really surprising. We have had nothing to retract, had no mishap of any kind, and every step we take I see my way better, and can advance more confidently.

. . . I have had letters from old chemical friends respecting my paper, which is being translated into German and French, and find that it is attracting attention. Professor Schönbein, of Basle, writes an amusing letter.

14th Dec., 1861, from London.

To MISS GRAHAM, Pau.—The bulletin of the Prince Consort's health is favourable this afternoon. Dr. Arnott says that all ground for uneasiness has ceased. Lord Monteagle, with whom I was talking, says that so serious did things appear yesterday that the Duke of Cambridge was called away from parade.

Miss Coutts has been to Compeigne, and reports the Emperor as speaking strongly against the Americans. He wished they had tried it on one of his ships.

16th Jan., 1862, from London.

To MISS GRAHAM, Pau.—Doctor Hoffmann has had a tempting offer of a Chemical Professorship at Bonn. His countrymen are anxious to have him back, which I can well understand. I was telling him his countrymen must look upon him as the "missing planet" between the orbits of Bunsen and Strecker. He was much shaken by the offer for several days, but he is too well situated in every respect here to make a change.

James Young, with John Thom, Dr. Angus Smith of Manchester, Dugald Campbell, and John, dined here the day before yesterday. The gathering was due to Mr. Young's friendly anxiety to support the claims of Dr. Smith to the Chemical Chair in Aberdeen, which has become vacant by the death of Dr. Fyfe.

In case the Rev. Mr. Yorke should be disposed to look into a correspondence on the glacier question, I beg to enclose a letter from Professor Hopkins, the eminent Mathematician and Physical Geologist, who is to lecture on the subject at the Royal Institution on the 31st., with my explanatory reply. I do not care to publish more on the subject till I have had an opportunity of making a few experiments to illustrate the subject.

3rd February, 1862, from London.

To MISS GRAHAM, Pau.—Since I had your last favour, Professor Hopkins' lecture on the glacial theories came off at the Royal Institution. It was very clear and satisfactory so far as it went, but of too elementary a character to take up such a point as the colloidal condition of ice. It was entirely decisive against Forbes's confused views of viscosity. A peculiar idea was brought out by Mr. Hopkins to account for the glacier movement, so far as it is a sliding motion. He had observed that when a block of ice is placed upon a plane, such as a Welsh slate, slightly inclined, the ice will slowly move downwards. This slipping motion was admitted to depend upon the melting of the lower surface of the mass of ice in contact with the slate. But whence the heat to melt in such a way the bottom of a glacier? The internal temperature of the globe to be sure. The Professor had a mass of ice placed as described on a slate, to show this motion, but somehow or other it was "no go;" but Faraday, the moment the cause of the expected movement was given, darted off, and placed a gas lamp below the slate, thus bringing in the "central heat" with a vengeance. The movement speedily followed. There may be something in this motion of Hopkins', although the prevailing opinion is that the effect of the central heat upon the surface of the globe is too small now to be appreciated. But the rocky bed of the glacier is scratched, showing a sliding motion; to which it may be replied again, that the bottom surface of the glacier ice itself has been found scratched also—not melted away as the hypothesis seemed to require. Mr. Hopkins, however, insisted most upon the crushing of the brittle ice, and its subsequent "regelation" into a homogeneous mass in its new position. Dr. Tyndall's pretty experiment of crushing the ice into the form of a transparent cup by a Bramah press was successfully repeated. And here the lecturer stopped. Mrs. Faraday had her enquiries for you. She reports Mr. F. as pretty well, "but we must not let him work so much as he would like." The Bucktons were also enquiring for you.

While we are scientific, I may refer to Mr. Yorke's problem of the brandy cherries and your own preserved greengages. The sugar and much of the flavouring material come out and are distributed through the brandy. It strikes me that the thing would be made plainer by using a little bag of animal membrane (or parchment paper) instead of the greengage. Supposing the little bag was tied up filled with gum-water, and placed in the bottle of brandy. The first result would be (I believe) that the water would go out to the brandy, which greatly attracts it, and the bag would collapse. The difficulty of the brandy entering the bag is greater than that of the water leaving it. But in a certain time the proportion of brandy would be the same inside and outside the bag, the latter still much collapsed. So it would end were it not for the gum in the bag. Gum is one of those substances which cannot get through the walls of the bag (gum being a colloid), and must therefore remain there. But the imprisoned gum now attracts water to itself from without by *endosmose*, and the consequence is that the bag is again distended, the gages become plump at last. The water that has so got in is also followed by alcohol, attracted by the water.

You refer to the sad coal-pit accident. Some one has cut out of a paper and sent to me the enclosed slip. It was an old idea of mine, but the material mentioned should be kept in store in all mines liable to choke-damp. But still, it is the old story, the life-boat not at hand when wanted. The surest guarantee against such accidents is the Scotch law, which gives to friends of the miners an action against the mine proprietor. In Scotland each of the widows would have thus received, at least, £250.

14th February, 1862, from London.

To Miss Graham, Pau,—He (Mr. John Graham) is very much engrossed with our bronze issue, which is truly a considerable affair. It requires a great deal of attention, but goes on prosperously.

Last Friday evening we had an interesting lecture from Professor Huxley, at the R. Institution. The great point was producing a human skull from the bone cavern—the skull of the man who made the flint implements found in the drift. It appears that the skull in question was found so long ago as 1830 or '40, in a bone cavern near Liege, and was described by a good native geologist as being contemporaneous with the accompanying elephant and tiger remains; but nobody believed it. Lately, however, Sir C. Lyell went into the question, and on re-examining the skull, found it to be in exactly the same fossil condition as the other bones, and manifestly of the same age. It was a fairly formed skull. Next Friday evening Dr. Odling is to illustrate "Dialysis." Calvert, of Manchester, lately gave a lecture there on the same subject. Redwood followed.

Saturday was the private view of the picture exhibition of the British Institution, which I would not like to miss. It is a kind of harbinger of Spring, the snowdrop of exhibitions. There I met old artistic friends, Wornam, D. Roberts, your Caucasian admirer Solomon Hunt, R.A., &c. It was a pretty show, remarkable, as usual, for the large number of fair pictures, rather than for any salient production.

19th Feb.—I have allowed several days to elapse. Odling's lecture I hear from all quarters went off exceedingly well. The subject admitted of a number of pretty experiments which he made the most of. I dined on that day with Bence Jones, found Col. Thos. Yorke, Dr. Hoffman, a Miss Raphael or Gabriel (I forget which angel), a clever creature and great favourite of our host. The Franklands were expected, but by some accident did not come although they were at the lecture with Miss Colvill afterwards. I had also a quiet dinner with Dr. Sharpey last week, to meet a brother of Professor Kölliker's, who is, I believe, a Swiss merchant. All your friends were enquiring for you. Miss Hoffman is still very weak, and has not come down stairs, but I was admitted to a visit. She also desired to be particularly remembered to you. Their sister, Mrs. Raw, is with her. James H. and Carl are both well. The Dr. has cleared up lately with great success the nature of the magenta colours, which was very obscure. The amount of good work he gets through is something extraordinary. The printed copies of my *second* paper came to hand to-day. It is a much less general subject than the last, and not of the same popular interest. But I shall post a copy to you.

I am drawing up my report on the Hong-Kong currency, which seems to drive everything else out of my head for the time, and is the only apology I can offer for scarcity of news.

[In 1863 appeared Graham's paper on "The Molecular Mobility of Gases and Atmolysis"—the remaining years of his life being chiefly occupied by this subject and that of the occlusion of gases.]

11th Feb., 1863, from London.

To Miss Graham, Pau,—I am getting a head of the Queen engraved by a German artist, C. Weiner, who has come to settle in London. His first attempt is certainly good as a likeness, but too realistic to please. He will probably try again, and if a portable impression can be taken, I shall send it for your opinion. Weiner's designs for the Rumford medal met with a qualified approbation only in the Committee of the R. Society. The Committee included Sir C. Eastlake and Mr. Westmacott, the sculptor, who pronounce rather decidedly against the *taste* of the work. Weiner has improved the designs greatly, acting upon suggestions of Mr. Westmacott, and it will now probably be passed. The old dies to be superseded were quite wretched.

9th May, 1863, from London.

To MISS GRAHAM, Pau,— . . . . The fact is I had an evening occupation for the last two months which has nearly excluded everything else, correspondence in particular. It is the composition of a paper for the R. S., embodying several years' work—the subject, the Gases—which I was very anxious to get off my hands. It has cost more work (I mean investigation) than the dialysis, but is necessarily a little mathematical, and consequently less generally interesting. I am glad that it is now off my hands, having been delivered in fairly copied only last evening. . . .

I had a few words with Playfair last night, who had just returned from Osborne. He describes the Queen as in fair spirits, and able to speak of the late Prince, which she is constantly doing, without suffering. He is very thankful for the information you sent him respecting the education of his boy at Pau. It was a meeting of the Chemical, and I found Stenhouse there, remarkably vigorous. Hoffman also in great force.

I have avoided any inquiry about the Bonn business. It seems publicly rather to have passed over. Stenhouse gave the general impression that the Bonn salary, about £400 a year, would be insufficient for the Dr. after his London experiences. I am to dine with him next week at Mrs. Wilson's, and then may perhaps hear more of his intentions. There is a little stir just now with the appointment to a new office, the Inspector of Chemical Works, for which Dr. Angus Smith, of Manchester (James Young's friend), is the favourite candidate. Young was in town, and went with me to dine at the R. S. club. He considered himself very fortunate on the occasion, as he was placed between Sir Wm. Armstrong and Sir R. Hill. He had been in Glasgow, and spoke favourably as to Mrs. Reid's health. I have not heard anything direct from Blythswood Square for a long time. I accepted an invitation from the "divine Rachel" to visit Bushey Park and see the chestnuts next Sunday afternoon, dining afterwards with the Chadwicks, at Richmond. I have been little out for some time, but I dined last Saturday with Dilke. Except Willis, of Cambridge, and myself, the party were chiefly old family friends. The President's *soirée* followed, at which the Prince of Wales made his appearance. Dilke says, by the way, that the Queen has spent a great deal of money lately, that she insisted on refunding all the Danish expenses from Copenhagen to London, also the Hessian, "because they had not the money to do it." It appears that the English officers who escort on such occasions receive large *douceurs*, two or three hundred pounds. She also paid the hotel bills. Being in mourning, she did not receive the visitors in Buckingham Palace, which explains why her visitors were in hotels. . . . Enclosed you have a little piece for Hong Kong, a thousand in a dollar, or twenty in a penny. . . .

[This completes the letters of Graham, who belonged to a group of chemists of the first rank, whose education and experience were entirely acquired on British soil, and, moreover, of a group of chemists whose powerful generalizations materially advanced the science throughout Europe.

The matter which follows is exactly as written by Dr. Angus Smith, and appears to call for no further remarks by the Editor.]

As a private man Graham led an uneventful life; but no man has passed through the world more uniformly respected. Too retired, too quiet, his life appears to have a deep tinge of melancholy in it, notwithstanding its eminent success. Very intimate friends he had few out of the circle of the family of brothers and sisters, who were strongly attached to him, and to whom he was much devoted, being himself unmarried.

It was by Dr. Thos. Thomson's teaching that Graham first learnt Chemistry, and however much his boyish mind may have been attracted by the wonders of experiment, it was at a very early age I believe that he began to think of the recondite laws of matter. In 1826 there is a memoir by him on the absorption of gases by liquids, and his shrewdness and calm mode of speculation are as apparent there at 21 as at any time of his after life. He supposes for example that absorption and liquefaction of gases are regulated by the same fundamental properties.*

* Faraday's experiments on the liquefaction of gases were made in 1823. [Ed.]

Next year, viz. 1827, speculating on the atmosphere, he thinks that it may become liquid or solid when cooled to 284°C. He says that if the air cools at the rate of 1 degree for 300 feet, it may lose its elasticity at 27·27 miles, and temperature occasion a limit to diffusion. Here then we almost say we find him beginning his work, and next year we have a memoir by him on the absorption of vapours by liquids, and another giving an account of experimental researches on the diffusion of gases, seizing boldly the subject as it was left by Dalton and others.

In the same year we have the nucleus of his work on the diffusion of liquids. This I consider to be found in his paper "on the influence of air in determining the crystallization of saline solutions."

Whilst he was speculating on laws of liquids, vapours, and gases at this time he was also very much occupied with practical ideas, and one would almost have expected from his note book to see him come forward as a manufacturer, or what is now called a Chemical Engineer. Some of his thinking is to be found in *Chalmers's Journal*—referred to in his letters—when it was published monthly in small octavo, and even then able to say that "from the highest to the lowest, the various ranks of the community appear to vie with each other in the acquisition of all that in Science and Literature hath a practical bearing on their convenience or comfort." In the first number, page 25, we have a short article on the great weight of charcoal, signed G., and showing some of the remarkable transformations of that element. There is also an article on the advantage of copper bolts in soldering.

Another short article is a proposal to use a sand-bath as a cooking apparatus, an idea that might readily come from a chemical student in early days. This seems to have pleased the readers of the *Journal*, and brought out in number 2 a longer discussion of its advantages. He recommends also that ladies should have for themselves, outside of the kitchen, a sand-bath for "the more delicate and interesting processes, and to present to our countrywomen a fair field for the exertion of their culinary powers, unencumbered by the coarser details of the art." A drawing is given in the third number, and thanks presented for the encouragement he had received. In this number—that is, number 3, page 121. May, 1827—there is an article by Graham on a "Marine Amulet or Life-preserver, on a new principle." This is the production of carbonic acid in an air-tight bag. The bag may be put round the body, and take up so little room as scarcely to be felt or seen, but the moment it is immersed in water a valve opens, and allows a certain amount of water to enter. This enables an amount of tartaric acid and soda bi-carbonate to act, and the bag distends, A bag capable of holding a gallon, when distended, is filled to the extent of one-third in ten seconds by using half an ounce of each of the powders. It is worth while giving this more fully. The production of buoyancy is a most valuable quality, and if we can so apply gases that the act of foundering produces the means of elevation, we have gained a great point. May this principle not be employed to raise sunken vessels?

The following is the account given of "The Marine Amulet" in *Chalmers's Journal of Useful Knowledge and Monthly Miscellany of Arts and Sciences,* 1827.

"The instantaneous production of a large quantity of air is familiar to everyone in the case of mixing the solutions of soda powders. Could such an evolution of buoyant gaseous matter be made to take place in a proper recipient, and without trouble, upon the occurrence of an accident, a life-preserver, correct in principle, would be obtained. This principle might be applied in different ways, but perhaps the following is the most simple:—

"A bag of Macintosh's water-proof double cloth, of cotton or silk, being procured, and found upon trial to be air-tight, a collar of strong and stiff leather may be fixed to the mouth of it. This collar should be three or four inches long, and of a flattened construction, perhaps two inches across in one direction and three-fourths of an inch in the other. The open mouth would now be closed with a valve or lid, *opening inwards.* The valve or lid would, of course, be as wide as the collar, or two inches by three-fourths of an inch. Supposing the bag to be capable of containing four quarts, or a gallon, a quantity of the soda powders, amounting to half an ounce of each, may be intimately mixed, and introduced into the neck of the bag, still dry, by pushing back the valve. The apparatus may be then fixed to the person, the empty bag stretching across the breast, and the flattened neck or mouth being brought out from beneath the waistcoat at the right side, so as to be easily got at by the hand should that ever be necessary. As, when the apparatus is so arranged, the collar would be in a position somewhat oblique, most of the powder would rest upon the interior side of the collar, and not press much upon the valve, which would therefore be loose, and probably not altogether shut, although sufficiently so to retain the powder. A sudden plunge into the water would therefore be unfailingly attended by a considerable insinuation of the liquid into the neck of the bag, the valve yielding inwards to the stroke. But the moment that water comes in contact with the powders they violently react upon each other, producing a copious evolution of *incoercible air* [carbonic acid gas], which would violently press down the valve, and inflate the bag with considerable force. We have found by experiment that the bag, in such circumstances, is filled with air to the extent of at least one-third, which is enough to render a man buoyant within ten seconds; in twenty seconds the bag is more than half-filled, and within a minute it is completely filled, and all perceptible action at an end. Should a person invested with such a preserver be accidentally thrown into water, the air-producing apparatus would be brought immediately into action, and the bag forcibly inflated, although previously it had been flaccid, and had occupied very little space.

"The collar which is appended to the bag, and the valve or lid, which are the most important parts of this apparatus, might be constructed of various materials, and of any shape which was found most convenient. The most important circumstances to attend to are, that the neck or collar be of such materials as would not readily collapse or close together from external pressure, and that the valve be of considerable size, and such as would admit the water with ease. Should it be thought advantageous to admit more water than has entered at the first rush, the valve could be pushed inwards by the hand near it, and more water would enter; or, if it was perceived that more air would be produced than was necessary to inflate the bag to its full dimensions, from too great a quantity of the powders being employed, a momentary opening of the valve would remove the injurious excess. From the construction of the apparatus, and the manner in which it is attached to the body, it is evident that when in actual use in the water, the inflated bag would rise and maintain a superior position, while the collar and valve would be constantly under water—an arrangement which would secure the retention of the air in the bag. The air, too, within the bag would not be in immediate contact with the valve, which would be protected and made certainly air-tight by the water which had entered and induced the action."

At this point it is remarkable how varied was the action of Graham's mind, both in practical and theoretical Chemistry. But, perhaps, chemists are, by the nature of their studies, driven to a greater variety of subjects than most other men. It may be interesting, however, to remark, that we find in the note-book, 1827-1828, reasoning on—

Nitrification, ...	page 1.
Sulphuric Acid, ...	16.
Steam	25.
Steam-engines—Cylinder—Piston.	
Steel Pens.	
Miscibility of Gases.	

On this point, his note-book contains the following at page 144—"Dalton's theory, of the tendency of gases to mix with each other, that they are vacua to each other, is faulty, as it would occasion an instantaneous and complete mixture, which certainly does not take place, and I have shown previously that cold could be produced on mixture as a necessary effect of Dalton's supposition, which I have found on trial not to be the case. The gases must, therefore, actually press against each other. Aqueous vapour in the atmosphere must be pressed upon, and be really as dense as the atmosphere.

Suppose a vacuum made in the receiver of an air pump, as nearly perfect as possible. Admit a little water, the vacuum would be filled completely with aqueous vapour of light tension, upon admitting air into the vacuum, the first effect would be to drive the aqueous vapour before it, compress and condense it. If the bottom of the receiver was not dry, aqueous vapours would rise without the artificial introduction of water, and it is probably the condensation of such vapours which occasions the haziness often observed on the entrance of air. This, of course, is quite contrary to Mr. Dalton's theory of vacua."

It is interesting to see one of his earliest private thoughts on this subject, but, also curious to find that he, like many others, had not entered fully into Dalton's mind on the subject. Dalton never doubted that one gas could drive forward another, as a rod of pine would drive forward a rod of oak if pressure were suitably applied, although, at the same time, he never could clearly express the mystical relationship which he supposed each gas to have to the particles of its own kind. Graham too had not made clear to himself at this time the distinction between specific gravity and tension.

The same note-book speaks of Phosphates, Iodides of Carbon, Alcohates, Salts of Magnesia, Atomic weights, and Combining Volumes. I think he must have been early on this subject. Then we have the absorption of salts from solution, evaporation of saline solutions, water of crystallization, a subject on which he spent much time; Howard's patent for sugar boiling, Perkin's ideas of steam, and a plan for an Academy of Science, with some other discussions.

When Graham came to Glasgow, in 1830, to lecture at the Andersonian University, after he had been some time in Edinburgh, there was a good deal of interest excited about him, and I fancy this was felt more by the boys than the general population. I was told by a boy, now pretty far advanced in years, that a wonderful young man had appeared, one who, although only nineteen years old, had still a great reputation. I went to hear one or two of his popular lectures, and I remember his looks well. He might have been taken for nineteen, but, in reality, he was twenty-five when he first lectured at the Andersonian. He had the same quiet, rather stiff, and hesitating manner which he never lost. He did not cause enthusiasm by brilliancy of address, but a certain reserve and a certain feeling of power, as well as of ambition, which his letters prove to have been strong in him, so acted on his demeanour, that students became attracted, and were ready to work beside him and devote themselves also to his service. Amongst these was James Young, of paraffin oil celebrity, who, from a student, became an assistant, and played a part in the industrial world which was not less remarkable than his teacher's part in the scientific. I remember hearing him also spoken of as a very wonderful individual, but I remember little more, as he stands out before me, chiefly as a head sufficiently red and uncombed, by no means like the smooth yellow hair of his middle age, or the venerable white of his later years.

When Graham first lectured here, it was certainly to an audience ready in interest, a Mechanics' Institute had been formed some years before, and the University was proud of Dr. Thomson. Dr. Ure, a man of great mental activity and power, a man who might have been a force in Europe, preceded him in the Andersonian, but lost his position by attending too much to his interest. It was pleasant to find a young man who took an interest in young people. I well remember how far the sympathy went. One of my playmates, Edward Craig, became a small walking laboratory, his waiscoat pockets had each a range of several small vials. He had no test tubes, beakers, or basins, he asked us to put out our hand and to make it as hollow as possible. Into these "loofs" he poured his solutions, made precipitates, solutions and smells. That boy made more than one chemist; he himself did not continue the subject, but next year had his pockets full of abridgements of Locke, Bacon, and others, and on these he raised many discussions. Nothing came of Edward Craig, he had too little ambition in any direction. This was a little offshoot from the scientific schools.

Graham's direct influence was exercised in a manner which I rather think may be called quite new. I incline to call him the founder of the practical system of teaching Chemistry, at least in this country. Liebig was two years older, and opened his Giessen Laboratory in 1826; Graham began his very naturally, and not by imitation. His pupils wanted work, and he gave them his to do. He early got into a very languid mode of life, owing to simple constitutional want of elasticity. The work on water of crystallization was all done in these early days at the Andersonian, whilst Graham himself sat looking at the fire in weather not at all wet or cold, too languid even to look at the experiments. But the cause of this was partly mental. He was often in great straits for want of money. Once he required a silver crucible to make some experiments for a commercial firm, but he could not buy one and he was refused credit. In that Laboratory were taught some chemists of which Glasgow might be proud. Dr. Stenhouse, who lately died, may be mentioned first, as a man of great force of character and wonderful energy, who has left numerous results of his shrewd enquiries in botanical chemistry. Sir Lyon Playfair and Dr. Gilbert being still in full vigour, I shall only allude to them; the same may be said of Mr. John Thom of Chorley, who in reality did the experiments which based the theories of constitutional and saline water. The Griffins made a departure in another direction of no small advantage to the community, and the family still continues their important services to chemists. Crum and Harvey are still great manufacturing names in Glasgow. These names first showed themselves as attached to chemists in Graham's Laboratory. Blyth and Thom still hold their ground as great manufacturing names in Lancashire, and promise to continue to flourish.

As a scientific man, his claims were never disputed; he was not called to assert his position, and he remained the undisputed head of his department. He received in early life (1834) the Keith Medal of the Royal Society of Edinburgh, and the Royal Medal of the Royal Society in 1838, and in 1862 the Copley Medal. He was made a Doctor of Civil Law of Oxford, Honorary Member of the Royal Society of Edinburgh, Corresponding Member of the French Institute and of the Academies of Berlin and Munich, and of the National Institute of Washington. His election into the Royal Society was in 1836.

In 1841, in concert with some friends, he formed the Chemical Society of London, and was elected its first President.

GRAHAM'S CHEMICAL AND PHYSICAL RESEARCHES: AN ABRIDGMENT OF THOMAS GRAHAM'S WORK, CONTAINING AN ABSTRACT OF ALL HIS PAPERS.

GASES.

I.—Absorption of Gases by Liquids—1826.

From "Annals of Philosophy," xii., 1826, pp. 69-74.

LIQUIDS miscible in all degrees, as alcohol and water; or in limited degrees, as ether and water. Some mixtures exhibit chemical union. Water impairs the volatility of alcohol. Gases may be considered volatilized liquids, and such bodies may have the common properties of liquids.

It is assumed then that gases liquefied by pressure or other means will mix with liquids in some proportion. Sulphuric acid, at boiling, or 600° F. (316° C.), will liquefy steam of the same temperature, although it would require to pass down to boiling point to be condensed. Gases may owe their absorbability to their capability of being liquefied.

Gases already liquefied, by Faraday, easily absorbed.

The sp. gr. of the liquids formed, and of their solutions, are confirmatory. Dr. Henry's law, that absorption is directly proportional to pressure at variance, *ex. gr.*, in muriatic acid, but may be approximately true when absorption is small.

By diminishing temperature we diminish the absorption of a gas, not in the same but in much greater proportion. Amount of gas absorbed by a liquid depends on the gaseous residue or amount of gas exposed to the liquid. This is deducible from the gases being liquefied. Analogous instances occur with liquids.

Finally, absorption of gases is in the same class as miscibility of liquids.

II.—On the Finite Extent of the Atmosphere—1827.

From "Philosophical Magazine and Annals of Philosophy," i., 1827, pp. 107-109.

Wollaston's opinion that the limit of the atmosphere would be arrived at on mechanical principles. Faraday's illustration. But it may be that by being cooled down to 284° C. it becomes liquid or solid. If the air cools 1 degree for 300 feet it will lose its elasticity at 27·27 miles, and temperature occasion a limit to diffusion.

III.—Longchamps' Theory of Nitrification, with an extension of it—1827.

From "Philosophical Magazine and Annals of Philosophy," i., 1827, pp. 172-180.

Reason to doubt Glauber's theory, the prevailing one, that "saltpetre is formed by the decomposition of mineral and vegetable substances."

The solution of lime, by the free carbonic acid of the air, enables the oxygen and azote to act on each other more effectually.

IV.—Experiments on the Absorption of Vapours by Liquids—1828.

From "Edin. Journ. of Science," xvi., 1828, pp. 326-335 ("Schweigger Journ.," liii. (Jahrb. xxiii.), 1828, pp. 249-264).

Saturated solution of chloride of sodium absorbs water in a position where water evaporates, and where the crystals do not deliquesce, at 57° F., 14° C.

Saturated solution of muriate of ammonia and sulphate of magnesia.

Water lost 23 grains. A solution of chloride of sodium, 1 in 4 of water, gained 39 grains. Carbonate of potash, 1 in 4 of water, gained 6·5. Table of gain of water in six days, and in fourteen days in 10 solutions of salts. Not only deliquescent, but even efflorescent salts, absorb water when in solution, saturated solutions being used, except in the case of carbonate of potash and chloride of calcium.

All saline solutions just as readily inhale as exhale vapours, according to the atmosphere. Column of boiling points. Index of the *in*vaporating powers of the solutions. Liquids *in*vaporate when they take in vapour, and evaporate when they give it out. Gain in five days with chloride of sodium. Loss with sea water.

Examples of solutions which boil at the same point. Absorption of water by muriatic acid, at temperature not above 55° (13° C.) giving out acid and absorbing vapour till the strength is 1·0960.

Sulphuric acid absorbs the vapour of alcohol.

Absorption of alcohol vapour by camphor.

Absorption of carbonate of ammonia. Passage of dry subcarbonate into water.

V.—On the Influence of Air in determining the Crystallization of Saline Solutions—1828.

From "Edin. Roy. Soc. Trans.," xi., 1831, pp. 114-118, Author's Reprint ("Phil. Mag.," iv., 1828, pp. 215-218; "Sillimann Journ.," xvii., 1830, pp. 373-374).

Various opinions. Solutions of Glauber's salts might be kept in tubes inverted over mercury in a trough without crystallizing, if the mercury were heated first, and the whole allowed to cool gradually.

Sometimes solutions did not crystallize by introducing a bubble of air, at temperature not exceeding 150° or 170°F.; in all successful cases crystallization commenced around the bubble of air. Effect may be by absorbing air, the smallest increase causing a change in saturated solutions. The most absorbable gases had the greatest effect.

Ammonia and sulphurous acid act vigorously. Hydrogen, less influential than common air. Minute quantities of foreign liquids, soluble in water, act well. The solution does not expand as water does in freezing. Expansion, temporary, caused by rise of temperature of 20°—30°F. (11°—17°C.).

VI.—A Short Account of Experimental Researches on the Diffusion of Gases through each other, and their Separation by Mechanical Means—1828.

From "Quart. Journ. of Science," ii., 1829, pp. 74-83 ("Poggend. Annal.," xvii., 1829, pp. 341-347; "Schweigger Journ.," lvii. (Jahrb. xxvii.), 1829, pp. 215-227).

Law that gases diffuse, developed by Dalton. Berthollet's results. Diffusion of the different gases into atmospheric air.

Evident that the diffusion of gases is inversely as some function of their density—apparently the square root. Diffusion of mixed gases into atmospheric air.

Of mixed gases, the more diffusive gas leaves the receiver in greater proportion than in solitary diffusion, and the less diffusive in less proportion than in its solitary diffusion.

By taking advantage of diffusion a light gas may be eliminated from others by a species of rectification—so of a denser gas by a converse method.

Diffusion of gases into other atmospheres than common air. Examples—hydrogen and olefiant gas, etc.

§5. Concentration of alcohol by spontaneous evaporation of water. It is conceivable that imperceptible pores or orifices of excessive minuteness may be altogether impassable (by diffusion) by gases of low diffusive power, that is, by dense gases, and passable only by gases of a certain diffusive energy.

VII.—Observations on the Oxidation of Phosphorus—1829.

From "Quart. Journ. of Science," ii., 1829, pp. 83-88 ("Poggend. Annal.," xvii., 1829, pp. 375-380; "Schweigger Journ.," lvii. (Jahrb. xxvii.), 1829, pp. 230-240).

Some curious properties of phosphorus mentioned. The presence of a minute quantity of certain gases and vapours entirely prevents the usual action of phosphorus upon the oxygen of common air.

$\frac{1}{400}$ of olefiant gas in common air, in July and August, prevented the smallest oxidation over water. With $\frac{1}{20}$ of olefiant gas in three months the phosphorus never became luminous. Essential oils prevent luminosity, so also 4 per cent. of chlorine in the surrounding medium, also 20 of sulphuretted hydrogen. Alcohol also at about 80°. Not so vapours from camphor, sulphur, iodine, benzoic acid, carbonate of ammonia, iodide of carbon, at 67°. Muriatic acid increases, nitric acid diminishes it. Phosphorus therefore cannot take oxygen from mixtures of olefiant gas.

In 1 of air and 1 of olefiant gas, luminosity begins at 200° F; 3 of air and 2 of ether vapour, luminosity begins at 215°; 111 of air to 1 of naphtha, at 170°; 166 of air and 1 of vapour of turpentine, at 186°. Luminous in air, with 50 per cent. olefiant gas under pressure of half-an-inch of mercury. Table of luminosity at different pressures and proportions.

Potassium oxidation retarded by vapour of ether or olefiant gas. Perhaps allied to the property olefiant gas has of preventing explosion of hydrogen and oxygen.

VIII.—Notice of the Singular Inflation of a Bladder—1829.

From "Quart. Journ. of Science," ii., 1829, pp. 88, 89 ("Schweigger Journ.," lvii. (Jahrb. xxvii.), 1829, pp. 227-229).

A bladder filled with coal gas became inflated in carbonic acid gas standing over water.

The outer portion of the bladder would be moist and absorb carbonic acid. The water would pass the bladder, and the carbonic acid would evaporate within it, as nothing, but the presence of carbonic acid within could prevent the disengagement of that gas.

IX.—On the application of Spongy Platinum to Eudiometry—1829.

From "Quart. Journ. of Science," ii., 1829, pp. 354-359 ("Erdm. Journ. Tech. Chem.;" viii., 1830, pp. 20-27).

Olefiant gas, which had been found to prevent the oxidation of hydrogen and oxygen, was found to lose this power when sedulously washed with caustic potash. Then the ball with platinum acts in a few minutes, and the heat is so great that some of the olefiant gas is oxidized, and carbonic acid always appears. The same with vapours of naphtha and essential oils. With sulphuretted hydrogen the latter gas only oxidized, and little of the free hydrogen. Sulphurous acid also hinders the oxidation of free hydrogen. The action of these gases is therefore unlike the action of the same gases and vapours in protecting phosphorus from oxidation. The action not influenced by diminishing barometric pressure.

X.—On the Law of the Diffusion of Gases—1838.

From "Phil. Mag.," ii., 1833, pp. 175-190, 269 276, 351-358 ("Poggend. Annal," xxviii., 1833, pp. 331-358; "Edin. Roy. Soc. Trans.," xii., 1834, pp. 222-258).

The object is to establish with numerical exactness the following law of the diffusion of gases. "The diffusion or spontaneous intermixture of two gases in contact is effected by an interchange of position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being in the case of each gas, inversely proportional to the square root of the density of that gas."

The volumes may be called equivalent volumes of diffusion:—air, 1; hydrogen, 3·7947; carburetted hydrogen, 1·3414; water vapour, 1·2649; nitrogen, 1·0140; oxygen, 0·9487; carbonic acid, 0·8091; chlorine, 0·6325. Gases being separated by a screen, with apertures of insensible magnitude, the interchange of "equivalent volumes of diffusion" is effected by a force of the highest intensity: if the gases are of unequal density, there is an accumulation on the side of the heavy gas. The process of exchange continues till both sides are equalized. Doebereiner's explanation, viz., the capillary action of the fissure in the case of a broken jar, is improbable; hydrogen is condensed and absorbed by porous bodies, according to Saussure, with greatest difficulty, and we have no reason to suppose that its particles are smaller than those of other gases. When hydrogen escapes air always enters.

Better than broken jars are Wedgewood stoneware tubes unglazed. But these are superseded by gypsum.

A diffusion-tube—a glass tube open at one end, and with a plug of gypsum at the other, was filled with hydrogen, over mercury, when that liquid rose 2 inches in three minutes; with water it is striking, the rise in a tube 14 inches long is 6 to 8 inches in as many minutes.

Amount of five gases absorbed in the pores of the stucco—oxygen hydrogen, nitrogen, carbonic oxide, olefiant gas, and coal gas, not in sensible proportions. Vacuity of the stucco one-third of the volume. Experiments on the diffusion of hydrogen in the air. Wet and parched plugs of stucco. Some anomaly in the action of hydrogen.

The more compact and dense plugs are the best. When the plug is lose, the return of air is notably diminished in the case of hydrogen. Passage of gases into vacuum in time through a stucco plug, not in direct proportion to pressure.

Tables of the times. The kind of gas in the receiver did not alter the velocity with which hydrogen entered under a certain pressure.

Dry sound corks answer well as a substitute for stucco-plugs. This passage of hydrogen is against Dalton's opinion that one gas is vacuum to another. Diffusion of carbonic acid. The gas confined over a solution of common salt.

Chlorine, Sulphurous acid, protoxide of nitrogen, Cyanogen.

Muriatic acid, ammoniacal gas, sulphuretted hydrogen.

Oxygen, Nitrogen, olefiant gases, carbonic oxide.

Carburetted hydrogen of marshes. General table. In the volumes of oxygen, nitrogen, and carbonic oxide, theory and experiment agree as closely as could be desired.

Density of any gas diffused into air, both being in the same state as to aqueous vapour, is obtained by the formula $D = (\frac{A}{G})^2$ where G is the volume of the gas diffused, and A the returned air. It is possible to come within 100th part of the specific gravity by operating on a cubic inch of gas.

Nitrogen and carbonic oxide diffuse without any contraction on either side. Same density. Inequality of density not essential requisite in diffusion. Plugs not used for some days did not diffuse. Hydrogen opened a passage in a few minutes, and they went on as before. Heat also restored the action; dust the cause of stoppage. Evaporation may be explained on the principle of diffusion.

Mechanism of respiration. Carbonic acid being carried out from the air-cells, oxygen is carried in.

Heavy carbonic acid exchanged for a larger amount of oxygen, and may explain inflation of the minute tubes. To insect respiration most distinctly perceived applicable. Diffusion takes place by interchange of position of indefinitely minute volumes of the gas, not between sensible masses. The law not provided for in the corpuscular philosophy of the day. Supplementary observations on the law of diffusion of gases.

Some gases mix more rapidly than others, still keeping in conformity with the law. This is connected, in the case of hydrogen, with the apparent deviation from the law of diffusion, mentioned p. 53. It is there shown that more hydrogen passes out than the exact quantity proportional to the return air.

XI.—On Phosphuretted Hydrogen—1835.

From "Edin. Roy. Soc. Trans.," xiii., 1835, pp. 88-106 ("Phil. Mag." v., 1834, pp. 401-415; "Erdm. Journ. Prak. Chem.," iii., 1834, pp. 400-416).

Spontaneously inflammable, prepared by heating phosphorous, lime, and water; not spontaneously inflammable when allowed to stand over water 24 hours, or when made by heating hydrated phosphorous acid. The peculiarity attributable to something adventitious. Not free phosphorus presumably, because that is less accendible than the hydrogen compound. Loss of accendibility over water caused by rise of oxygen from air in the water. Free hydrogen unexpectedly rendered the gas self-accendible, but not if quite pure.

Caused by nitrous acid vapour.

Spontaniety destroyed by charcoal, caustic potash,—phosphorous acid, strong arsenic acid, essential oils, and most hydrocarburets, alcohol, olefiant gas slowly and potassium.

Spontaneous action restored by small quantities of nitrous acid, not by a large amount, between 1 and 10 to 10,000 of phosphuretted hydrogen.

Nitric oxide acts only when nitrous acid is in it.

In the reaction there is a spontaneous formation of compounds of phosphorus and oxygen.

The gas obtained by ordinary processes probably owes its peculiarity to a minute trace of such a compound, analogous to nitrous acid.

XII.—On a New Property of Gases—1845.

From Report of Brit. Assoc. for the Advancement of Science, 1845 (Part ii.), p. 28.

The passage of gases into a vacuum. Speed of effusion. Proposal to find the density by speed of effusion. Passage of gases through porous bodies, called transpiration. Air more rapid than oxygen. Carbonic acid more rapid also, and under low pressure more rapid than air.

XIII.—On the Composition of the Fire-damp of the Newcastle Coal Mines—1845.

From Memoirs of Chem. Society, iii., 1845-48, pp. 7-10.

Only carburetted hydrogen, nitrogen, and oxygen found. The power of resisting oxidation remarkable in light carburetted hydrogen; cause of its being found in coal mines. Resists the action of platinum black, but permits other gases to be oxidated.

XIV.—On the Motion of Gases. Part I.—1846.

From Phil. Trans. iv., 1846, pp. 573-632; ii., 1849, pp. 349-362.

Necessary to keep apart the phenomena of the passage of a gas through a small aperture in a thin plate, and its passage through a tube of sensible length. Rate of discharge independent of the material of the tube.

Rate of discharge of gases by tubes has no uniform relation to the density of the gases. Passage of gas through a thin plate. Effusion tube. Transpiration.

Part 1.—Effusion of Gases.—Effusion of hydrogen into a vacuum by a glass jet.

Effusion of oxygen and nitrogen; of carbonic oxide; of carburetted hydrogen of marsh gas.

Effusion of carbonic acid and nitrous oxide; of olefiant gas.

Effusion into a vacuum by a perforated brass plate A, with results. Different gases pass through minute apertures into a vacuum in times which are as the square roots of their specific gravities. For a proper effect the plate ought to have no sensible thickness.

Tubularity of the opening quickens the passage of carbonic acid and nitrous oxide in reference to air, they being more transpirable and less diffusive than air. Effusion of nitrogen and oxygen, and of mixtures of these gases under different pressures, by a second perforated brass tube B.

Table of effusion of air, nitrogen, oxygen, and mixture.

Effusion of air, carbonic acid, oxygen, and mixture, at different pressures, by plate B.

Effusion of carbonic acid, air, and of mixtures of carbonic acid and air, at different pressures, by plate B.

Effusion of mixtures containing hydrogen.

Effusion into a sustained vacuum by platinum plate E.

Effusion of air of different elasticities or densities by brass plate B. The effusion time of air of two atmospheres falls below that of air of one atmosphere.

Effusion of air of different temperatures by plate F.

Effusive time of air of different temperatures is proportional to the square root of its density at each temperature. Moist air, great effect in opening fissures.

Part 2.—Transpiration of Gases.—Transpiration of air of different densities or elasticities by a glass capillary tube E.

For equal volumes of air of different densities the times of transpiration are inversely as the densities. Transpiration of air of different temperatures.

Preliminary experiments on the transpiration of different gases by capillary A.

Preliminary experiments on the transpiration of different gases by capillary B.

Preliminary experiments on the transpiration of different gases by capillary C, 1 inch long.

Preliminary experiments on the transpiration of different gases by capillary C, 2 inches long.

Preliminary experiments on the transpiration of different gases by capillary C, 4 inches long.

Transpiration by capillary H, 22 feet long, of glass, into vessels of different sizes.

Transpiration by capillary H with cupped ends. By cupping the end of ingress the passage of air became slender in the proportion of 509 to 496; cupping the egress caused no further change, pressure being between 28·5 and 23·5 inches.

Transpiration of different gases by a capillary tube of copper; tube, 11 feet 8 inches; diameter, 0·0114 inch.

Transpiration of different gases by a glass capillary E. This was 20 feet long and 0·0187 inch diameter.

All these tubes gave the same co-efficient of transpiration to each gas.

A certain length of tube most favourable.

A certain interference supposed to depend on friction.

Theory of transpirability, that it is a kind of elasticity, depending upon the absolute quantity of heat, latent as well as sensible, and more immediately connected with specific heat than any other property of gases.

Transpiration of olefiant gas.

Transpiration of nitrous oxide.

Transpiration of sulphuretted hydrogen.

Conclusions drawn from Tables XLII. to LV.

Tables of the observed transpiration of gaseous mixtures.

XV.—On the Motion of Gases. Part II.—1849.

From Phil. Trans., 1849, pp. 349-392.

The velocities of gases attain a particular ratio with a certain length of tube and resistance. After attaining this the passage of of gases become slower.

Transpiration velocity of hydrogen double that of nitrogen; that of carbonic oxide and nitrogen the same. Velocity of nitrogen and oxygen inverse to the densities, equal weights and not equal volumes being transpired.

Points demanding attention. Capillary tubes for transpiration.

Transpiration by capillary H reduced to 237·875 inches. Transpiration of equal volumes by capillary H of different lengths.

Transpiration of all gases does not become normal for the same length of tube or resistance.

Use of a thermometer tube of fine flat bore, capillary K, 52½ inches.

K shortened to 39·375 inches.

"	26·25	"
"	13·125	"
"	8·75	"
"	6·4375	"
"	4·3125	"

Use of capillary M 52½ inches thermometer, tube as above, bore cylindrical. K and M have 50 times the resistance of E and H capillaries of extreme resistance recommended.

Capillary P, a compound one, formed of 30 capillary tubes each 4 inches in length, in a sheaf 400 times the resistance of K and M. Gases compressed instead of being drawn into a vacuum.

Results obtained by this sheaf of capillaries of extreme resistance the most uniform of all.

In carbonic acid the carbon gives velocity to the oxygen, showing the important chemical bearing of transpirability.

Transpiration of various gases and vapours:—

Protocarburetted hydrogen.

Olefiant gas.

Ammonia.

Cyanogen. Hydrocyanic acid.

Hydrosulphuric acid.

Bisulphide of carbon. Sulphurous acid.

Sulphuric acid.
Chlorine.
Bromine, Hydrochloric acid.
Ether.
Methylic ether.
Chloride of methyl.
Water.
Alcohol, naphtha and coal gas.
Transpiration of air of different densities or elasticities.
Transpiration of air and other gases at different temperatures.
Transpiration of equal volumes at different temperatures.
Transpiration of air under pressure (into air) at different temperatures.
General results of the inquiry.

XVI.—On the Molecular Mobility of Gases—1863.

From Phil. Trans., 1863, pp. 385-405 ("Chemical News," viii., 1863, pp. 79-81; "Paris Comptes Rendus," lvii., 1863, pp. 181-192; "Pharmaceut. Journ.," v., 1864, pp. 166-171; "Poggendorf Annal., cxx., 1863, pp. 415-425).

The passage of gases under pressure through a thin porous plate or septum.

Description of the graphite diffusiometer. Pores so small that molecules only can pass, not gases in mass. Sole motive agency, the intestine movement of molecules. The hypothesis of the movement of gaseous particles, by Bernoulli, revived in recent times by J. Herapath.

The passage of gas through a graphite plate agrees with the times of diffusion, not of transpiration.

Diffusion tube with pressure of 100 mms. of mercury.

Diffusion tube described to obtain a torricellian vacuum.

Movement through graphite and stucco plates.

Stucco under pressure gives mixed diffusion and capillary transpiration. Movement through biscuit ware.

Passage through graphite closely proportional to pressure. Passage of a mixture of air and hydrogen.

Time neither diffusion nor transpiration. Gas altered in composition. Each gas impelled by its own molecular force. Separation is a consequence of the movement being molecular.

Permeation through the graphite plate into a vacuum, and the diffusion into a gaseous atmosphere due to the same inherent mobility of the gaseous molecule. The diffusive mobility is a property of matter fundamental in its nature.

The physical basis is the molecular mobility.

Diffusion of mixed gases into a vacuum with partial separation —Atmolysis.

Oxygen and hydrogen, amount of separation in proportion to the pressure.

Oxygen and nitrogen, separation of.

All porous masses will have some effect in separating mixed gases. The tube atmolyser—a clay tube within a glass or metal one to increase rapidity.

Separation of oxygen from hydrogen and nitrogen.

Interdiffusion of gases—double diffusion. Remarks on Bunsen's results.

Interdiffusion without an intervening septum.

A portion of carbonic acid travelled at an average rate of 73 millimetres per minute, of hydrogen a third of a metre in one minute

XVII.—On the Absorption and Dialytic Separation of Gases by Colloid Septa.

From "Phil. Trans.," 1866, pp. 399-439.

Part I.—Action of a Septum of Caoutchouc—Dr. Mitchell's experiments.

Important to remember the complete suspension of the gaseous function during the transit through the colloid membrane.

Passage of gases through caoutchouc.

The relation not that of diffusion. The first absorption of the gas by the rubber must depend on a kind of chemical affinity.

A film of caoutchouc has no porosity, and resembles a film of liquid in its relation to gases.

Liquids and Colloids have an unbroken texture, and afford no opportunity for gaseous diffusion.

Penetration of varnished silk.

Absorption of gases by a block of caoutchouc. Dialytic separation of oxygen from atmospheric air (1) by means of other gases; (2) by means of a vacuum.

Dialytic action of caoutchouc in various forms.

Dialytic action of silk cloth, varnished with caoutchouc on one side, slightly vulcanized.

Percolation of air through gutta-percha and other septa.

Part II.—Action of Metallic Septa at a Red Heat.

Platinum porosity when heated. Deville.

Platinum tube 1·1 millm. in thickness. Not permeable to hydrogen till of a red heat, and then more permeable than caoutchouc. Is the difference of temperature the cause?

Heated platinum, with oxygen and nitrogen, not sensibly permeable. Carbonic acid penetration incalculably small.

Heated platinum, with chlorine, not sensibly penetrating. Hydrochloric acid do., vapour of water do., ammonia do. Carbonic oxide, marsh gas, and olefiant gas not penetrating.

Hydrosulphuric acid not penetrating. Table of above results.

Absorption and detention of hydrogen by platinum.

A new property in platinum to absorb and retain hydrogen at a red heat, occlusion of hydrogen 3·79 vols. for wrought platinum.

Palladium.

Occlusion of hydrogen 643·3 vols. Hydrogen absorbed at natural temperature in vacuo.

Action of the occluded hydrogen on salts.

Palladium, its absorptive power for liquids. Alloy of silver and palladium.

Conclusions regarding the absorption by palladium. Hydrogen taken up as a volatile liquid.

Separation of pure hydrogen from a gaseous mixture. Passage of ether through palladium.

Speculations on the pores of metals, and the excess obtained in the diffusive co-efficient of hydrogen, caused by a small amount of liquefaction. Osmium-iridium, no absorbent power. This is consistent with its crystalline character.

Copper—occludes 0·306 vol. hydrogen. Gold—occlusion of nitrogen, oxygen, carbonic acid and oxide, hydrogen.

Silver—occlusion of above gases.

Iron—penetration by hydrogen.

By carbonic oxide 4·15 vols. Acieration and carbonic oxide.

XVIII.—On the occlusion of Hydrogen by Meteoric Iron—1867.

From Proceedings of the Royal Society, vol. xv., p. 502, May 16, 1867; "Comptes Rendus," May 27, 1867, vol. lxiv., p. 1067; "Poggendorf's Annal.," vol. cxxxi. (1867), p. 151.

That of Lenarto yielded 2·85 vols. of gas, with 86 per cent. of hydrogen.

XIX.—On the occlusion of Hydrogen Gas by Metals—1868.

From Proceedings of the Royal Society, June 11, 1868, vol. xvi., p. 422; ("Comptes Rendus," May 25, 1868, vol. lxvi., p. 1014; "Archives des Sciences," June 1868, vol. xxxii., p. 148; "Annales de Chemie et de Physique (4)," vol. xiv., p. 315; "Poggendorf's Annal," vol. cxxxiv., 1868, p. 321; "Philosophical Magazine (4)," vol. xxxvi., p. 63.

A thin plate of palladium in contact with zinc which is giving off hydrogen in sulphuric acid occludes that gas. Still more, when the plate is the negative electrode to a Bunsen battery of six cells. This gas is not given out in a vacuum, it is taken out by heat, or by reversing the position of the palladium in the cell, and evolving oxygen on the surface.

Palladium charged with hydrogen apt to heat spontaneously in the air.

Platinum charged with hydrogen by voltaic action. Soft iron occluded 0·57 vols. hydrogen.

Explanation by the electro-chemical theory.

Precipitated palladium occluding 982·14 vols. of hydrogen.

Penetration by hydrogen and carbonic acid. Liquid diffusion explains the passage of hydrogen through a soft colloid metal.

"Solution affinity" of metals confined to hydrogen and carbonic oxide.

XX.—On the Relation of Hydrogen to Palladium, and on Hydrogenium—1869.

From Proceedings of the Royal Society, vol. xvii., pp. 212, 500, Jan. 14, 1869; "Comptes Rendus," Jan. 18 and June 28, 1869, vol. lxviii., pp. 101, 1511; "Annales de Chemie et de Physique (4)," vol. xvi., p. 188; "Chemical News," vol. xix., p. 478; "Berichte der deutschen chemischen Gesellschaft (No. 2)," 1869; "Annalen der Chemie," vol. clii., p. 168; "Philosophical Magazine," December 1869; "Poggendorf's Annalen der Physik," vol. cxxxviii. (1869), p. 49.

Properties of occluded hydrogen, which if metallic would have to be called hydrogenium. Palladium charged with hydrogen is lowered in density. Emits bubbles when in water, so that the density is obtained best by measurement and calculation.

A piece of palladium wire 609·144 millims. long, charged with 936 vols. of hydrogen, was increased to 618·932 millims., showing increase of 9·778 millims. (or 23·982 inches gained 0·385 in.), increase in length 100 to 101·605, and in cubic capacity from 100 to 104·908.

Alloy:—Palladium 95·32.
Hydrogenium 4·68.

100· giving density of hydrogenium 1·708,

The palladium wire lost when heated 9·7 millims. below original length: gravity less: length only was contracted. Mobility of metallic particles. Combustion of the wire. Another experiment, hydrogenium 1·898 specific gravity, and a third 1·977.

Further trials, highest number 2·055, retraction of the wire continues in four experiments.

Loses its power to occlude hydrogen, and after 6 times only 320 and 330·5 volumes. Power restored partly by heat, and wholly by extracting the hydrogen by electrolysis in an acid fluid. Tenacity of palladium being 100, that of palladium and hydrogen was 81·29 electrical conductivity, copper 100, palladium 8·10, palladium and hydrogen 5·99.

Hydrogenium, a magnetic metal. Slight absorption of hydrogen at a high temperature. Speed of movement through palladium 1 millim. in thickness is 4 millims. per minute.

Alloy contains about 20 volumes of palladium, and 1 of hydrogenium. An approach to equivalents. Summary.

XXI.—Speculative Ideas concerning the Constitution of Matter —1864.

From the "Philosophical Magazine" for February, 1864.

It is conceivable that the various kinds of matter may possess one and the same ultimate or atomic molecule in different conditions of movement.

Ponderable matter and a common atom. The atom is not at rest. Motion due to primordial impulse. This gives rise to volume. The more rapid the movement the greater the space occupied by atoms. Matter of different density forms different substances. This not applied to known gases which are composed of molecules or groups of atoms. There may be infinite repetitions of such steps. Molecular volumes of different elementary substances have the same relation to each other as the subordinate atomic volumes of the same substances.

Volumes uniting retain a portion of the original movement and volume. This is chemical combination. The same result attained by hypothesis of a fluid medium caused to undulate. A special rate of vibration or pulsation imparted to a portion of the fluid medium. No incompatibility between the liquid, solid, and gaseous states. Possibility of something like an undeveloped condition of matter connecting two states.

Molecular mobility has an obvious bearing on the communication of heat to gases. The impact of the gaseous molecule, when more rapid, produces more frequent contact. A hot object in hydrogen is touched 3·8 times more frequently than in air. Dalton ascribes this to mobility. Hydrogen in an air engine favours the alternate heating and cooling of a confined volume of gas.

SALTS AND SOLUTIONS.

I.—On Exceptions to the Law that Salts are more Soluble in Hot than in Cold Water, with a new instance—1827.

From "Philosophical Magazine and Annals of Philosophy," ii., 1827, pp. 20-26.

Phosphate of magnesia prepared by precipitation.
Efflorescent. Solution cloudy at (120°F.) about 50°C.
Precipitate like anhydrous phosphate.

774 parts water dissolve 1 at 7°·2, and
1151 do. do. 1 at 100°.

Time required to dissolve it.

Mere continuance of heat did not increase the precipitate. A solution once precipitated by heat did not precipitate more except by greater heat. Efflorescence in hydrates shows a weak affinity for water.

As all the hydrates of salts lose water by heat, there is a point at which every salt diminishes in solubility.

Some substances insoluble in water whilst the hydrates are soluble, such as silica.

II.—An Account of the Formation of Alcoates. Definite Compounds of Salts and Alcohol analogous to the Hydrates —1831.

From "Edin. Roy. Soc. Trans.," xi., 1831, pp. 175-193 ("Phil. Mag.," iv., 1828, pp. 265-272, 331-336; "Journal de Pharm.," xv., 1829, pp. 105-124; "Poggendorf Annal.," xv., 1829, pp. 150-153; "Quart. Journ. of Science," ii., 1828, pp. 442, 443; "Schweigger Journ.," lvi. (Jahrbuch xxvi.), 1829, pp. 180-203).

Mode of concentrating alcohol by quicklime. Quicklime absorbs a small quantity of alcohol vapour.

Sulphuric acid absorbs the vapour of absolute alcohol, as of water.

Chloride of calcium absorbs it. Compound described.

Alcoate of nitrate of magnesia.

Do. of lime.

Do. of protochloride of manganese.

Do. of chloride of zinc; chlorides of magnesium and iron.

Dent-absorption of dent-oxide of azote by protochloride of iron and alcohol more than by the salt alone.

Absorption by the iron salt may show a tendency to deliquesce in an atmosphere of that gas, the retaining power being equal to the absorbing power.

III.—Researches on Arseniates, Phosphates, and Modifications of Phosphoric Acid.

From "Philosophical Transactions," 1833, pp. 253-284.

1.—Subsalts.—Description of subarseniate and subphosphate of soda.

Explanatory hypothesis that phosphoric acid is disposed to unite with three atoms of base, common phosphate of soda being phosphate of soda and water. For the water another base may be substituted, as an atom of soda in the subphosphate, or an atom of silver in the corresponding salt. Pyrophosphate produced by heating phosphate, not by subphosphate, as it leaves an excess of base. Phosphoric acid by burning phosphorus.

Water in the crystals of subarseniate of soda, probably 24 atoms differing from the neutral arseniate by an atom of soda substituted for water.

Subphosphate dried, heated, fused with protoxide of lead. Calcined with exposure to carbonic acid.

Subsesquiphosphate formed.

Subphosphates and subarseniate of potash.

Barytes, lime, and lead salts.

2. Neutral Phosphates and Pyrophosphates.—Clark on the action of heat on the phosphate of soda, and removal of the last atom of water.

Phosphate of soda has three atoms base—two soda, one water; added to earthy and metallic salts, gives precipitates, with three atoms base; one may be watĕr, the basic function of which is essential to the phosphate of soda. The pyrophosphate has two atoms base, and gives accordingly bibasic precipitates. Unusual appearance of the crystals after long boiling of the solution.

3. Of the Superphosphates. (1st) Of the biphosphate. Remarkable body. Its dimorphism. Crystals contain four atoms of water, and lose two at 212° F. (100° C.), and no more till 375° F. (191° C.)

Two atoms essential to the salt—one atom soda, two water, and a double atom of phosphoric acid—these three atoms replaceable by three of oxide of silver. Second variety of bipyrophosphate of soda. The biphosphate heated to 400° F. (204° C.) rapidly semifuses, and one atom of water comes off in ebullition.

Salt, still soluble, but altered. Precipitates silver white, and with two atoms to a double atom of the acid, pyrophosphate of silver.

Third variety of the biphosphate of soda. The above biphosphate heated between 400° and 470° F. (204° and 243 C.) for several days. Small part insoluble nearly, see p. 341. Solution exactly neutral. Probable composition.

Fourth or insoluble variety of biphosphate of soda. If sufficient heating were possible, water would probably be incapable of acting on it. Reasons for this. Fifth variety of biphosphate of soda. The preceding insoluble variety heated to low redness.

Description of its properties. Metaphosphate.—Not crystallized, changed. The acid coagulates albumen. The glacial acid was supposed to be pyrophosphoric. The hydrated salt retains one atom water heated to 400° F. (204° C.), becoming bipyrophosphate The anhydrous fused metaphosphate is not changed by heating for several days, change effected by water; dried sharply, or with sufficient alkali, becomes common sub-phosphate.

Metaphosphate of Barytes.—Insoluble even by washing in hot water, but on long boiling is decomposed and dissolved, biphosphate of barytes being in the solution.

4. On the Modifications of Phosphoric Acid.—Acid in common phosphate of soda has three atoms of base to the double atom of acid. Does not effect albumen. Other modifications pass into it in solution, after some days in water or boiling, on fusion with three proportions of fixed base. Pyrophosphoric acid of the fused phosphate forms salts with two atoms base. Does not disturb albumen, or make a precipitate in muriate of barytes.

The metaphosphoric acid disposed to form salts, with one atom of base to the double atom of acid. Made by heating the other modifications *per se*, or with only one atom of certain fixed bases. These three acids, even when free, have still their usual proportion of base, and that base is water, or they are a terphosphate, a biphosphate, and a phosphate of water.

Table of oxygen in the salts. Loss of water by heating phosphoric acid. Possible hydrates of the acid. Binarseniate of soda does not undergo the same changes as biphosphate.

IV.—On Hydrated Salts and Metallic Oxides: with Observations on the Doctrine of Isomerism—1834.

From "Brit. Assoc. Report," 1834, pp. 579-582 ("Liebig Annal.," xii., 1834, pp. 1-12; "Poggend. Annal.," xxxii., 1834, pp. 33-75.)

Certain salts mentioned with five or seven atoms of water retain one in more intimate union than the others.

Sulphate of zinc loses 6 atoms at 65° F. (18·2° C.), and retains one up to 410° F. (210° C.) This salt may be viewed as a sulphate of zinc and water, with 6 atoms of water of crystallization. This last atom discharges a basic function explaining the disposition of the salt to form double sulphates. Sulphate of potash may take the place of this water, and form the double sulphate of zinc and potash. Sulphate of lime, peculiarity.

Protochlorides and cyanides. Tin. Two oxides. Different hydrates. Various peroxides and some salts obtained as débris of hydrates by heating. Incandescent on heating to redness. Isomerism, which has been proposed to explain the change of character in bodies of the same composition, may often be explained by certain differences in composition when carefully examined, *ex. gr.*,—(1) The water of the phosphoric acids; (2) Some bodies are mere débris of chemical compounds; (3) The proximate constitution may be very different when the ultimate is the same. No evidence of cyanogen in fulminic acid. Tartaric acid and racemic probably have as little relation to each other as any two vegetable acids which could be named, certainly contain different radicals. (4) A minute trace of adventitious matter may affect a body. See phosphuretted hydrogen.

V.—On Water as a Constituent of Salts—1836.

From "Edin. Roy. Soc. Trans.," xiii., 1836, pp. 297-314 ("Phil. Mag.," vi., 1835. pp. 327-334, 417-424; "Annal. de Chimie," lxiii., 1836, pp. 45-64; "Erdm. Journ. Prak. Chem.," v., 1835, pp. 90-109, xv., 1838, pp. 437-439; "Liebig Annal.," xx., 1836, pp. 141-149; "Poggend. Annal.," xxxviii., 1836, pp. 123-142).

Water in salts of ammonia. Water of crystallization, feeble affinity. Water in the caustic alkalies has the function of an acid.

The inseparable water of acids, as of sulphuric acid, has been held to be basic, and that acid to be a sulphate of water and bisulphate or bitartrate of potash a double salt. Development of this view in the memoir on the phosphates. Now finds water to exist in a different state, replaceable by salt. The tendency of phosphate of soda to unite with more soda was traced to the existence of basic water in the former. The question arose, Does an analogous provision exist in such salts as form double salts?

In the sulphates of magnesia, zinc, iron, manganese, copper, nickel, and cobalt, an atom of water seems essential. In sulphate of zinc and potash one of the seven atoms of water in the first salt is replaced by sulphate of potash, and the six atoms of water remain as water of crystallization.

Supersulphates, double salts. Second atom of water in sulphuric acid replaced by sulphate of potash, but the first replaceable only by a true base. The sulphuric acid hydrate, therefore, has an atom of basic and an atom of saline water, just as the hydrous sulphate of zinc is a sulphate of zinc with saline water. No supersulphates of magnesia, &c., are analogous in constitution, and do not tend to combine. Sulphate of water with saline water. The primary sulphate. Specific gravity, 1·78. The dilute acid may be concentrated at a temperature not exceeding 380°F. (193°C.), without loss of acid, the water reduced to two atoms. At 400° or 410° F. (264°—210 C.) this hydrate begins to be decomposed.

Sulphate of water and sulphate of potash. Bisulphate of potash, without water or crystallization.

Sulphate of potash, sulphate of soda. Of the ten atoms of water in crystals of sulphate of soda, none essential to its constitution, crystallizes anhydrous from a hot solution.

Sulphate of Zinc with saline water. Heated above 410°F. (210°C.), loses the last atom of water, but regains it with evolution of heat when moistened. Sulphate of zinc and potash. Retains the six atoms with more force than the sulphate of zinc alone. The sulphate of potash does not neutralize the acidity.

Sulphate of zinc and soda. Not obtained merely by crystallizing together the mixed salts, but from a solution of bisulphate of soda and sulphate of zinc in a day or two.

Sulphate of copper and saline water. Loses its fifth or saline atom above 430° F. (221°C.) Takes it up with evolution of heat. Sulphate of copper and potash. Table of water at different temperatures.

Sulphate of copper and soda.

Sulphate of manganese, with saline water, five atoms of water reduced to one, at 410° F. (210° C.) Table of water at different temperatures. Sulphate of iron with saline water. The saline atom of water retained to the temperature of 535° F. (279° C.)

Hydrated sulphate of lime. A sulphate with one atom of water obtained by heating the hydrate over sulphuric acid in vacuo at 212°F. (100° C.) The salt can be made anhydrous at 300°F. (149° C.) Table of water at different temperatures. Monohydrate does not adhere like stucco. This power retained to 270°F. (132° C.) The heated sulphate—the débris of the hydrate, not to be confounded with anhydrate.

Anhydrous gypsum may be written with a – or minus after it to show that something is wanting. Anhydrate requires no – as it takes up no water. Sulphuric acid and other salts may be equally represented. This view of the constitution of sulphates must not be hastily applied to other classes of salts, as each has its own peculiarities to be studied.

VI.—On the Water of Crystallization of Soda-Alum—1836.

From "Phil. Mag.," ix., 1836, pp. 26-32 ("Liebig Annalen," xxiii., 1837, pp. 269, 270; "Poggend. Annal.," xxxix., 1836, pp. 582-585).

Of soda-alum. This alum, like potash alum, has octohedral crystals, but is said to contain two atoms more water than the latter. No proof of isomorphism of soda and potash, but rather that soda, plus two atoms water, is isomorphous with potash.

Mode of preparation. Efflorescent. Contains, like potash alum, 24 atoms of water.

VII.—Inquiry respecting the Constitution of Salts, Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides—1837.

From "Phil. Trans.," 1837, pp. 47-74 ("Liebig Annal.," xxix., 1839, pp. 1-35.)

From the paper on water in salts would be inferred an analogy between any hydrated acid and its magnesium salt. So the oxalate and nitrate of water resemble the oxalate and nitrate of magnesia. In certain subsalts of the magnesium class of oxides the metallic oxide replaces the crystallization water. As in the case of sulphates, an atom of water seems to be replaced in the oxalates, so as to form double oxalate.

1.—Of the Oxalates.—Oxalate of water, or hydrated oxalic acid. Three atoms of water one basic. The two others may be termed constitutional, and are found in the magnesium oxalates. By drying, two atoms are lost or none.

Oxalate of Zinc.—Retains its two atoms water strongly.

Oxalate of Magnesia.—Retains these two atoms still more strongly.

Oxalates of Iron, Nickel, and Cobalt.—Not obtained pure, as they appear to carry down part of the precipitant. No binoxalate of magnesia.

Oxalate of Lime.—Contains two atoms of water, lost readily. Oxalate of barytes—one atom water, and thus differs from preceding.

Oxalate of Potash.—Crystalizes with one atom of water. Binoxalate of potash contains three atoms. The constitutional water of the neutral oxalate displaced by oxalate of water, which has two atoms.

Quodroxalate of Potash.—The two atoms of preceding salt replaced by two atoms of hydrated oxalic acid. Has seven atoms of water, four of which it loses at 240° F. (116° C.), being the two atoms of water of the two atoms oxalic acid.

Oxalates of Ammonia.—Agree with those of potash. There is also a quadroxalate as well as binoxolate.

Oxalate of soda.—Least soluble of soda salts. Anhydrous. With oxalic acid forms a binoxalate. No quadroxalate. Absence of water probably shows an indifference for further combination.

Binoxalate of soda—three atoms of water. Like the potash, salt loses only two at 300° F. (147° C.)

Double oxalates.—One member of the magnesium class of oxides, namely—copper forms binoxalates with an alkaline salt.

Oxalate of copper and potash.—One form of crystal has two, the other four atoms of water. The second loses two by efflorescence. The oxide of water is substituted for the basic water of the binoxalate.

Oxalate of chromium and potash, with six atoms water.

Oxalate of peroxide of iron and potash.

Oxalate of peroxide of iron and soda.

These three salts occupy the same important position among the oxalates as the alums among the sulphates.

2. Of Nitrates. — (1) Hydrated nitric acid the nitrate of water. This has one atom of water as base, and three held less powerfully.

(2) Nitrate of Copper.—Two crystals. The prisms contain three, the rhomboids six atoms of water, and lose three readily; the other three cannot be expelled without loss of acid.

(3) Subnitrate of Copper.—Thinks the single atom of water to be the base, and the three atoms of oxide of copper in place of the constitutional water of the nitrate of water.

(4) Nitrate and Subnitrate of Bismuth.—The neutral nitrate contains three atoms of water like the nitrate of copper.

(5) Nitrate of Zinc.—Contains six atoms of water which cannot be reduced below three without loss of acid.

(6) Nitrate of Magnesia.—Contains six atoms of water. At heat of melting lead reduced to one atom. This intimate combination of one atom does not indicate a disposition to form a double salt, and is probably the result of a new arrangement of the constituents at a high temperature. No proof of the existence of supernitrates.

3.—Of Phosphates.—Table of phosphoric and salts. Nomenclature—monobasic, bibasic, and tribasic.

(1) Tribasic phosphate of soda, ammonia, and water.—The name expresses the view of the constitution of the salts.

(2) Tribasic phosphates containing oxides of the magnesium class.—Tribasic phosphate of zinc and water has two atoms of water difficult to expel, besides one basic. Tribasic arseniate of magnesia and water has fifteen atoms water of which three, one being basic, are retained at 212° F. (100° C.)

(3) Tribasic phosphate of magnesia and water—has fifteen atoms of water reduced at 212° F. (100° C.), to seven atoms one of which is basic.

(4) Tribasic phosphate of magnesia and ammonia.—This not a double phosphate, but formed from the tribasic phosphate of magnesia and water by the substitution of oxide of ammonia for the basic water of that salt.

4. Sulphates. (Chromates.) See former paper.

As to the suggestion that bisulphate of potash is a double sulphate of water and potash, and therefore nearly neutral in composition, if all bisalts are neutral in composition, what explains the condition of the bichromate? The vapour of anhydrous sulphuric acid is absorbed by sulphate of potash and chloride of potassium without decomposition. Chromic acid forms analogous compounds; with neutral chromate, it forms the red chromate, and with chloride of potassium Peligot's salts analogous to, but more permanent than Rose's sulphates. The red chromate may then belong to a new order of combination.

Sulphates hold water with remarkable force. That of copper retains its four atoms at 212° F. Alum an alkaline sulphate with alumina attached.

5. Chlorides.—Hydracids have a weak affinity for water.

The law seems to be that chlorides of the magnesian class have two atoms of water pretty strongly attached. Some have two or four more, advancing by two's.

(1) Chloride of copper, two atoms of water.

(2) Chloride of manganese, four atoms water.

(3) Protochloride of iron, four atoms water.

(4) Chloride of magnesium, six atoms water.

(5) Chloride of calcium, six atoms water, reduced in value over sulphuric acid to two.

(6) Double chloride of copper and amonium with two atoms water.

The disposition of the protocyanide of iron and of the cyanide of copper to combine with two atoms of cyanide of potassium may depend upon the cyanides of iron and copper possessing two atoms of constitutional water.

VIII.—On the Theory of Voltaic Circle—1839.

From "Brit. Assoc. Report," 1839 (Pt. ii.), pp. 29-31.

The polar molecules of a salt contain negative and positive, or rather chlorous and zincous affinities, called also halogenous and basylous. (Graham's elements of Chemistry, 1850, p. 239, etc.) Explanation of the apparent travelling of hydrogen to the copper plate, by a series of transferences, beginning with the first partially fixed atom of hydrogen (in the case of hydrochloric acid), *i.e.* by a propagation of a decomposition through a chain of particles of the acid (see also Elements of Chemistry, p. 272, etc.)

IX.—On the Constitution of the Sulphates, as illustrated by late Thermometrical Researches—1842.

From "Chem. Soc. Mem.," i., 1841-43, pp. 82-84; "Phil. Mag.," xx., 1842, pp. 539-541.

No heat evolved in uniting sulphate of magnesia with sulphate of potash, some with sulphate of ammonia, but on stirring the latter when the double salt fell the temperature rose 5°·40 F. (3° C.), and fell on resolution. No heat of combination, because sulphate of potash and water are equicalorous in the constitution of such salts.

X.—Experiments on the Heat disengaged in Combinations. Part 1—1842.

From "Chem. Soc. Mem.," i., 1841-43, pp. 106-126—ii., 1843-45, pp. 51-70. "Annal. de Chemie," viii., 1843, pp. 151-180; xiii., 1845, pp. 188-216. "Erdm. Journ. Prak. Chem.," xxx., 1843, pp. 152-183. "Phil. Mag.," xxii., pp. 329-352; xxiv., 1844, pp. 401-420.

Sulphuric acid.
Hydration of magnesium sulphates.
Sulphates and chromates of the potash family.
Double sulphates.

Table of heat absorbed by equivalent quantities of crystallized salts on dissolving in water

Simple relations observed between the heat disengaged by the sulphates of magnesia and zinc, which appear to belong to one class, whilst the sulphates of water, copper, and manganese belong to another.

XI.—Experiments on the Heat disengaged in Combinations. Part II.—1843.

Neutralization of various acids by hydrate of potash.

1. Neutralization of potash by nitric and hydrochloric acids.
2. ,, potash by sulphuric acid.
3. ,, bichromate of potash by hydrate of potash.
4. ,, acetic acid by hydrate of potash.
5. ,, oxalic acid by hydrate of potash.
6. ,, bicarbonate of potash by hydrate of potash.
7. ,, arsenic and phosphoric acids by hydrate of potash.

XII.—On the Diffusion of Liquids—1849.

From "Phil. Trans.," 1850, pp. 1-46, 805-836; 1851, pp. 483-494. "Annal. de Chemie," xxix., 1850, pp. 197-229; "Brit. Assoc. Report," 1851 (Pt. 2), p. 47; "Chem. Soc. Journal," iii., 1851, pp. 60-67; "Jour. de Pharm.," xix., 1851, pp. 394-401; "Liebig. Annal.," lxxvii., 1851, pp. 56-89, 129-160; "Phil. Mag.," xxxvii., 1850; pp. 181-198, 254-281, 341-349.

Any substance in solution is diffused uniformly through the mass of the solvent by a spontaneous process.

The subject has been obscured by the experiments being made witb a membrane which imbibes the substance and produces endosmose, not connected with diffusibility. A diffusibility like that of gases would afford means of separating bodies, and present a scale of densities for substances in solution analogous to vapour densities. Gay-Lussac's theory. Chemical combination attended solution with cold; dissimilar bodies combine, whilst soluble substances and their solvents are like, or analogous. Solubility is not the same as the intensity of the solvent force, the gradations of which may depend on unequal diffusibility.

Diffusion of liquid or dissolved carbonic acid slow compared with gaseous.

Mode of experimenting without a diaphragm.

Solutions of salts of the same density diffuse equally.

1. Characters of liquid diffusion. Diffusion of chloride of sodium.

Quantity diffused closely proportioned to the quantity of salt.

Diffusion increases with the temperature. Nearly the same for eight days, but diminishing.

2. Diffusion of various salts and other substances. 20 salt to 100 water at 60°·5 (15°·8 C.) for eight days. Salt, sugar, gum, albumen. Sugar less than half the diffusibility of chloride of sodium.

The diffusion of albumen very much lower than of saline bodies.

Chloride of sodium appears 20 times more diffusible; actually more so, as nearly one-half the matter diffused from the first consists of inorganic salts. Value of this low diffusibility in retaining serous fluids in the blood vessels.

Diffusion of solutions of 10 salt to 100 water, at 59°·5 F. (15°·3 C.)
Do. do. do., at 37°·5 F. (3°·05 C.)

Near equality of certain isomorphous salts.

Diffusion of acid solutions, 4 acid to 100 water, at 59°·3 F. (15°·1 C.)

Considerable latitude in the diffusibility.

3. Diffusion of ammoniacal salts of copper.

4. Diffusion of mixed salts.

Inequality of diffusion is not diminished but exaggerated in mixtures. Same observed of mixed gases.

5. Separation of salts of different bases by diffusion. The potash salts more diffusive than the soda salts. Experiments. Sea salt. Slowness of diffusion.

In the sea salt does magnesium exist as a chloride or sulphate? Will diffusion show this by taking out the most diffusible? Or will the most diffusible, the chloride, form because of its opportunities to diffuse? Liquid diffusion, as well as gaseous evaporation, can produce decomposition.

6. Decomposition of salts by diffusion. Bisulphate of potash. Potash alum. Ammoniated sulphate of copper. Alkaline salts with lime.

7. Diffusion of double salts. A mixture of sulphate of magnesia and sulphate of potash gave in diffusion the sum of the separate diffusions. The salts probably not combined immediately on dissolving.

8. Diffusion of one salt into the solution of another salt. Chloride of sodium does not resist carbonate of soda diffusing into it. Sulphate of soda does. Nitrate of potash is not resisted by nitrate of ammonia, although isomorphous. They are inelastic to each other, like two gases.

At a certain pressure attraction impairs the elasticity of a gas, so at a certain saturation the salt molecules tend to crystallize.

Further analogy between liquid and gaseous diffusion.

Diffusion of salts of potash and ammonia. Vials for diffusion holding 2,080 grains (134·74 grammes), outside vessel or water atmosphere 8,750 grains (or 571·1 grammes). Density 2, 4, $6\frac{2}{3}$, and 10 to 100 water by weight. Some at two temperatures.

Diffusion of carbonate of potash, sulphate of potash, and sulphate of ammonia. Sensibly equal except at the densest point.

Diffusion of chromate of potash and acetate of potash. Close correspondence, even to the 10 per cent. solution.

Diffusion of bicarbonate of potash and bichromate of potash. Close approximation: so that equality is not confined to isomorphous salts.

Diffusion of nitrate of potash and nitrate of ammonia. A slight superiority in the latter.

Diffusion of chloride of potassium and chloride of ammonium. Agree well: related to the preceding nitrates. Crystallizing attraction in strong solutions resisting diffusion.

Diffusion of chlorate of potash. Inferior to the nitrate. Its less solubility may be connected with a tendency to crystallize, uniformity of diffusion greater in weak solutions.

Diffusion of salts of the sulphate of potash class. Diffusion of 1 per cent. solutions. Ferrocyanide and ferricyanide of potassium.

Diffusion of salts of the nitre class.

Nitrate of potash and carbonate of potash.

Times of equal diffusion. A 4 per cent. solution of nitrate of potash diffused for 7 days, or as 1 to 1·4142, the square root of 2. Leading to the idea of solution densities, and further analogy with gases. Further trials.

Hydrate of potash. Compared with nitrate and sulphate.

Diffusion of salts of soda.

Diffusion of sulphate and carbonate of soda. Nitrate of soda.

Relation of salts of potash to salts of soda. Times of equal diffusibility as the square root of 2 to that of 3.

Diffusion of sulphate of magnesia.

Has half the diffusibility of sulphate of potash, and one-fourth that of the hydrate at 52° F. (12°·2 C.) Sulphate of zinc differed from sulphate of magnesia 4 per cent., although isomorphous.

Table of solutions of 1 and 2 salt to 100 water.

Summary of results:—Diffusion a property of a fundamental character. Supplies the densities of a new kind of molecules.

Relations in diffusion refer to equal weights, not to atomic weights, or to larger molecules, if not of equal weight, of weights with a simple relation to each other. Classes of equidiffusive substances. Division of salts of potash. Separation of salts by diffusion. Decomposition by diffusion. Use in the study of endosmose.

Supplementary Observations on the Diffusion of Liquids—1850.

Diffusion of hydrochloric acid. One of the most diffusive substances. Effect of time.

Diffusion of hydriodic acid, hydrobromic acid, and bromine.

Diversity of physical properties compatible with equal diffusibility in substances which are isomorphous.

Hydrocyanic acid. Nitric acid.

Sulphuric acid. Chromic acid.

Acetic acid. Sulphurous acid.

Ammonia.

Alcohol.

Nitrate of baryta.

Nitrate of strontia. Nitrate of lime.

Acetate of lead. Acetate of baryta.

Chloride of barium. Chloride of strontium.

Chloride of calcium.

Chloride of manganese. Nitrate of magnesia. Nitrate of copper.

Protochloride of iron. Sesquichloride of iron.

Sulphate of magnesia.

Sulphate of zinc.

Sulphate of alumina.

Nitrate of silver. Nitrate of soda.

Chloride of sodium.

Chloride of potassium.

Iodides and bromides of potassium and sodium.

Chloride of ammonium.

Dichloride of copper. Diffusion diminished by excess of hydrochloric acid. Same effect of acid on chloride of sodium. Effect of acids.

Bicarbonate of potash. Bicarbonate of ammonia.

Bicarbonate of soda.

Hydrochlorate of morphine.

Hydrochlorate of strychnine. Observations in this paper favour the existence of a close similarity or equality in diffusibility between certain classes or substances. Chlorides and nitrates and their acids show this, isomorphous salts also.

Additional Observations on the Diffusion of Liquids—1851.

In the following the salts of potash and soda only are used, and in four different proportions. Hydrate of potash.

Hydrate of soda.

Carbonate of potash.

Carbonate of soda.

Comparison of potash and soda. Sulphate of potash.

Sulphite of potash.

Sulphite of soda. Hyposulphite of potash. Hyposulphite of soda.

Sulphovinate of potash. Sulphovinate of soda. Oxalate of potash.

Sulphate and oxalate compared. Oxalate of soda.

Acetate of potash. Sulphate and acetate compared.

Acetate of soda. Acetates of soda and potash compared.

Tartrate of potash.

Tartrate of soda. Tartrate of potash and soda compared. Double tartrate of potash and soda decomposed by diffusion. In nine pairs of potash and soda salts the potash salts uniformly exceed the soda in diffusibility, the ratio sensibly the same, the hydrates not included.

XIII.—On the Application of Liquid Diffusion to produce Decompositions—1849.

From "Chem. Soc. Journ.," vol. iii., p. 60, 1851.

Observations made on the spontaneous diffusion into pure water of several salts already dissolved in 100 times their weight of water. Apparatus as before. The water of the water jar is an atmosphere into which the salts diffuse from the solution phial within. Together they form a diffusion cell. Time, seven days, unless stated otherwise. Temp., 50° F. (10° C.)

Salts diffuse from a one-per-cent. solution in equal times.

Decomposition of sulphate of potash by lime in diffusion cells.

Decomposition of sulphate of soda. Potash salt most decomposed.

Alkaline chloride very slightly decomposed. Decomposition of sulphates by lime in dilute solutions, which cannot deposit sulphate of lime. Decomposition of sulphates by carbonates of lime.

Effect in the soil. No decomposition in saturated solutions of chlorides.

An agency in the soil by which the alkaline carbonates required by plants may be formed from the chlorides of potassium and sodium, as well as from the sulphates. This brought into action by top-dressing with lime.

XIV.—On the Concentration of Alcohol in Sömmering's Experiments—1854.

From "Brit. Assoc. Report," 1854, pt. ii., p. 69.

The alcohol was, by Sömmering, concentrated in a bladder; it was here shown that gelatine had the same effect.

XV.—Liquid Diffusion applied to Analysis—1861.

From "Phil. Trans.," 1861, pp. 183-224 ("Roy. Soc. Proc.," xi., 1860-62, pp. 243-247; "Chem. Soc. Journ.," xv., 1862, pp. 216-270; "Liebig Annal.," cxxi., 1862, pp. 1-77, cxxiii., 1862, pp. 90-112; "Nuovo Cunento," xv., 1862, pp. 92-94; "Paris Comptes Rendus," liii., 1861, pp. 275-279; "Phil. Mag.," xxiii., 1862, pp. 204-223, 290-306, 368-380; "Poggend. Annal.," cxiv., 1861, pp. 187-192).

Volatility a means of separating bodies. Diffusion similar in character. Hydrate, sulphate of potash, etc., "volatile" as regards diffusion. The fixed class are silicic acid, albumen, gums, etc.: crystalline bodies the type of first; gelatine the type of colloids. Distinction intimate molecular. The crystalloid state is statical, the colloid dynamical.

Colloidal has energia. The source of force in vital phenomena.

Colloid substances assisting diffusive separations. Starch and pectin greatly resist passage of less diffusive substances, and entirely cut off colloid substances. A mere film of jelly is enough.

Instances with sized paper. Not as a filter. Only molecules permeate, and not masses. The water of the colloid is in a state of combination, although feeble, and cannot be used as a medium for diffusion. The crystalloid can separate the water, molecule by molecule, and make its way through the gelatinous septum. This action called dialysis.

Vegetable parchment or parchment paper made by M. Gaine. Unsized paper dipt in sulphuric acid. Dialysing vessel or saucers of paper. Jar diffusion explained.

Diffusion of 10 per cent. solutions of chloride of sodium, sugar, gum, and tannin in fourteen days, at 10° C.

Diffusion of sulphate of magnesia, albumen, and caramel for fourteen days.

Diffusion of chloride of sodium in different times.

Diffusion of a 10 per cent. solution of cane sugar in different times.

Hydrochloric acid compared with salts, sugar, and albumen. Diffusion in alcohol of iodine and acetate of potash. Diffusion of alcoholic solution of resin.

Diffusion of 5 per cent. solution of chlorides of potassium and sodium.

Diffusion of 5 per cent. solution of chloride of sodium and sulphate of soda at 10° C. for seven days.

Same for fourteen days. Can two unequally diffusive metals be separated by varying the acid?

Diffusion of 5·12 per cent. chloride of potassium and 4·88 of sulphate of soda for seven days at 14° C.

Diffusion of 4·01 per cent. chloride of sodium and 5·99 of sulphate of potash for seven days at 14° C. Effect of temperature on diffusion.

Dialysis.

Diffusion of a 10 per cent. solution of chloride of sodium in the jelly of gelose for eight days at 10° C. A crystalloid passes through a firm jelly with little or no abatement of velocity.

Heat. The effect of heat increasing diffusion is diminished in dialysis. Bell jar. Bulb dialyser.

Effect of volume of liquid in the dialyser. Effect of acids.

Dialysis through parchment paper during twenty-four hours at 10°—15° C.

Dialysis through animal mucus during twenty-five hours at 10° —15° C.

Dialysis through parchment paper during twenty-four hours at 12° C.

Purification of colloid substances by dialysis.

Crystalloids eliminated by diffusion. Soluble silicic acid. Hydrochloric acid and chloride of sodium removed from it in a hoop dialyser in four days. Pure solution of silicic acid, limpid and colourless, preserved a few days.

Coagulated by earthy or alkaline carbonates and carbonic acid, precipitated on calcareous stone without penetrating; acid like carbonic acid, precipitated by certain other colloids (ordinary silicate of soda not colloidal). Soluble silicic acid forms a peculiar class of compounds also colloidal.

Silicate of gelatine. Colli-silicates or cosilicates.

Cosilicic acid. Albuminic acid. Soluble alumina made by placing on the dialyser a solution of alumina in chloride of aluminium. Very unstable. Coagulated by extremely minute amounts of salts. Colloidal alumina has a high atomic weight, like cosilicic acid.

Soluble metalumina. Crum's discovery. Heating a binacetate to the boiling point of water, and keeping thus for several days; also by dialysing an acetate of alumina that has been altered by heat; not a mordant. Soluble peroxide of iron. Preparation.

Solution of the peroxide in the chloride goes on for months. Slow action characteristic of colloids.—Very unstable solution. Separation by diffusion.

Soluble metaperoxide of iron. Ferrocyanide of copper.

Neutral Prussian blue.

Ferrocyanide of iron, soluble in oxalic acid and binoxalate of potash. Sucrate of copper. Sucrate of peroxide of iron. Sucrate of peroxide of uranium. Sucrate of lime. Soluble chromic oxide. Hydrated uranic oxide and glucina. It appears that the hydrated peroxides of the aluminous type when free are colloid bodies; two species exist, of which alumina and metalumina are the types.

The double type in fibrin of blood.

Peroxide of tin. Metastannic acid. Titanic acid.

Dialysis of organic colloid substance.—Tannin diffuses, at most, 200 times less than chloride of sodium. Gum, 400 times less diffusive than chloride of sodium.

Gummic and metagummic acids.—Gummate of gelatine, dextrin, caramel, varieties of. Cane sugar, albumen.

Emulsin. Coagulated albumen. Gelatinous starch. Extract of flesh. Separation of arsenious acid from colloidal liquids.

95 per cent. of the arsenious acid diffused out in 24 hours. Amount with albumen and gum.

Arsenious acid with other organic substances.

Colloidal condition of matter.—Colloids and crystalloids different worlds of matter, distinct as the material of a mineral and the material of an organized mass. Colloids may be soluble or insoluble. Easily thrown out of solution by crystalloid. Crystalloids sapid; colloids insipid.

Tendency to change in colloids.—Colloids equivalents high. Possibly a collection of crystalloid molecules.

Gelatine an important colloidal base.

Ice.—Colloidal and crystalline. No abrupt transitions.

Osmose.—Caused by hydration and dehydration of the membrane. Outer surface being pure water is most hydrated. With full hydration through the membrane osmose is checked. Weak alkalies swell the colloids, and cause extreme osmotic sensibility.

XVI.—On Capillary Transpiration of Liquids in relation to Chemical Composition—1861.

From "Phil. Trans.," 1861, pp. 373-386 ("Roy. Soc. Proc.," xi., 1860-62, pp. 381-384; "Chem. Soc. Journ.," xv., 1862, pp. 427-445; "Paris Comptes Rendus," liii., 1861, pp. 774-777).

Poiseuille.—Greatest retardation of alcohol is with six equivalents of water.

Nitric acid.—Retardation greatest with three atoms water.

Sulphuric acid.—Twenty-four times slower than water. Least transpirability in the crystallizable hydrate, or one atom of water added to SHO_4. Transpiration time of water 109 seconds.

Acetic acid.—The hydrate $C_4H_4O_4 + H_2O$ has a transpiration time 2·7 times longer than water.

Butyric acid.—With three equivalents water greatest retardation.

Valerianic acid.—Retarded most by two equivalents of water.

Formic acid does not follow the same rule.

Hydrochloric acid.—Its retardation indicates a 12-hydrate. No appearance of a point indicating a 16-hydrate.

Alcohol.—Poiseuille's fundamental discovery confirmed by numerous experiments. Methylic alcohol, 6-hydrate of amylic alcohol.

Ethers.—Formiate, acetate butyrate, and valerianate of ethyl, give transpiration times progressively rising—this may be connected with the increased weight of the molecule.

Acetone.—Time greatest with twelve equivalents of water.

Glycerine.—No point of retardation found.

Importance of observing transpiration. Retardation of hydrates may be caused by the increased size of the molecules, but it is possible to conceive a decomposition caused by the friction of passage and the propelling power lost by conversion into heat.

Table of the transpiration of water at different temperatures.

Table of the transpiration of alcohol at different temperatures.

XVII.—On the Properties of Silicic Acid and other Analogous Colloidal Substances—1864.

From the "Journal of the Chemical Society," 1864.

Hydrate of silicic acid soluble according to hydration. A jelly with 1 per cent. of silicic acid gives a solution with one of that acid in 5,000. Time alone may cause coagulation which is opposed by concentration and warmth.

Pectization by pounded graphite. No crystallization. Colloids transpire slowly. The particles aggregate.

Alcohol and silicic acid.

Ether and silicic acid. Glycerine and silicic acid.

Sulphuric acid and acetic acid.

Restoration of the liquid from the pectose state. Liquid stannic and metastannic acids.

Liquid titanic acids. Liquid tungstic acid.

Molybdic acid.

UNCLASSED PAPERS.

I.—On the Heat of Friction—1826.

From "Annals of Philosophy," xii., 1826, pp. 260-262.

A speculation on the separate existence of heat.

II.—Alcohol derived from the Fermentation of Bread—1826.

From "Annals of Philosophy," xii., 1826, p. 363.

There was found alcohol equal to 0·3 to 1 per cent. of the flour employed.

III.—Effect of Animal Charcoal on Solutions—1830.

From "Quart. Journ. of Science," i., 1830, pp. 120-125 ("Dingler Polytechn. Journ.," xl., 1831, pp. 443-446; "Poggend. Annal.," xix., pp. 139-144).

Here it is shown by several instances, chiefly of metallic salts, that the charcoal removes more than mere colouring matter from solution.

IV.—Note on the Preparation of Chlorate of Potash—1841.

From "Chem. Soc. Mem.," 1841-43, pp. 5-7.

Loss of oxygen by acting with chlorine on carbonate of potash alone. Recommends addition of hydrate of lime. The chlorine attacks the potash, and the lime takes the carbonic acid at the same time. Similar result of secondary action found when sulphate of soda is added to lime, which mixture takes up two equivalents of sulphuretted hydrogen to one of lime.

V.—Lettre de M. Graham à M. Dumas—1842.

From "Annal. de Chimie," iv., 1842, pp. 177-186.

Some ideas arising from the law of substitution—even an isolated element, such as a metal, held to be made up of several parts. Combination the natural state of matter. Electro-chemical theory, and mode of writing the symbols under this theory. A view of the constitution of several molecules of compound bodies.

VI.—Note on the Useful Application of the Refuse Lime of Gas-works—1845.

From "Chem. Soc. Mem.," ii., 1843-45, pp. 358-359; "Erdm. Jour. Prak. Chem.," xxxvi., 1845, pp. 48-49; "Frankl. Inst. Journ.," xi., 1846, pp. 281-282.

By exposure to air refuse lime becomes quickly oxidized, and one-sixth of its weight has been taken from it as crystallized hyposulphite of lime by a single crystallization.

VII.—Note on the Existence of Phosphoric Acid in the Deep Well Water of the London Basin—1845.

From "Chem. Soc. Mem.," ii., 1843-45, pp. 392, 393; "Bibl. Univ.," lx., 1845, pp. 385, 386.

Deposits carbonate and phosphate of lime. Green confervœ grow readily. Does the value of water for irrigation depend on the phosphoric acid?

VIII.—Observations on Etherification—1851.

From "Chem. Soc. Journ.," iii., 1851, pp. 24-28; "Journ. de Pharm.," xviii., 1850, pp. 124-130; "Liebig's Annal., lxxv.," 1850, pp. 108-116.

The most direct method of preparing ether is to add 4 to 8 of alcohol to 1 of strong sulphuric acid of 83 per cent., and to heat to 320° F. (160° C.) under pressure. No distillation or sensible formation of sulphovinic acid necessary. Action of the acid com pared to that on the essential oils. A polymerizing action.

APPENDIX.

CRITICAL REMARKS ON THE FOREGOING PAPERS OF THOMAS GRAHAM, BEING PREFACE BY DR. R. ANGUS SMITH TO "PHYSICAL RESEARCHES OF THOMAS GRAHAM," PUBLISHED FOR PRIVATE CIRCULATION BY DR. R. ANGUS SMITH AND JAMES YOUNG IN 1876.

GRAHAM AND OTHER ATOMISTS.

ATOMS and eternal motion are amongst the first known scientific ideas. We find them discussed with full keenness of insight by the earliest Greeks of whom we have received definite accounts. If the opinions did not spring from Hellenic soil they certainly found themselves in a land well fitted for their development; but so many of the thoughts of men have come from the East that it is natural to suppose that both had their origin there. India has had several atomic systems and acute discussions on atoms and matter. I shall leave it to others to decide at what time the earliest known were written, but it is at least clear that the subject was thought out in a peculiar method, with occasional logic of the keenest known, moving forward into regions where reasoning is difficult and mysticism reigns. There we may find thoughts, the same apparently as led to the centres of force of Boscovich and original fluid or universal ether, as well as the various atoms of the Greeks, or the clearest ideas of moderns, too clear at times to be sufficiently comprehensive. It is said that Leucippus obtained his opinions from a Phœnician called Moschus (and Mochus), and it is pleasant to think of this earlier name of an atomist to whom it seems unnatural to deny existence, even if he be little known. And although Zeller[1] tells us that the roots of the ideas lay in the earlier Greek mode of thought, this scarcely is sufficient for separating the East and the West, since tradition in very remote times led to the belief in frequent and interesting communication. Such names are not forged, and when characters are invented we find them rarely to be men of science. Still, the accounts are vague, and Indian literature waits arrangement. Although Kapila as a philosopher is almost lost as a personality, his system[2] shows a long train of successfully cultivated thought. How and when did Cánáde's notion arise that an atom was the sixth part of a mote in a sunbeam? If, however, we turn to the second idea, namely, "eternal motion," we must claim for it decidedly an Eastern origin, and perhaps it is still to be found among the remnants where Chaldæans or Accadians may have recorded their thoughts, when their astronomers and astrologers sought wisdom out of the heavens. Science seems to have begun amongst

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1 In *Die Philosophie der Griechen*, p. 688.

2 Colebrooke's Essays on the Religion and Philosophy of the Hindus.

the heavenly bodies, and even amongst the Greeks Anaximander brought forward his mystic idea of the immeasureable, before the smaller movements in matter were considered. To follow a late writer,[1] even the idea of eternal motion, as held by that early Greek speculator, referred only to the motion of the stars and the unceasing changes of the sun and moon. When Leucippus gave his mind to the study of atoms he saw there also the need of movement, and the restlessness of all creation in detail was thus early expressed by him. It was the result of a process of reasoning, begun by observing objects that strike all men unceasingly, and ending amongst those too small to be visible; it began with what to us is infinitely great, and ended with what to our senses is infinitely small. Leucippus seems to have concluded, as so many thinkers have since done, that arguments concerning the infinite divisibility of matter must end in empty words, or in thoughts of awe regarding the unlimited, which we cannot comprehend. To assume an actual end of division was to take a basis of thought; without such a foundation where would he stand to observe the building up of the world? The assumption, if it be one, formed the atom of science, and its study, although without a distinct advance for ages, has richly rewarded the student of the present century, giving us the basis of a rational view of matter, as the atoms are the basis of the matter itself. In the mind of this early Greek, first or not, the action of the atom, as one substance taking various forms by combinations unlimited, was enough to account for all the phenomena of the world. By separation and union, with constant motion, all things could be done.

These great thoughts, the oneness of matter and the power of motion and combination, were given to gifted men of the early days, and, like some other great thoughts, they have been misunderstood or neglected to a very late time. It is because Graham took a similar view, and advanced the idea of atomic motion with unity of material to that which might be fairly called its utmost limit, that he is brought forward here with Leucippus. The Greek told us that all was in motion. Graham conceived the idea that the diversity in the motion was the only basis of the diversity of the material, or, in other words, that an atom constituted an element of a special kind, according to rate or peculiarity of its movements. To the study of the motion of atoms, or at least of molecules, the practical atom of our chemistry, or that which although in a sense divisible is not known to be divided, Graham gave all the leisure of a thoughtful life. When he came to the subject it had advanced far beyond the early stage, notably by the thoughts of Dalton and the chemists of the earlier part of this century. To pass over more than two thousand years in a bound is apparently to forget the effect these thoughts had on mankind; but it cannot be denied that no important results followed for the human race, and only a few men seemed seriously to think on the subject until very modern times. A glance at a few of the more striking halting-places will help us to see better the relative position of Graham as an atomist and a philosopher, words which suit his character better than chemist, although chemistry was his profession and the source of all his speculations and reasoning.

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1 Gustav Teichmüller, *Studien Geschichte der Begriffe.* Berlin, 1874.

Leucippus was soon almost forgotten, until a time came when men gladly treasured the little that was remembered of him; but his successor was better known, and Democritus has obtained much of the honour of the acute thoughts that taught us atomic motion. It is said that Xerxes himself stayed, when at Abdera, with the father of Democritus, who was very wealthy, and that the magi in the great king's train taught the theory of atoms to the young man. It may be true, for the East ever appears at the beginning of things, and the fresh minds caught up the richest crops as they showed themselves on the face of the creation, expanding before men's eyes. A few words, quoted from Lange,[1] give the position of Democritus so far as we are here concerned:—

"The difference of substances arises from the difference in the number, size, shape, and arrangement of the atoms. The atoms have no internal conditions; they act by pressure and percussion only."

These words express the early theory with great beauty and precision. We do not say here with Aristotle, "Matter cannot move itself," and so inquire further into the subject; for us it is enough to remember the physical theory. So far as is apparent, Democritus left the pure physical theory of oneness of atom, and he makes many varieties, so that at last it is said that "the soul consists of round atoms, fine and smooth like those of fire, and by their motion, which penetrates the whole body, the whole phenomena of life are produced." But although this theory leaves the oneness of atoms, it keeps closely to the idea of motion as the only one phenomenon conceivable for matter or mind, and it is without much doubt the origin of the saying that life is motion, the great thought of Heraclitus. All life consists of this constant moving, nothing is but as a movement. If we care to view the subject very strictly, we may say that the simplicity of the atom is gone even at this early stage; but if the simplicity of the atom is gone, the age of the molecule commences, a body composed of bodies not to be separated; and although the atom of Democritus is spoken of as one in quality, its diversity of size gives it a composite character, and one might even choose to find in him the germ of an opposition to the theory showing itself during the very building.[2] The peculiar soul-atoms[3] destroy even the supposed qualitative uniformity of atoms; but Leucippus is said to have had the same opinion. At best this early atom is a practical one, like that of Newton's, not an intelligible one, an idea that has still to be imagined in a reasonable manner. This difficulty produces the non-atomists, but the atomist may see it quite as clearly, and leave the reasoning for a stage, since he cannot proceed with any firmness of step. A consistent atomist of the Dalton type may be a thorough non-atomist when he breaks up in his mind the present elements.

We do not find that anything fitting our present purpose was added to the theory of atoms until the time of Lucretius, if indeed he can be said to have added anything. He, however, as the only full expositor of the doctrine among the ancients, must be viewed with attention. As a philosophic poet, Lucretius is interesting, it ought rather to be said interesting

1 F. A. Lange, *Geschichte des materialismus.* 1873.

2 Zeller, vol. i., 699; Aristotle quoted in the note. 3 P. 729.

for the time in which he lived, but he contradicts himself more than the early Greeks in his theories, and his explanations, that are so satisfactory to himself, seem to many men to explain little. Nevertheless he deserves a full share of honour.

If it is attempted to give by quotations from Lucretius a good idea of his system, we get rather into confusion. The reason probably is that he had not thought out the details clearly in his own mind, and indeed every man who has attempted consistency in this department has failed; but it may be said that his atoms are solid and eternal, with some unalterable motion, and a tendency downwards as well as to the side, easily agitated also by many forces. They are made of parts, which parts cannot exist by themselves, the atoms are partly like our molecules, but the parts seem hypothetical existences, conceived apparently in order to bridge over the distance between nothing and something, a difficulty frequently attempted by those who reason outside the atomic idea. Motion is to him everything that can be found in life and thought, which are only the clashing of atoms, light or heavy, round or smooth, made by more or fewer of the combinations of small points that do not exist by themselves. This theory allows of any shape of molecules, even hooked ones, which are spoken of as explaining combination both in Lucretius and more modern writers, and to which the name of atom is applicable only with suitable explanation.

In reality, Lucretius sometimes loses the clear meaning of the atom he admires; as he loses also and objects to the idea of centres in the universe, preferring up and down as fundamental ideas. After the first great thoughts are uttered, the rudest ideas come into play, and the carpenter and hammer seem to be set to do the whole work as was done by his predecessors, and which his successors continue to admire.

The alchemists got rid of this, but lost also the idea of atoms. They retained, however, the one fundamental matter of which they were composed; with a loss of clearness they gained in breadth. Roger Bacon, so much superior to the most, may be looked on as an ideal one. He says logically: "The elements are made of *ylé*.[1] Barley is a horse by possibility, that is occult nature, and wheat is a possible man, and man is possible wheat."—*De Arte Chymiæ.*

If, however, we come to the atom which has done most good work in modern chemistry, we are after all obliged to turn back to one of the descriptions of it by Lucretius in one of his best moments, and after it has been modified by the powerful hand of Newton, and freed from connecting obscurities. Lucretius says, book i. line 603, "Primordial bodies are solid in their simplicity, and consist of the smallest parts closely united, not combined by a union of others, but rather endowed with eternal simplicity: from them nature allows nothing to be taken away or to be diminished, reserving them as seeds for bodies."[2]

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1 Or the one original matter.

2 "Sunt igitur Solida Primordia simplicitate,
Quæ minimis stipata cohærent partibus arcte;
Non ex ullorum conventu conciliata,
Sed magis æterna pollentia Simplicitate:
Unde neque avelli quicquam, neque diminui jam
Concedit natura, reservans semina rebus."

Newton's well-known words may be quoted, since they form an era in the theory of atoms, not so much by new ideas as by distinctness, and the elimination of much confusion:—"It seems probable to me that God in the beginning framed matter in solid, massy, hard, impenetrable moveable particles, of such sizes and figures, and with such other properties, and in such proportion to space, as most conduced to the end for which He formed them; and that these primitive particles being solid, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear or break in pieces; no ordinary power being able to divide what God himself made one in the first creation. While these particles continue entire, they may compose bodies of one and the same texture in all ages; but should they wear away, or break in pieces, the nature of things depending on them would be changed. Water and earth composed of old worn particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And, therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles; compound bodies being apt to break, not in the midst of solid particles, but where those particles are laid together, and only touch on a few points."

Here, then, is the real atom of the chemist—explaining a mode of combination which Bergman, Wenzel, and Richter laboriously tried to understand, which Higgins reached but did not see with vigour—the atom which Dalton made his fertile discovery—the atom which has built up the modern science of chemistry, and from which our most precise ideas of the constitution of matter have been acquired, one which must remain probably without fundamental alteration until a new mode of analysis is found, and one which probably is in principle true even at stages considerably below that which contains our unaltered elements. This atom, however, is by no means of necessity indivisible in every sense; it is only indivisible by us, although some chemists may have different views. It may be in a strict sense a molecule, but it is much better to keep that word for a combination of atoms of known bodies rather than of hypothetical existences, a double atom of hydrogen or other element being our simplest molecule.

When we arrive at this point we pass to the next important stage, namely, the motion of gaseous molecules, if not of atoms, and the beginning of the attempt to define it precisely, The first definite ideas are by D. Bernoulli. They are explained in his Hydrodynamics,[1] which, although published in Strasburg in 1738, were previously worked at when he was Professor in St. Petersburg.

D. Bernoulli says,—"The chief peculiarities of fluids are these: 1st, they are heavy; 2nd, they expand in all directions unless they are confined; and 3rd, they allow themselves to be compressed more and more, according to

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1 Danielis Bernoulli, Joh. Fil., Med. Prof. Basil. *Hydrodynamica, sive de viribus et motibus fluidorum Commentarii* (Argentorati, 1738), p. 200. Sectio decima :—

"Fluida nunc elastica consideraturis licebit nobis talem iis affingere constitutionem, quæ cum omnibus adhuc cognitis conveniat affectionibus, ut sic ad reliquas etiam nondum satis exploratas detur aditus."

the increased force applied."[1] Speaking of a vessel of air with a weighted cover, and which he illustrates with a diagram, he says, "So the minute bodies, whilst they impinge on the cover E F, keep it up by their continually repeated strokes, and form an elastic fluid, which expands itself when the weight is removed or diminished."[2] "We shall consider the corpuscles enclosed in the hollow of the cylinder as infinite in number, and when they occupy the space E C D F we shall say that they constitute the natural air."[3]

Davy and Count Rumford entered the field when this theory of gaseous motion was forgotten, and inaugurated a new theory of heat founded on molecular activity. That heat is immaterial was no rare opinion last century, or since Lord Bacon spoke of it as *motus et nihil aliud.* However, atomic motion ceased from the time of Rumford to be a vague idea. Davy[4] spoke definitely when, without calling in the aid of forces, he supposed that in solids the particles are in a vibratory motion, the particles of the hottest bodies moving with greatest velocity and through the greatest space; that in fluids and elastic fluids, besides the vibratory motion, which must be considered greatest in the last, the particles have a motion round their own axes with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances the particles move round their own axes and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend on the velocity of the vibrations, increase of capacity on the motion being performed in greater space, etc.

This is evidently the work of Rumford and Bernoulli, with additions after passing later through an original and powerful mind.

We may take the next step to Herapath.[5] At p. 15, vol. i., he says:—

"*Theory of Gases.*—From these considerations it follows that if a number of small bodies be inclosed in any hollow body, and be continually impinging on one another, and on the sides of the inclosing body; and if the motions of the bodies be conserved by an equivalent action in the sides of the containing body, then will these small bodies compose a medium, whose elastic force will be like that of air and other gaseous bodies; for if the bodies be exceedingly small, the medium might, like any aeriform body, be compressed into a very small space; and yet if it had no other tendency than what would arise from the internal collision of its atoms, it would, if left to itself, extend to the occupation of a space of almost indefinite greatness. And its temperature remaining the same, its elasticity would also be greater when occupying a less, and less when occupying a greater space; for in a

1 "Fluidorum autem elasticorum præcipuæ affectiones in eo positæ sunt: 1°, ut sint gravia; 2°, ut se in omnes plagas explicent, nisi contineantur, et 3°, ut se continue magis magisque comprimi patiantur crescentibus potentiis compressionis: ita comparatus est aër, ad quem potissimum præsentes nostræ pertinent cogitationes."

2 "—— Sic corpuscula dum impingunt in operculum E F idemque suis sustinent impetibus continue repetitis fluidum componunt elasticum quod remoto aut diminuto pondere P sese expandit ——."

3 "Corpuscula cavitati cylindri inclusa considerabimus tanquam numero infinita, et cum spatium E C D F occupant, tunc aërem illa dicemus formare naturalem."

4 *Collected Works*, vol. iv., p. 67.

5 *Mathematical Physics, etc.*, by John Herapath. 2 vols., 8vo, 1847.

compressed state the number of atoms striking against a given portion of the containing vessel must be augmented, and the space in which the atoms have to move being less, their motions or periods must be shorter, and the number of them in a given time consequently greater; on both of which accounts the elasticity is greater the greater the compression. Besides, when other things are the same, the elastic force augments with an augmentation of temperature and diminishes with a diminution; for an increase of temperature, according to our theory, must necessarily be attended with an increase of velocity, and therefore with an increase in the number of collisions."

Joule took up the subject immediately after Herapath, and says: "Since the hypothesis of Herapath, in which it is assumed that the particles of a gas are constantly flying about in every direction with great velocity, the pressure of the gas being owing to the impact of the particles against any surface presented to them, is somewhat simpler, I shall employ it in the following remarks on the constitution of elastic fluids," etc.[1] Dr. Joule continued the subject, and introduced it into the region of experiment and observation, or, in other words, to the science of modern times.[2] Joule might have added that the view he adopted is not only in accordance with the known laws of the elasticity of gases, but conforms to the ratio of the specific heats of gases. The mathematical development continued to make progress in the hands of Clausius, Clerk Maxwell, Holtzmann, and others, taking us to new fields, and outside the region of Graham's activity.

Graham is as strict an atomist as perhaps can be found. We have seen how difficult it has been to hold the doctrine pure, how even Democritus looked on the atoms as containing parts, and to get rid of the difficulty, Lucretius makes the parts incapable of existence *par se*,—thus introducing the inconceivable; even, with Newton, and decidedly with Dalton, hitherto the greatest of atomists, because his system has had the most substantial results, the atoms are either compounded of matter, besides many properties which for want of a better name we might call metaphysical, or are surrounded with forces which act very independently, and attach themselves to the atoms doing apparently most of the work.

It was a true movement in the direction of atomism, when Davy gave his ideas; and one may almost say that it was the object of Graham's life to find what the movement of an atom was. Davy does not define his vibratory motion, which could scarcely be imagined in an unconfined space unless the body had parts; for fluids he gives a motion of rotation making thereby a definite boundary; but we do not see in his opinions what an atom, say of iron, would do when alone. He would probably in such a case give it the same revolving motion as he gives the parts of gas. Graham avoids picturing the most primitive motion in all its character, but he seems to indicate one of revolution as he brings in the similarity to the orbit of a planet, and in this way with Davy connects in an interesting manner the very first speculations on the eternal motion of the heavens with those movements the smallest conceivable of atoms.

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1 *Memoirs of Lit. and Phil. Soc. of Manchester*, vol. iv. 1851, p. 111. Read in 1848.
2 See *Math. Physics*, vol. i. p. 264.

He has, however, advanced further, when he adopts the theory of one kind of matter, each atom being distinguished by the extent of its motion, one primordial impulse for each kind. This movement is an unalterable one, unaffected even by heat. These atoms do not come to us free, but are believed to be congregate, forming the compound atom or molecule, whose movements have of late been more carefully studied, and which form, in fact, the elements as the chemist finds them. And then, in his cautious manner, he adds that they have a singular relation connected with equality of volume. Equal volumes can coalesce and form a new atomic group, retaining the whole or the half, or some simple proportion, of the original movement and consequent volume. This is chemical combination.

It is therefore clear that Graham was a true descendent of the early Greeks, his mind altered, of course, by intervening thinkers; but in one point he differs from most, if not all, known to us, in that he devoted his whole life to the study of this subject.

In all his work we find him steadily thinking on the ultimate composition of bodies; he searches after it in following the molecules of gases when diffusing; these he watches as they flow into a vacuum or into other gases, and observes carefully as they pass through tubes, noting the effect of weight and of composition upon them in transpiration. He follows them as they enter into liquids and pass out, and as they are absorbed or dissolved by colloid bodies, such as caoutchouc, he attentively inquires if they are absorbed by metals in a similar manner, and finds the remotest analogies, which, by their boldness, compel one to stop reading and to think if they be really possible. He follows gases at last into metallic combination, and the lightest of them all he makes into a compound with one of the heavier metals, chasing it finally through various lurking-places until he brings it into an alloy and the form of a medal, and puts upon it the stamp of the Mint. Indeed, he is scarcely satisfied even with this, and he finds in bodies from stellar spaces—in meteoric iron—this same metallic hydrogenium, which he draws out from its long prison in the form of a gas.

It was a wonderful, and one may say romantic, ending to a series of inquiries, to the eyes of most men the most uninteresting. His works are full of care, but not of joy. He slowly collected figure after figure, never idle with his theories, but never venturing without a reason.

If we examine his work on Salts and on Solutions we have a similar train of thought. One might have slighted the importance which he attached to the water of salts and the temperature at which it was reduced, but in his hands it was a revelation of some of the most mysterious internal phenomena of these bodies. Although the inquiry was made without reference to the new position which hydrogen takes in acids, it was entirely on a track of preliminary necessary thought; the modern view he received favourably at an early period in his history of chemistry. Other advanced ideas were stimulated by him in the 1st vol. of the publications of the Cavendish Society—a Society established mainly by his influence.

A chemist must take great pleasure in following Graham when he seeks the laws of the diffusion of liquids, and traces their connections, especially when they lead to such results as he has expressed by dialysis, a process founded on a new classification of substances, and promising still the most valuable truths. We see in the inquiry how carefully Graham thought on the internal constitution of bodies, by examining the motion of the parts, and from the most unpromising and hopeless masses under the chemist's hands—amorphous precipitates of alumina or of albumen—brought out analogies which connected them with the most interesting phenomena of organic life. Never has a less brilliant-looking series of experiments been made by a chemist, whilst few have been so brilliant in their results or promise more to the inquirer who follows into the wide region opened.

It is easy to see that, however various the subject on the surface of his writings, Graham's inquiries all led to one end, namely the condition and motion of the molecule or atom. In using the word *atom* chemists seem to think that they bind themselves to a theory of indivisibility. This is a mistake. The word atom means *that which is not divided*, as easily as it may mean *that which cannot be divided*, and indeed the former is the preferable meaning. Even when Lucretius speaks of primordial bodies that cannot be divided, he does not deny that they have parts, although these, as we have seen, cannot exist by themselves, and Graham, as well as other atomists, give a similar opinion, that is, that the original atom may be far down. Graham speaks of this in more than one place. There comes to us a something indivisible by us, and it is consistent to call it an atom, as it is consistent to call the smallest particle of alum, with its twenty-four equivalents of water, an atom, simply because it is the smallest possible portion of alum, which to divide would be to destroy.

Some have preferred to leave the atom without believing in infinite divisibility, and it is strange that when we come to the point of deciding on the existence of atoms the mind insists on going further and finding of what the atom is composed. By doing this the atomist often ends by being a non-atomist, and probably this is the necessary double conclusion: we believe in atoms because Nature seems to use them, and we break them up continuously because we know not when to stop. There are various methods of spanning the distance from nothing to something. Boscovich, by his centre of force, forms an atom, but it differs in nowise from Newton's but in name. Newton and Dalton, and all others, would allow all the forces that Boscovich can desire. Sir W. Thomson has made a vortex atom in the perfect fluid described by Helmholtz, and some such fluid is much required for the explanation of many phenomena.

In 1868 Graham acknowledged the possibility of the vortex atom when he said, "We may imagine the same result from atoms and from a fluid medium to a position of which a special rate of pulsation is imparted, enlivening that portion of matter into an individual existence and constituting it a distinct substance or element."

Davy's revolving atom has been more fully considered by Czirnianski, but our real success is confined to the fuller development of the idea of motion in gases and in solutions.

We see, then, that Graham advances the idea of atomic motion, and teaches us also how to observe it. The accuracy of his method, as well as of his work, is proved by the results; and it is not without reason that he is placed in that chain of eminent thinkers which has been represented by such as Leucippus, Lucretius, Newton, Higgins, and Dalton.

P.

In Graham's lifetime chemistry had been enriched with inquiries into atomic volumes, the volumes of compound molecules, and the specific heat of the atom, or the relation of heat to the atomic weight. The relations in which the atoms of a compound stand to each other have been viewed very differently by chemists, and given rise to a certain change in the names, whilst the idea of atomicity has been developed more fully. To some of these Graham contributed his part in his inquiries on Salts, but to the mechanics and statics of the molecule, so to speak, he gave his special attention; and although new names have already arisen of men who have carried the subject in directions not followed by Graham, it is still true that his own mode of thought is peculiar, and he has left no one who can be called a successor.

His *Elements of Chemistry* form two admirable volumes, where the kernels of thought could be obtained free from shell, and where the student was led up to the newest opinions. As a text-book, however, time has removed much of its value.