XXVIII.—*Memoir of* Dr THOMAS CHARLES HOPE, *late Professor of Chemistry in the University of Edinburgh.* By THOMAS STEWART TRAILL, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh.

(Read December 6, 1847.)

It is presumed that a notice of the life and labours of ONE, who was, for more than fifty years, a most skilful and successful teacher of chemistry in the Universities of Scotland, where he was the instructor of more than 15,500 pupils; who initiated in that interesting science many who now hear me; who long filled the office of vice-president amongst us, will not be unacceptable to the Royal Society of Edinburgh.

THOMAS CHARLES HOPE was a son of Dr JOHN HOPE, the first Regius Professor of Botany in the University of Edinburgh, and of JULIANA STEVENSON, daughter of an eminent physician in that city.

Professor JOHN HOPE was a grandson of Lord RANKEILLOR, an eminent Scottish judge in the early part of the last century, and son to Mr ROBERT HOPE, a respectable surgeon in Edinburgh. Professor HOPE died in 1786, at the age of 62. His family consisted of four sons and a daughter. ROBERT, the eldest, was bred to the bar, but died in early life; MARIANNE married JAMES WALKER, Esq., of Dalry, and died in 1837, leaving an only daughter, who became the wife of Sir JOHN WALL; JOHN, a Major in the army, who died in 1840; THOMAS CHARLES, the subject of this memoir, who was born on the 21st of July 1766, and died on the 13th of June 1844; JAMES, a writer to the Signet, who died in 1842, leaving several children.

THOMAS CHARLES, the third son of Dr JOHN HOPE, received the elements of his classical education in the High School of Edinburgh, to which he was sent in 1772; but in 1778 he was removed to a school at Dumfries, and was, 1779, entered as a student of general literature in this University, at the early age of thirteen; a practice still too common in this country. There he pursued the usual curriculum of general study, before he began to apply to medicine.

As was natural, he had devoted much attention to Botany, and, under his able father, had made such proficiency, that on the death of the latter in 1786, he aspired to the Botanical Chair; and, though supported by the influence of Sir JOSEPH BANKS, Sir GEORGE BAKER, and even by the favour of Royalty, the all-powerful influence of Mr DUNDAS prevailed, and he was unsuccessful. In June 1787, he obtained the degree of Doctor in Medicine at our University. Dr IRVINE, who held the Lectureship of Chemistry in the University of Glasgow, having died in the following month, Dr HOPE was appointed to fill the vacant chair, on

the 10th of October of the same year; and thus a new field was opened to his ambition.

It was no easy task with which young HOPE had to grapple. The Glasgow Lectureship had been successively held by men of consummate abilities, and high chemical acquirements. The immediate predecessors of Dr IRVINE, had been Dr ROBISON, Dr BLACK, and Dr CULLEN; men whose names will ever stand conspicuous in the science of Scotland. Instead of acting as a discouragement, this consideration only stimulated Dr HOPE to make every effort to prove himself no unworthy successor of these eminent teachers of chemical science. With the general doctrines of chemistry he was well acquainted; he possessed ingenuity in devising illustrative experiments, and a rare delicacy in chemical manipulation. Yet, as he has confessed to the writer of this memoir, from the shortness of the period for preparation, the scantiness of his apparatus, and the utter want of assistance in his laboratory, he regarded his first course of chemistry as very imperfect. But the novelty of his mode of teaching, and the neatness of his experiments, seem to have won the approbation of his auditory.

He was, at that period, a strenuous supporter of the then generally received doctrines of STAHL—that inflammable bodies owed that quality to the presence of a principle which was termed *phlogiston*, and of course taught that doctrine in this his first course of lectures. But his conversion to the Lavoiserian or French theory of chemistry was at hand.

It is generally known, that from 1777, LAVOISIER had doubted the existence of such a principle as *phlogiston*, and in 1785 proposed the antiphlogistic theory, supported by such facts and decisive experiments, that his views were speedily adopted by his own countrymen; though for a considerable time afterwards, they were not received in Britain.

The late Sir JAMES HALL happened to pass the winter of 1787 in Paris, and was much in the society of LAVOISIER, who showed great anxiety to make a convert of Sir JAMES, and, through him, to spread his doctrines among British chemists. For this purpose he not only gave Sir JAMES free access to his papers, but exhibited to him several very important experiments, even before they had been communicated to the Academy of Sciences, or made known to the chemical world in general. Sir JAMES HALL returned to Scotland in the autumn of 1787, well versed in the new doctrines, of which he became an able and zealous propagator. He had many long discussions on this subject with Dr HOPE, who was then a keen supporter of the phlogistic hypothesis, but was soon convinced by the arguments and facts communicated by his friend; and next winter he taught them to his class, the first occasion on which the Lavoiserian doctrines were introduced in a public course of lectures in Great Britain.

In the beginning of 1783 Dr HOPE was admitted as a Fellow of this Society; and soon after he resolved to pass the summer vacation in Paris.

In his way to the French capital, he made a short stay in London, where he was very kindly received by Sir JOSEPH BANKES; and he had the high gratification of being introduced to CAVENDISH, BLAGDEN, HERSCHEL, and several other English philosophers.

At Paris he experienced marked attention from LAVOISIER and BERTHOLLET; which he ascribed partly to his having been a pupil of Dr BLACK; but was principally, I believe, owing to his having been the first chemist who had publicly taught the new French doctrines in Great Britain.

Dr HOPE considered this an important era in his life, as introducing him to men whose names were then becoming celebrated over Europe for their skill in a science to which he was ardently devoted. The amiable manners and great abilities of LAVOISIER made a deep and lasting impression on the Scottish professor; and few persons more sincerely deplored the sad fate of that accomplished man, from whom he had received the most flattering attentions.

During his connection with the University of Glasgow, Dr HOPE enumerated as his colleagues and his friends, Dr THOMAS REID, the celebrated Professor of Moral Philosophy; the eminent Mr JOHN MILLER, Professor of Law; and Mr GEORGE JARDINE, Professor of Logic.

Dr HOPE, for some years, entertained the wish to join the practice of medicine with his chemical labours; and in 1789, sought and obtained the appointment of assistant Professor of Medicine, and successor to his uncle Dr STEVENSON in the University of Glasgow. For two years he taught the Theory and Practice of Medicine, at the same time with Chemistry. On the death of his uncle, in 1791, Dr HOPE became the sole Professor of Practical Medicine, and then resigned the office of Lecturer on Chemistry; but he continued his private researches in his favourite study, the first result of which was his masterly paper "*On a new mineral from Strontian.*"

This was communicated to the Royal Society of Edinburgh on the 4th Nov. 1793; and it proved, what indeed had been previously conjectured by others, that this mineral contained a new earth, differing decidedly in its qualities from Barytes, to which it bears the greatest affinity. To this earth Dr HOPE gave the name of *Strontites*, from the place at which it had then only been found. From the appearance of this mineral, which had been, I believe, first noticed, about six years before, by Dr WALKER, Professor of Natural History in Edinburgh, it was generally supposed to be a variety of heavy-spar, or perhaps might contain a new ingredient. Yet KIRWAN, in the Second Edition of his Mineralogy, published late in 1794, takes no notice of Strontites; except we may consider such, his statement, "that he had heard of the discovery of *Barolite*, or aerated Barytes in Argyleshire;" and SCHMEISSER, whose "Mineralogy" appeared in London two years after the reading of Dr HOPE's paper, disingenuously passing over his experiments, states, "by analysis I found it yielded 68 of Strontian earth, 30 of

carbonic acid, 1 of calcareous earth, and a little of phosphate of iron and manganese, which probably gives it colour." This would lead the unsuspicious reader to infer that SCHMEISSER had been the discoverer of the new earth, which is certainly not the case; but this is only one of the many plagiarisms of this writer. The only chemist who has the slightest claim to the merit of an original detecter of Strontian earth, besides Dr HOPE, is M. KLAPROTH; who, in the *Chemische Annalen* for 1793-94, compared Strontianite with Witherite. In his first paper, KLAPROTH conjectured that the two minerals differed in composition, because the salts of Strontian colour the flame of combustibles red, while those of Barytes do not; and this conclusion was afterwards confirmed by some experiments of SULZER and BLUMENBACH. Neither KLAPROTH nor HOPE seem to have been aware of what the other had discovered, and both may therefore be considered as original discoverers, but the first full investigation of the subject is undoubtedly due to Dr HOPE.

The success of these investigations, and the popularity of Dr HOPE's Chemical Lectures at Glasgow, suggested to the celebrated Dr BLACK, then in declining health, the idea of having his promising pupil Dr HOPE associated with him, as his assistant and successsor in the Chemical Chair. He accordingly made the proposal to Dr HOPE in 1795, obtained the concurrence of the Patrons, and on the 4th of November of that year, the latter body chose Dr HOPE in that capacity. In that session but a few of the lectures were delivered by Dr HOPE: But in the session of 1796-97, after Dr BLACK had concluded his admirable Lectures on Heat (as I find from M.S. notes of a friend who attended that course), the venerable Professor introduced Dr HOPE to the class in the following terms:— "After having, for between 30 and 40 years, believed and taught the chemical doctrines of STAHL, I have become a convert to the new views of chemical action; I subscribe to almost all M. LAVOISIER's doctrines; and scruple not to teach them. But they will be fully explained to you by my colleague and friend Dr HOPE, who has had the advantage of hearing them from the mouth of their ingenious author." Accordingly, Dr HOPE delivered a considerable portion of that wintercourse to a large audience; and in the summer of the year 1797, he also gave a three months' course of Chemistry.

The eminent men who were at that time the ornaments of our University, were Professors MONRO *secundus*, BLACK, GREGORY, ROBISON, DUGALD STEWART, and PLAYFAIR—and HOPE always remembered with much satisfaction his earlier intercourse with Principal ROBERTSON, ADAM SMITH, and especially with HUTTON the geologist—a constellation of names that shed a lustre on the society of Edinburgh at hat period.

It would seem that the subject of our memoir still intended to conjoin the practice of medicine with his academical duties. For this purpose, he became a Fellow of the Royal College of Physicians in November 1796; and, until some

time after the beginning of this century, regularly took his share of the duties of Clinical Professor of Medicine.

Dr BLACK, who was always of a most delicate constitution, did not feel himself able to lecture after the session of 1796–97; but the life of this truly great philosopher and most accomplished teacher, was protracted to the 14th of November 1799. On his death Dr HOPE became the sole Professor of Chemistry in our University.

It was in the Session of 1798–99, that the writer of this memoir first became Dr HOPE's pupil; and he remembers, with gratitude, that it was from the clear and able prelections, and most happy experimental illustrations of the leading principles of the science by Professor HOPE, he imbibed that predilection for chemical pursuits, which long formed his chief relaxation from the severer duties of his professional life; and which, he hopes, will continue to afford interest and amusement to his declining years.

I may here remark, that Dr HOPE had become, from the variety and excellence of his illustrations, and dexterity in chemical manipulations, the most popular teacher of the science that had ever appeared in Great Britain. Not only was his lecture-room crowded with medical students from every part of the British dominions, but numerous foreigners resorted to Edinburgh, and became his pupils. Many of our nobility at that time were among his students. During one of the winters that I attended his class, among my fellow-students were the late Earl of LAUDERDALE, the present Earl (then Lord MAITLAND), Lord SEMPILL, and the late Lord ASHBURTON. The large class-room was filled to overflowing; and he who was not there before the commencement of the lecture had no chance of a seat.

The rage for chemistry continued for several years; and certainly no chemist ever had larger audiences than Dr HOPE. I find that the average number of chemical pupils here, during the six years preceding Dr HOPE's appointment as Dr BLACK's assistant and successor, was 225. When I attended him in the end of the last century and beginning of this, his annual pupils were above 400; in 1813 they had risen to 500, and in 1827 they had actually amounted to 575.

While HOPE lectured at Glasgow, the total number of his pupils amounted to about 300. After his removal to Edinburgh, his chemical lectures were attended by 15,500 persons, and the number of tickets issued for his chemical class was no less than 16,800.

His reputation as a lecturer induced a number of the Faculty of Advocates to request him to give a summer course of chemistry in 1800; which was also attended by many gentlemen engaged in other pursuits.

I shall now offer some remarks on the original investigations in which Dr HOPE at different times engaged, after his paper on Strontites.

It is well known that BOYLE, MARIOTTE, and other philosophers, ascertained

experimentally, that the diminution of bulk in atmospheric air is always proportional to the compressing force; or its volume is inversely as the pressure which it sustains; and philosophers had generally, from analogy, inferred the same of other gases.

I find, from some notes of Dr HOPE, that in 1803 he instituted a series of experiments to ascertain "whether the principal permanently elastic fluids, viz., oxygen, nitrogen, hydrogen, and carbonic acid, observe the same law of compressibility from pressure which air does."

In these experiments the compression was obtained by means of a column of mercury in a siphon tube, in the same manner as in the experiments of BOYLE, and of later experimentalists. The result was, that they all follow the same law of compression.

On the 9th of January 1804, Dr HOPE read a memoir to the Royal Society of Edinburgh, "*On the contraction of water by heat, at low temperatures*," which appeared in the *5th volume of the Transactions, in* 1805, *p.* 379.

The Florentine Academicians had published, in 1667, the singular fact, that water expands as it cools towards its freezing point; and in 1683, the same was stated to the Royal Society of London by Dr CROUNE, the Gresham lecturer. His experiments shewed that water, when cooling, begins to expand as its temperature sinks, from several degrees above the freezing point, until it begins to congeal. Several subsequent writers endeavoured to confirm these observations, but differed as to the point at which water attains its maximum density; some contending for the 40° or 41° of Fahrenheit; others for the 42° or 43°. All those experiments were made in tubes with large bulbs at one extremity, resembling in form the glass of a thermometer, but on a larger scale.

On the reading of CROUNE's paper, it was contended by Dr HOOKE, one of the most acute but most disputaceous philosophers of his age, that this expansion was apparent, not real; arising from the sudden contraction of the material of the bulb, on the application of cold. This opinion has since been maintained by several very eminent men; among whom we may mention DALTON, whose experiments on this subject are most ingenious, and who, in a private letter, drew Dr HOPE's attention to this curious phenomenon. It occurred to Dr HOPE, that this point might be decided by experiments, in which a change in the capacity of the containing vessels could have no influence on the result.

He took a cylindrical glass vessel, $8\frac{1}{2}$ inches deep and $4\frac{1}{2}$ inches wide, which was filled with water at the freezing point, 32°. Two delicate thermometers were suspended in the axis of the jar, so that the bulb of one was half an inch below the top of the liquid, and that of the other as far from its bottom. This apparatus was placed in a room at a temperature 60°, and the progressive temperature of the water was carefully noted, as indicated by both thermometers. The result was, that up to 38°, the lower thermometer was invariably one degree higher than the

upper; a proof that, as its temperature rose from 32° to 38°, the water had become more dense. On reversing this experiment, by placing water at 53° in a medium cooled to 32°, he found, while the temperature of the water descended to 40°, that the water at the bottom was always the coldest, and that this difference between upper and lower thermometers was sometimes as much as 7° or 8°; but that in cooling from 40° to the freezing point, the thermometer at the bottom remained higher than that near the surface of the liquid.

The experiments of Dr HOPE, which were varied in different modes, led him to fix the point of greatest density of water at the temperature of 39°·5 Fahrenheit.

These well-devised though simple experiments are perfectly conclusive on the question of the greatest density of water being several degrees *above* its freezing point; and Mr DALTON, the most able advocate of the opposite doctrine, afterwards admitted the general correctness of the observation, though he considered that the greatest density was not at so high a point as Dr HOPE supposed. There are, however, many facts which would lead us to infer, that the greatest density of water cannot be far from the point assigned by HOPE—as, for instance, the remarkable uniformity of temperature in deep alpine lakes, which is about 40°, according to the observations of PICTET and others.

From a long note attached to this paper of Dr HOPE* we also learn, that at an early period he had experimentally proved the fallacy of Count RUMFORD'S assertion, that liquids were absolute non-conductors of heat. This philosopher had alleged, that when heat was applied to the upper surface of a fluid, the heat could only affect a thermometer placed *below* the surface of the liquid, by transmission downwards through the medium of the sides of the containing vessel; because, according to him, the particles of fluids communicate none of the caloric they receive to the contiguous particles (as takes place in solids), and that when heat is applied below, they become heated only by currents set in motion by the diminished gravity of the heated particles.

In these experiments, Dr HOPE employed a wide glass jar to contain the liquid to be the subject of trial, and applied heat to the surface of the liquid in a vessel 11 inches in diameter. The bulb of a delicate thermometer was placed half an inch below the surface of the liquid; and all conduction by the sides of the vessel was prevented, by keeping it immersed in water equally cold as high as the surface of the liquid within the vessel. Notwithstanding these precautions, the thermometer, in several experiments, slowly rose. The liquids subjected to such trials were water, olive-oil, and mercury.

Other experiments were conducted in a different manner. Equal portions of liquids, such as alcohol, were rapidly mixed together at different temperatures; and the mixture immediately indicated a mean temperature—which HOPE

* *Trans. R. Soc. Edin.* V., p. 394.

contended could not have happened, if liquids had been absolute non-conductors of caloric.

These experiments seem sufficiently conclusive; but Count RUMFORD still insisted, that the rise of the thermometer was only owing to the conduction by the sides of the containing vessel, in HOPE'S experiments, as well as in the analogous investigations of THOMSON, NICHOLSON, and DALTON.

This objection suggested to the late Dr JOHN MURRAY the ingenious idea of employing a hollow cylinder of ice as the containing vessel; which, as its temperature could not rise above 32°, could not conduct or communicate any heat to the thermometer. Water could not be employed in this apparatus, on account of its anomaly in expanding by cold near its freezing point; but olive-oil, cooled to 32°, was used; and in experiments made by suspending the heating cause in contact with the surface of the oil, the thermometer rose, in a longer or shorter interval, in proportion to the greater or less depth of the instrument below the surface of the oil.—*(Nicholson's Journal, 8vo series, I.* 425.*)*

In considering these experiments and the objections stated, it occurred to me, that if the *same* apparatus were employed with different fluids, did the rise of the thermometer depend on the conduction of the sides of the vessel, that rise should be nearly equal, whichever liquid was employed. I tried this with ten different liquids; and though the apparatus was the same, and the distance between the source of heat and the thermometer similar, yet the time required to raise the thermometer to the same point, was very different with the different liquids: this I ascribed to the difference in the conducting power of each liquid.—*(Nicholson's Journal, XII.* 137, *for* 1805.*)*

All these investigations confirmed the view taken by HOPE, that though liquids were very slow conductors of caloric, they could not be considered, as was alleged by RUMFORD, absolute non-conductors.

Dr HOPE'S reputation as a teacher of chemistry, arising from the causes already noticed, and his tact in exciting in his hearers his own enthusiasm for the study, long continued to attract vast crowds of pupils. His honours kept pace with his reputation.

In 1810 he was elected a Fellow of the Royal Society of London; in 1815 he was chosen President of the Royal College of Physicians of Edinburgh, an office which he continued to fill for four successive years; in 1820 he was admitted an honorary member of the Royal Irish Academy; and in 1823 he became one of the Vice-Presidents of this Society, an office which he held until his death. During his connection with the College of Physicians, he took an active part in the preparation of the ninth and tenth editions of their Pharmacopœia, especially in that published in 1817. For several years, besides his duties as a Professor of Chemistry, Dr HOPE gave an annual course of Clinical Medicine in this University, which was also numerously attended. But for many years before his death, he

resigned to his younger brethren the duties of the Infirmary, and of Clinical instruction.

Dr HOPE is the author of a decided improvement on the Eudiometer of SCHEELE, which, by permitting the convenient agitation of the included air with the liquid that absorbs the oxygen, expedites and simplifies that process; and is described in most elementary works on Chemistry.—(*See Nicholson's Journal, Vol. VI.*)

The establishment of Mechanics' Institutions, or Schools of Art for the instruction of the humbler classes, gave to that rank of society means of acquiring information beyond that usually obtained by many of the wealthier classes; and, in the opinion of some, diminished the respect of mechanics for individuals less knowing than themselves. The system of courses of popular lectures on scientific subjects for both sexes, which had prevailed for many years in various parts of England, was comparatively little practised in Scotland, when Dr HOPE delivered, in the spring of 1826, a short course of chemical lectures to Ladies and Gentlemen. His vast lecture-room was crowded with what he described to me as a "most brilliant audience;" and his example was soon followed by more than one of his colleagues in the University, and by several of the eminent men who then taught different branches of natural science in their private establishments; undoubtedly with no small benefit to the rising generation, and the more general diffusion, among all ranks, of interesting subjects of contemplation, and of conversation. Even admitting that the knowledge thus diffused is not deep, it has imparted to social intercourse, a vigour and variety that contrasts favourably with the former insipidities and frivolities of fashionable society.

Dr HOPE had always endeavoured to impress his pupils with the importance of Practical Chemistry, and introduced into the University classes for the cultivation of that branch of study; but, from increasing years, and love of ease, this department he soon almost wholly abandoned to his assistants.

In 1828, to encourage the study of chemistry among the students in the University, particularly in the practical department, he instituted a chemical prize; and, for this purpose, presented to the Senatus Academicus a sum of £800, as a fund, the interest of which should, annually or triennially, be given as a prize to the author of the best essay on a given chemical subject, illustrated by experiment. It should be observed, that money thus liberally bestowed, was the sum which Dr HOPE had received for his popular lectures on chemistry, which he appears from the beginning to have destined for this purpose.

For many years Dr HOPE appears to have abandoned the pursuit of original research, with which he had so auspiciously commenced his chemical career, and to have confined his efforts to the improvement of his lectures, and the devising of striking experimental illustrations.

I find no original paper of his, from the publication of his investigations on

the conducting power of fluids, until the 18th January 1836, when he read, to the Royal Society of Edinburgh, the first part of a paper entitled, "*Observations and Experiments on the coloured and colourable matters in leaves and flowers of plants, upon which acids and alkalies act in producing red, yellow, or green colours.*" A second part of this paper was laid before the Society on the 21st of the following March.

Although chemists have at all times used coloured vegetable infusions for indicating the presence of acids and alkalies, no researches appeared to have been made on the peculiar vegetable principle on which the acid and alkali acted; and it was generally taken for granted that both descriptions of agents acted on one and the same principle. Dr HOPE endeavoured to shew, by various experiments on the general colouring matter of plants, that vegetable infusions, which became red by the addition of an acid, and green or yellow by an alkali, contained two distinct principles, on one of which acids acted, and alkalies on the other. To the former he proposed the name of *Erythrogene*, and for the latter that of *Xanthogene*. DECANDOLLE had distinguished the colouring matter of flowers by the name of *Chromule*; and ELLIS speaks of the substance which may become green, red, or yellow, under different circumstances, as the *colourable matter* of plants. The object of Dr HOPE's researches was to prove, that this matter was not an individual substance, but consisted of two distinct vegetable principles, which exist either separate or combined in different plants. He illustrated this by many experiments on different sorts of plants, and gave the results in eight tables. He shewed that all green leaves, all white and yellow flowers, contain only one of these principles, viz., *Xanthogene*, that all red and blue flowers, also all leaves with red colours, contain both Xanthogene and Erythrogene (with the single exception of Litmus, which contains no Xanthogene), and that red flowers abound in Erythrogene. The distinct nature of these proximate principles of vegetables he inferred from the different modes in which they are affected by chemical re-agents.

In the same year Dr HOPE made a communication to the Society "*On the Chemical Nomenclature of Inorganic Compounds.*" He pointed out the disadvantages of the want of a discriminating and uniform nomenclature among teachers and writers on chemistry; and stated certain changes which he had for some time employed in his lectures.

The changes proposed were—

1. To discard the prefixes *proto*, *per*, *super*, *sub*, for compounds.

2. To adopt rigidly the happy suggestion of Dr THOMSON, viz., to employ the Greek numerals to denote the number of atoms or equivalents of the base of a compound, and the Latin numerals for the number of atoms of the oxygene or acid.

3. To avoid as much as possible the intermixture of Greek and Latin in numerical indications.

He added examples thus—

1	atom of base to	1	of oxygene,	oxide of base.
1	...	2	...	bis oxide.
1	...	3	...	ter oxide.
2	atoms of base to	1	of oxygene,	dis oxide.
3	...	1	...	tris oxide.
2	...	3	...	dis-ter oxide.

and so forth.

The general adoption of some such nomenclature, he conceived, would give a desirable accuracy to chemical language.

In a conversation with Dr HOPE in the early part of 1837, I noticed the discordant opinions held by various philosophers on the maximum density of sea-water, and asked whether he had applied to this fluid the same beautiful and simple investigations by which he had ascertained the point of greatest density in fresh water. He replied in the negative. I strongly recommended the subject to his notice; because, as it appeared to me, several geologists and hydrographers had deduced erroneous explanations of certain phenomena in the ocean from this undecided point. I added, that I should long ago have attempted to solve it, had I not considered that it would have been an interference with a subject he had already so ably discussed. He thanked me for the hint, and the consequence was, the completion of the series of experiments, which he communicated on the 2d of April 1838, to the Society, in an "*Inquiry whether sea-water has its maximum density at some degrees above its congealing point, after the manner of fresh water.*" Most philosophers seem to have assumed, that sea-water followed the same law in cooling as fresh water; and its greatest density was generally considered to be at temperature $36\frac{1}{2}^\circ$ F.

Dr HOPE first tried the effect of cooling sea-water from 40° in vessels shaped like large thermometers, and found that it continued to shrink, by a diminution of temperature, like other bodies. He afterwards employed the same apparatus with which he had examined the peculiarity in fresh water; and he found, that in cooling from 40° to its freezing point, the coldest water was invariably at the bottom of the vessel. Therefore, the striking anomaly which so remarkably distinguishes the cooling of *fresh water*, does not take place in *sea-water*. The importance of this conclusion will be manifest to those who have examined the theories of oceanic currents, and the remarkable fact, that the existence of banks or shoals in the ocean is marked by a fall in the temperature of the superincumbent water.

Dr HOPE reserved the examination of the precise point of the maximum density of sea-water for a future communication—which was never made.

In 1843, the Society had two communications from Dr HOPE. The first was—"*Observations on the Flowers of the Camellia Japonica, Magnolia Grandiflora, and Chrysanthemum Leucanthemum.*" This paper was read on two evenings,

the first on the 23d of January, the last on the 3d of April. The author, from the action of different re-agents on infusions of these flowers, established the existence in each of a distinct proximate principle, which, however, he had been unable to exhibit in a separate state; to these he gave the name of *Camelline*, *Magnoline*, and *Chrysanthemine*. He shewed, also, that notwithstanding the fine white of the petals of Camellia Japonica, they contained much iron.

The second paper, his last communication, was read on the 1st of May 1843, the very last time that Dr HOPE was ever at the meetings of our Society. It is styled "*An Attempt to explain the Phenomena of the Freezing Cavern at Orenburg.*"

This cavern is described by Sir RODERICK MURCHISON, as one of several occurring in a low hill of Gypsum. In winter, the air of this cavern feels warm to those who enter it; but in summer an intensely cold air issues from it. This has been explained by Sir JOHN HERSCHEL, as being produced by the long time the *waves* of heat and of cold take to penetrate to the interior of the cavern—each requiring six months to penetrate to that depth; just as SAUSSURE found, that it required, at Geneva, six months for the heat of summer, or the cold of winter, to penetrate to the depth of $29\frac{1}{2}$ feet. While admitting this general explanation, Dr HOPE considered that it would require something more to explain the forcible issue of such cold air during the summer months; and he makes an ingenious conjecture, on the part performed by the air cooled in the fissures, described as existing in the inmost recesses of the cavern, in producing that phenomenon.

The subject is very interesting though obscure; but I may observe that such streams of cold air are not peculiar to the Orenburg cave. Streams of air, cooled from 15° to 34° below the external air in the shade, are known to issue from the crevices of the small artificial hill at Rome, named *Monte Testaccio;* from the limestone grottos of *Cesi*, in the Roman states, so well described by SAUSSURE, in *Journal de Physique* for 1776; from the caves in the sandstone hill, on which is perched the miniature republic of *San Marino;* from the *Cantines* in the potstone rock near Chiavenna; from the caverns of *Caprino*, on the Lake of Lugano; and from the calcareous caves of *Hergisweil*, at the base of Mont Pilate, nearly opposite to Lucerne. What is still more extraordinary, such cold caves exist in countries the seats of not yet extinguished volcanic fire. Sir WILLIAM HAMILTON describes the cold winds issuing from the cave of *Ottajano*, at the base of Vesuvius; and in the Isle of Ischia, the air which issues from the *Ventarola* of *Funera* is as cold as 43° F., when a thermometer in the shade, without the cavern, is at 58°—*(See Saussure, Voyages dans les Alpes, III.* 1405.*)*

Such are the chief contributions of Dr HOPE to physical science.

It has been alleged that they are fewer and less important than we had reason to expect, from the long period during which he filled the Chemical Chair,

his acknowledged skill in experiment, and the brilliant path then opening for important discoveries in chemistry, which have immortalized the contemporary names of BLACK, PRIESTLEY, DAVY, WOLLASTON, DALTON, and FARADAY among ourselves—of LAVOISIER, BERTHOLLET, VAUQUELIN, GAY LUSSAC, VOLTA, KLAPROTH, BERZELIUS, and LIEBIG on the Continent. That there is foundation for this criticism, I will not attempt to deny: and, indeed, Dr HOPE seems to have anticipated it, by some observations he once made verbally to myself, and has stated in a paper now in my possession, as his apology. "Those," says he, "who devote themselves to the science of chemistry, may be divided into two classes—1*st*, Those whose labours are employed in original researches, to extend our knowledge of the facts and principles of the science. 2*dly*, Of those whose business it is, from university or other appointments, to collect the knowledge of all that has been discovered, or is going forward in the science, to digest and arrange that knowledge into lectures, to contrive appropriate and illustrative experiments, and devise suitable apparatus for the purpose of communicating a knowledge of chemistry to the rising generation, or others who may desire to obtain it. From my professional situation, I consider myself, as Dr BLACK had done before me, as belonging to the second class of chemists. I consider my vocation to be the teaching the science."

It is true that it is the paramount duty of one appointed to teach a science to make that his principal object; but this, I humbly conceive, is quite consistent with most extensive original research. It may be that the regular recurrence of the labour of teaching the elements of a science, requiring several hours of daily personal exertion, may sometimes indispose a lecturer to experimental investigations of a similar kind; but such has not been its effects on DAVY, THOMSON, BERZELIUS, or LIEBIG; all of whom have combined the business of teachers of chemistry with the most valuable and laborious original researches. Dr BLACK had certainly made all his great discoveries before he was Professor in the University of Edinburgh; but his health was always very delicate, and his example can scarcely be pleaded for one who enjoyed such uninterrupted and vigorous health, that he never was a single day prevented from lecturing by indisposition, for a period of more than fifty years.

Dr HOPE undoubtedly fulfilled admirably the duty of a public teacher of chemistry, as we have already stated. His mode of lecturing was methodical and clear, though his style was occasionally too laboured; he scarcely ever failed in the performance of the nicest and most difficult experiments, which he introduced to an extent previously never attempted in chemical prelections; and he possessed the faculty of impressing his hearers with just notions of the importance and interest of the science. Still it is to be regretted, that one so well qualified to advance the boundaries of the study, had limited his ambition and his exertions

almost so exclusively to the business of methodizing and detailing the discoveries of others.

We may here remark, that besides the eminent philosophers already mentioned as his friends, HOPE was on terms of very friendly intercourse with WATT, DALTON, WOLLASTON, and DAVY. His acquaintance with the latter began in 1799, ere that illustrious man had yet risen to celebrity. In passing through Bristol, HOPE visited the Pneumatic Institution of Dr BEDDOES, and was much struck with the originality and inventive genius of young DAVY. Soon afterwards, a lecturer of talent was wanted to fill the Chemical Chair in the Royal Institution established in London, under the management of Count RUMFORD. Dr HOPE was consulted; he strongly recommended DAVY to the notice of the Count; and in 1801, the young chemist was established in the Royal Institution. This anecdote, which I have extracted from the original correspondence, once in my hands, is honourable to the discernment of HOPE, who thus early recognised that energetic genius, which was destined to win the proudest laurels in the career of physical discovery.

Among Dr HOPE'S most intimate friends in Scotland, were Dr HUTTON, the geologist, and Sir JAMES HALL. From the intercourse with these eminent men, he had early imbibed their geological tenets; and for many years he was the only public teacher of science in this country, who inculcatedthe doctrines of the *Plutonic* theory of the earth. During the many years of my studies in this University, HOPE regularly gave several interesting lectures on geology in his chemical course, and was a strenuous assertor of the truth of the Huttonian theory, which he continued annually to teach in many subsequent years; while the rival Wernerian doctrines were most ably, and no less strenuously maintained, by my friends, Professor JAMESON, and the late most eminent and eloquent lecturer Dr JOHN MURRAY. At that time the chemical history of mineral bodies formed no inconsiderable part of a course of chemistry; and it was in introducing the mineral kingdom to the notice of his pupils, that Dr HOPE exhibited many of the proofs of the igneous formation of stony bodies; which was also illustrated by a well-selected series of rocks, chiefly collected by himself in different excursions in the Highlands and Western Isles, and in various other parts of the United Kingdom.

For many years Dr HOPE enjoyed uncommon health, and continued to discharge the duties of the Chemical Chair with his usual success, until within a year of his death.

A few years before that event, he complained to me of inability to read by candlelight, and of suffering severe pain in his eyes on making the attempt. On examining his eyes, I discovered on each cornea those minute depressions like the marks of the point of a pin, which have been described by some authors as abrasion,

or commencing ulceration of the cornea. The daily use of a weak solution of nitrate of silver gradually removed the disease; but, after some months, it recurred in a less violent degree, and again yielded to the same remedy.

In 1838, on completing the fiftieth year of his career as a Professor of Chemistry, Dr HOPE was invited to a public dinner by a numerous body of his former pupils. The meeting was attended by many philosophers from a distance, as well as by a great number of the inhabitants of Edinburgh. It was on this gratifying occasion that he stated, among other causes of thankfulness, that he never had been for a single day, either as a student or as a teacher, detained from the duties of his class.

Dr HOPE had continued his lectures in the University until the conclusion of the winter session in 1843. It was observed, that his voice was feeble, and although his experiments were, as usual, neatly performed and successful, that he had lost something of his wonted energy. Increasing debility induced him, in the autumn of that year, however, to resign his Professorship, rather unexpectedly, a short time before the commencement of the session of 1843–44; so that the Patrons had not sufficient time to deliberate on the choice of a successor in this important Chair. In the mean time, it was very necessary for the interest of the University, that a course of chemistry should be there delivered as usual. I was then in England; but, at the earnest request of the Senatus Academicus and the Patrons, after some hesitation, I undertook the duty, and taught the chemical class during the session of 1843–44. I know that Dr HOPE also was gratified by my undertaking the task. He not only freely gave me the use of his manuscript lectures, which were fairly and fully written out, and of his whole apparatus, but relinquished, in my favour, that portion of the emoluments of the class which had been secured to him as an annual retiring allowance, by the terms of his resignation.

It is but justice to Dr HOPE to state, that I found his lectures far more nearly written up to the advanced state of chemistry at that period, than I had been led to expect; and although it was necessary to make various alterations and additions, especially in the disquisitions on organic chemistry, these alterations and additions were less extensive than I had anticipated. Whether he had employed the interval between his last course and mine in improving his manuscript, I cannot tell; but the fact I have mentioned ought to be recorded. During that winter I had much intercourse with Dr HOPE. He was pleased to express a strong interest in my exertions, and said, that he had frequently enquired from others how I carried on the duties of the chemical class.

In the end of 1843 and beginning of 1844, he seemed rather more vigorous than in the preceding autumn; but as the spring advanced, his strength began very visibly to fail, and he spoke of his gradual decay with firmness and resignation.

During the month of May, he was much in bed; yet even then he took an interest in general conversation, and warmly congratulated me on the termination of my chemical labours.

A few days before his death I saw him for the last time, and although apparently not in suffering, he took leave of me as if we should meet no more.

He quietly expired on the 13th of June 1844, in the 78th year of his age.

Dr HOPE was never married. An excellent portrait of him, by the late Sir HENRY RAEBURN, which has been engraved, is in the possession of his family; and a fine bust of him by our eminent artist STEELL was presented to the University.