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# RECORDS



# GENERAL SCIENCE,

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BY

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PHILOSOPHY

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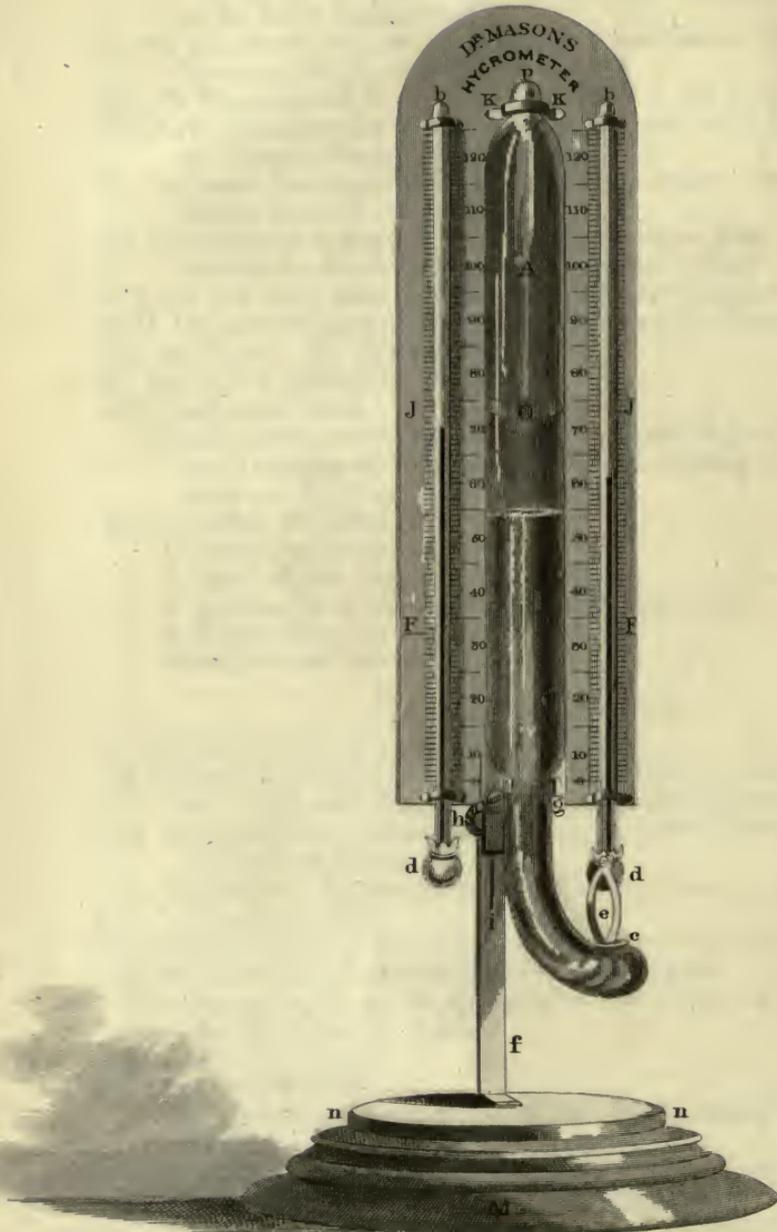
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# RECORDS

OF

## GENERAL SCIENCE.

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### ARTICLE I.

#### *Biographical Account of Sir Humphry Davy, Bart.\**

THE actions of those who have stood well in the eyes of their fellow-countrymen form always subject for pleasant consideration, but when these actions have been in the highest degree brilliant, when they have conferred benefits upon society which we have no measures for precisely calculating,—and have opened new channels for the improvement of society, the claims upon our attention are completely irresistible. Such a demand, is but an act of justice to that great philosopher Davy, whose fame is not of the ephemeral nature of those who like some flowers of other lands shoot up, bedecked with imposing beauty, and rapidly disappear—the tenants of an hour—no more to spread their buds to the refreshing dew drop.

Humphry Davy, the son of Robert Davy, a Builder, and Grace Millett, was born on the 17th of December, 1778, in Market Jew Street, Penzance. The origin of his family, he appears to have been anxious to ascertain, but could not succeed in tracing it back beyond 200 years, during which period, according to the tombstone in the parish churchyard of Ludgvan, many of his forefathers occupied the respect-

\* This Memoir has been drawn up from the materials afforded by the able and delightful "*Memoirs of the Life of Sir Humphry Davy, Bart.*," by his brother John Davy, M. D., &c." It is impossible to refrain from admiring the affectionate spirit which has dictated these memoirs. But it is a subject of question, whether it is consonant with that strict impartiality with which every public man ought to be, and will ultimately be dealt with.

able position in society of yeomen. His father, who was a person of some capacity and ingenuity, died in 1794, and left his family consisting of five children to the care of his widow, then in her 34th year. She lived to the advanced age of 76. She possessed a good understanding, benevolent disposition, and a pious mind. When she became a widow her income was about £150 a year, and it was incumbered with a debt of about £1300, contracted by her husband in consequence of losing speculations in mining. Her prudence, economy, and admirable exertions speedily, however, relieved her from her distressing circumstances. To aid in extricating herself she engaged in partnership as a milliner with a young French lady, who had fled from France during the revolution. The business was only carried on for three or four years, when she was left some property which increased her income to £300 a year. Under the fostering care of such a parent, it would have indeed been remarkable, if the subject of our memoir had not begun to shew early proofs of developement of genius, and to have imbibed some of her excellent disposition.

The first school he was sent to, was kept by a Mr. Bushell, a teacher of reading and writing, and who, observing the rapid progress of his pupil, recommended him to be sent to the grammar school, when only six years of age. The teacher of the establishment (Mr. Coryton) was a careless, indiscriminating tyrant, like too many of his class at that period. In accordance with his character, he punished slight offences very severely, thus holding out encouragement to his pupils to commit grave offences, and quenching the acuteness of the moral principle. He was continually torturing his poor dependants by pulling their ears. Davy suffered often in this way. On one occasion, the pupil delicately reproached the master. The master observing him with a large plaster on his ear, asked him the nature of the disease, he replied, with proper gravity, that "he had put the plaster on to prevent mortification."

At the grammar school he was not distinguished for his scholarship, although his appearance was respectable. He shone more in the facility with which he wrote Latin and English verse, and in writing valentines and love letters. He shewed an early disposition for tales and imaginative

descriptions, and for angling, an art which is so intimately allied to the wild scenery of nature. He quitted school in December, 1793, at the age of 15, and returned from Truro to Penzance; here he took up his abode with Mr. John Tomkin, Surgeon, who had defrayed his expenses during his twelve months, residence at Truro. In the subsequent year his time was occupied according as inclination dictated, in fishing, shooting, swimming, and in solitary rambles.

His father dying in 1794, produced an alteration in his employment, and in 1795, he was apprenticed to Mr. Bingham Borlase, a surgeon and apothecary, in Penzance. At this period, he began to register carefully inferences from the information acquired during his reading, and some extracts from his note books, exhibit the nature of the studies which interested him most. Extracts given in his brother's life of him, exhibit his idea of a plan of study. In consonance with the results of his after life, we find that he places mathematics at the end of the course; a plan which has since been advocated by others. To say the least of it, however, it is one which strikes at the root of conciseness and precision, for nothing can contribute more to these desirable, nay, indispensable objects in science than a substantial mathematical ground-work. The absence of this is obvious in many parts of Davy's scientific writings, and renders his meaning sometimes very ambiguous. It would be an easy task to point out faults, from a similar defect in the writings of all those who have not been reared in the mathematical school. He did not begin to study the mathematics till 1796, when in his 18th year. But his more favourite study was metaphysics occasionally intermixed with poetical sallies. In 1797, he engaged in natural philosophy, and principally as with his other studies under his own tuition.

In November or December, when just entering upon his 19th year, he turned his attention to chemistry. This was an important period of his life. Like most of his great predecessors his apparatus was of the most simple nature. His bed-room constituted his laboratory; some phials, wine glasses, a few tea-cups, tobacco-pipes and earthen crucibles, his instruments. That this was the line in which he was destined afterwards to shine was soon apparent; for in the

April following, or in the short space of four months, he entered upon a correspondence with Dr. Beddoes, relative to his researches on heat and light, and a new hypothesis of their nature, to which the doctor became a convert. His results were published in 1799, in his "Essays on Heat and Light." His rapid advancement was promoted by several fortunate circumstances. In the winter of 1797, Mr. Gregory Watt, fresh from the University of Glasgow, visited Penzance, and boarded with Davy's mother. Davy speedily became acquainted with him, and the acquaintance soon ripened into a close and disinterested friendship. They explored the coast together, visited the most remarkable mines and investigated the natural history of the district. These were no small advantages.

The acquaintance of Davy with Mr. Davies Gilbert was also of great importance to him, although the benefits resulting from it have been more insisted on by some authorities than Dr. Davy considers the facts of the case warrant. The manner in which they became acquainted was according to the authority of Mr. John, of Penzance, as follows: Davy having requested Mr. John to witness some chemical experiment, he remarked that he did not understand these things, but that his friend Mr. Davies Giddy (now Davies Gilbert) did. An introduction was the consequence.

During his medical studies his progress must have been considerable, for in the fourth year from their commencement he was considered competent by Dr. Beddoes, to take charge of the patients belonging to the Pneumatic Institution. On the 2nd of October, 1798, he left his home to accept the office of superintendent of the Clifton Institution. This was the second grand period in his life. It was the outset on his splendid career. He had made as much progress it would appear as circumstances would admit of: "I have now made all the experiments I can make here," he observes, in one of his note books. It was in 1799, that he first appeared in the character of a scientific author; when his *Essays on Heat and Light* were published, in a miscellaneous volume edited by Dr. Beddoes, termed "Contributions to Medical and Physical Knowledge, principally from the North of England." They deserve little attention, except as being his first productions, for they consist prin-

cipally of inferences hastily deduced from a few isolated facts. Davy himself often regretted that they had ever been published, as he fancied they had left a bad impression. Strange it is, however, that although the theories contained in them are but the rambling speculations of a youthful imaginative mind, some of them were actually adopted by the grave Dr. Beddoes. In 1800, he wrote his "Researches, chemical and philosophical, chiefly concerning nitrous oxide and its respiration." The discovery that nitrous oxide produces the remarkable physiological effects of exciting symptoms of inebriety, produced a great sensation, not only in Bristol, but all over the world. Fortunately, the fame of Davy does not rest upon such a deservedly fleeting and frail basis. The value of the discovery was exaggerated beyond all bounds; according to some authorities the very nature of man was prospectively altered; he was to breathe a new atmosphere. But when excitement has attained a high pitch the succeeding stage is low and depressed. The discovery is now almost forgot. The action of the gas in the manner described by Davy, is actually considered by some as problematical. A remarkable instance of the ambiguous nature of its effects was elicited by a distinguished chemist, who administered a dose of nitrous oxide to an Irish gentleman, who exhibited all the fantastic manipulations described by Davy. The effect, therefore, appeared decisive; but unfortunately the philosopher, who was the subject of the experiment, having requested, after recovering from his fit of gladness, that another dose should be administered, a cargo of common air was served up instead of the oxide; the consequences were even more violent than in the previous experiment. It is obvious, therefore, that the imagination plays no inconsiderable part in this species of simulated mania, or at least, that it is only on persons of lively imaginations that such effects can be produced.

Notwithstanding his labours in the Pneumatic Institution, Davy continued to devote a considerable portion of his time to essay writing upon miscellaneous subjects, and to sonnet and poem writing. The fragments of some of these which have been published, shew, that had his mind been more closely devoted to the study of poetical refinements, he

would have figured as a poet. But more useful work was in store for him. In February, 1801, he went to London, having been appointed assistant lecturer at the Royal Institution, a fine establishment, founded by an American of considerable celebrity, Count Rumford. He delivered his first lecture in the course of six weeks after, and was favourably received. It has been asserted, that the simplicity of Davy after this time was converted into flippancy and neglect of friendships. However much this assertion may have been overstrained, it is generally understood that his demeanour underwent a change, and that his manners were not sufficiently of that open, frank, and unostentatious kind which are so becoming in the true philosopher.

Davy's success was remarkable, and it would have required uncommon care to have seasoned his mind against the tempting powers which were mustered around him. It has been stated, that he now "assumed the garb and airs of a man of fashion," that "the bloom of his simplicity was dulled by the breath of adulation." Dr. Davy excited by those affectionate feelings which are so delightfully apparent in every page of his life of his brother, has denied that there is any truth in these calumnies, as he appears to consider them. He has, in consequence, brought forward proofs from a number of individuals to whom he wrote friendly letters, as he considers them, that Davy was not what he has been represented to be in this respect. But, if such letters prove any thing, they only shew that towards those individuals he entertained the feelings expressed in his letters. They do not prove that he did not exhibit *hauteur* to other individuals, or that his notions and manners were not too aristocratical for a man of science. His success as a lecturer, however, was at once complete. He was received by crowded audiences, and the Institution became a fashionable resort.

During the greater part of the day he was engaged with the business of the laboratory, where the powers of his expanding mind were applied to original research. Yet his time was not systematically occupied. When in town, he generally entered the laboratory after breakfast, about ten or eleven o'clock, and if uninterrupted remained there till three or four. Instead of returning to the laboratory after

dinner, he was much in the habit of attending evening parties and devoting much of his time to amusement. When his duties did not keep him in town, he often made short visits to friends residing in the neighbourhood of London, or he went to some good trouting stream, and enjoyed the country and his favourite exercise of fishing.

After the conclusion of the session of 1801, he visited his friends at Bristol and Penzance. In 1804, he visited Scotland and the western islands. In the summer of 1805, he made a journey to the North of Ireland for the purpose of examining the giant's causeway, and passed through the greater part of the island, indulging, when favourable opportunities occurred, in his favourite exercise of angling. In the mountain district of Donegal he "met with a singular race of beings, the most gifted with vague curiosity of any men I have seen. They asked questions, without considering whether they were civil or uncivil, and seemed little daunted by reproof. 'Where do you come from?' 'Ramelton.' 'Do you belong there?' 'No.' 'What place do you belong?' 'London.' 'Is it war or peace?' 'War.' 'Have the English lost any men?' 'There has been no battle lately.' 'When was the last?' 'Lord Nelson's. Did you never hear of him?' 'No.' 'What is your name?' 'It is a name you have never heard and never will hear of.'"

They all agreed that there were old men who knew the history of the Finns and Finn Macoul in Gaelic, but no one could shew me the abode of these sages. His sentiments in reference to the political state of that lovely, but unfortunate portion of our empire, is worthy of note: "All are slaves, without the probability of becoming free; they are in the state of equality which the *Sans culottes* wished for in France; and until emulation and riches, and the love of clothes and neat houses are introduced among them, there will be no permanent improvement; changes in political institutions can at first do little towards serving them. It must be by improving their habits, by diffusing manufactures, by destroying *middle men*, by dividing farms and by promoting industry." In the note book which he carried with him, he inserted observations on the geology of Ireland. His power and deep observation were ever active, and he allowed no opportunity to escape of benefiting science,

even amid the bleakness and wildness of the least favoured of nature's scenes.

For some years, he appears to have made no original discoveries, or at least, his laboratory labours were not attended with any striking results. Electricity constituted at the commencement of the century a fruitful field for investigation. The pile of Volta had opened the way to many curious discoveries; but want of care led to some strange deductions; the pile was actually supposed to generate muriatic acid and alkali in water; because these substances were obviously present when water was made to complete the circuit. Our author turned his attention to this curious question. "It was in the beginning of 1806," says he, "that I attempted the solution of the question, and after some months' labour, I presented to the society the dissertation, to which I have referred in the beginning of the lecture. Finding that acid and alkaline substances, even when existing in the most solid combinations, or in the smallest proportions in the hardest bodies, were elicited by voltaic electricity, I established that they were the results of decomposition, and not of composition or generation. I drew the conclusion, that the *combinations and decompositions by electricity were referable to the law of electrical attractions and repulsions*, and advanced the hypothesis that *chemical and electrical attraction were produced by the same cause, acting in one case on particles, in the other on masses, and that the same property under different modifications was the cause of all phenomena exhibited by different voltaic combinations.*"

The paper which developed the deductions from his experiments was in reality a noble one. It "constitutes," says Dr. Thomson, "one of the most important contributions ever made to scientific chemistry, and threw a ray of light upon chemical affinity which may ultimately produce the most important consequences." To this paper the French Institute awarded the prize founded by Napoleon for the most important discoveries in galvanism. The researches described in this memoir were quite original, and were carried on without the most distant connexion with any experiments conducted by others. The claims, therefore, of some continental chemists to supersede him are quite futile. The observation of Dr. Davy is perfectly correct. The

Bakerian lecture of 1806, owed nothing to the labours of these gentlemen, and it would have been in every respect as complete had they never been published; but had this lecture been suppressed a vast accession of knowledge would have been lost to the world."

But a short interval elapsed after his installation at the Royal Institution, till, at the request of the managers, he turned his attention to tanning. He set about the investigation with proper spirit, probed the subject to the bottom by visiting the tanyards, eliciting information from the workmen, and where their evidence was deficient in reference to the grand object of improving science, he endeavoured to fill up the hiatus by the results of original research.

The consequences of his labours were published in his "Account of some Experiments and observations on the constituent parts of certain Astringent vegetables, and on the operation in tanning," in the Philosophical Transactions for 1803. About the same time, he entered upon the study of agricultural chemistry, and gained so much information that he was requested in 1802, to deliver a course of lectures to the board of agriculture "On the connexion of Chemistry with Vegetable Physiology." His lectures, which were afterwards regularly delivered before the board for ten years, were published under the title of "Elements of Agricultural Chemistry." Several important points were made known in these lectures, and the method pointed out by which agriculture, instead of being a mere random miscellany, might become a true science. His application of scientific views to practice is every where obvious in the pages of the work, but is particularly so under the head of manures; "the great object in the application of manure should be to make it afford as much soluble matter as possible to the roots of the plant, and that in a slow and gradual manner, so that it may be entirely consumed in forming its sap and organized parts, he connects this with the chemical principles of his theory; viz., that the fermentation of manures necessary for the solution of these soluble parts should be a regulated process, and as it is connected with decomposition it should be stopped as soon as the end for which it was instituted is attained." It is not meant to be maintained that his views were infallible, but that they

were plausible and formed important contributions to agriculture as a science was at once obvious. He conceived, that lime was injurious in the form of quick-lime, and that it proved beneficial by accelerating the decomposition and promoting the solution of any hard vegetable matter in the soil, contrary though it be to the views of many, who consider that it acts as an instrument of saturation to the free ulmic acid generated in the vegetable portion of the soil. The beneficial action of gypsum on vegetables he attributes to its entering into the composition of vegetables. This opinion as stated exactly by Davy has not been corroborated by Peschier\* who found that the action of this salt upon vegetables was in direct proportion to its state of solution, and that its influence is null except when it is dissolved. His experiments lead to the conclusion, that the substance is decomposed by electrical powers possessed by the assimilating organs of the plant, and that the sulphuric acid is set at liberty and combines with the potash in the sap.

Besides his study of agriculture, he became interested with the investigations of meteorology, and in one of the numbers of the Institution Journal, he described his *eudiometer* affording a simple method for ascertaining the proportion of oxygen and azote in the atmosphere. It consisted merely of a graduated glass tube and a solution of muriate or sulphate of iron at the minimum of oxidation, saturated with nitrous gas. He published about this time, a paper on the collision of steel and hard bodies, bearing on the question of light, in the Institution Journal. In the Philosophical Transactions for 1805, he published an analysis of Wavelite, and another on the uses of boracic acid. But his most important contribution to analytical chemistry, was contained in his Bakerian lecture for 1806. "On some chemical agencies of electricity." At the conclusion of this paper he makes some interesting observations on the nature of electric chemical action in the mineral kingdom, which have since been confirmed by the researches of Becquerel. He attributes the alteration in many of the rocky strata to the influence of electrical agency.

We come now to the year 1807 the most important in his

\* Records of General Science, iii. 477.

brilliant career. In conformity with his views of overpowering chemical attraction by electrical power, he instituted a series of experiments on the vegetable alkali. He began the investigation in September, and on the 19th of November, he delivered his second Bakerian lecture to the Royal Society. It was entitled "On some new phenomena of chemical changes produced by electricity; particularly the decomposition of the fixed alkalies and the exhibition of the new substances which constitute their basis, and on the general nature of alkaline bodies." Many vague notions had been formed of the nature of the vegetable alkali. Some Italian and French chemists considered it a compound of lime and hydrogen. Others supposed that it contained azote. Davy thought it might consist of phosphorus, or sulphur united to azote; for, as ammonia was regarded as a compound of hydrogen and azote, he conceived, that phosphorus and sulphur, much denser bodies, might produce denser alkaline matter. In his first trials, he used strong aqueous solutions of potash. "Dry potash," says he, in one of his lectures, "is a non-conductor; I then employed *fused potash*, and in this instance, inflammable matter was developed; then a piece of potash moistened, and to my great surprise, I found metallic potash formed. October 6th. This matter instantly burned when it *touchèd water*, and swam on its surface re-producing potash. In dry oxygen gas likewise, it burnt into perfectly dry potash. Soda was decomposed in the same manner. The earth had been suspected by the elder chemists, particularly by Boyle, Becher, and Stahl, to be capable of conversion into metallic substances though they had vainly sought for modes of effecting this important desideratum. When I had discovered in so unexpected a manner that potash and soda are *metallic oxides*, all the former analogies became strengthened to a degree that the question of the nature of the earths was of easy solution; but though so much more like metallic oxides than the fixed alkalies, yet I found much more difficulty in effecting their decomposition." His delight on discovering these facts was excessive; Mr. Edmund Davy, who was present, states, that when our philosopher saw the globules of potassium burst through the crust of potash and take fire as they entered the atmos-

phere, he could not contain his joy; he actually danced about the room in ecstatic delight.

Thus was a new light thrown upon this branch of chemistry. Perhaps, the discovery has been too highly lauded, but there can be no question that it was a great discovery. The author, undoubtedly, had some strong analogies to conduct him in his research. It was, therefore, dissimilar to the results of Newton; the one was the consequence of the action of a powerful agent, directly applied; the other, of observation, deep reasoning, and mathematical calculation.

(To be continued.)

## ARTICLE II.

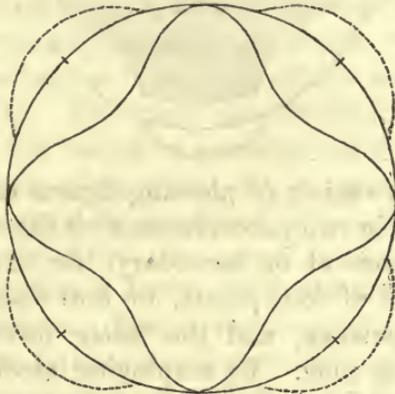
*Experiments and Observations on Visible Vibration.* By  
CHARLES TOMLINSON, ESQ.

(Continued from vol. iii. page 370.)

101. THIS splashing of the water can, however, be prevented by covering the surface with a very thin coating of lycopodium; the vibration of the glass will dispose the curved line round its interior side and a similar line will be formed on the surface of the water, the arcs in both cases corresponding with each other; and, in this way, by careful application of the bow, sets of figures can be obtained analogous to that which has already been given (81) as the fundamental figure when mercury is employed. These figures can be obtained from glasses of any shape or dimensions, and I have usually employed coloured water, sifting a light coating of lycopodium upon the surface. The following and analogous figures, however, were obtained from the conical glasses two-thirds filled with coloured water: figure 3 indicating four nodes resulting from the lowest tone. The second tone affords a figure of six curves, and from the third tone a figure of eight curves indicating eight nodes is obtained, and so on, increasing two curves in the figure for every ascending tone. The curves within the circumference represent the extent of the repulsion of the dust from the centre of vibration, which is indicated by dots placed upon the circumference, which is of course the

interior surface of the glass itself, and the dotted curves without the circumference may be considered as indicating the upward motion of the liquid waves against the sides of the vessel, caused probably by the rebound of the liquid after the first impulse. The points of collapse of the two systems of curves are the nodes where the liquid is not disturbed during vibration; thus the figure during vibration represents four, six, or eight waves, with as many corresponding troughs or hollows between every two waves.

Fig. 3.

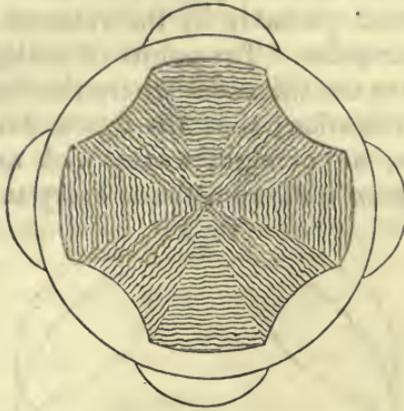


This figure may also convey an idea of the six, eight, ten, and twelve curved figures, all of which are very perfect of their kind, and indicate correctly the divisions of a sonorous vessel necessary to produce a note of a certain pitch.

102. The perfection and beauty of the figures is much increased, when, instead of a slight coating of lycopodium, a thick one is applied to the surface of the water, so that the upper surface be kept dry and free to move; thus, figure 4 represents an effect of this kind where the outer dots as before represent the vertical curves, and the surface exhibits a star of four fans, composed of the dry powder; these fans are truncated within one-sixth of an inch of the glass, while the semicircular spaces have only a thin adhesive coat. It would appear that the first application of the bow throws the dust up the glass where it remains, while the horizontal figure is the result of many oscillatory movements of the water, the distance of the truncated ends of the fans from the glass remaining constant somewhere between a quarter and one-sixth of an inch. In some cases by close inspec-

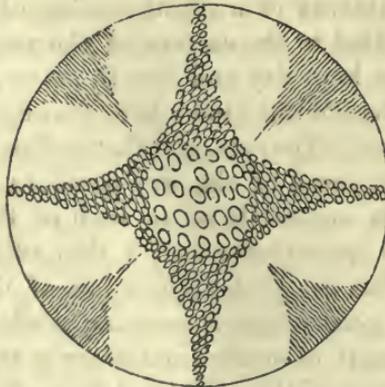
tion the imprint of minute parallel waves between the truncated ends and the glass could be seen in action.

Fig. 4.



In this way a variety of pleasing figures can be obtained, all of which are in strict accordance with the note produced, whether fundamental or secondary, the former invariably producing stars of four points, or four distinct accumulations of the powder, and the latter increasing two for every ascending tone. By employing alcohol the fundamental tone produces Chladni's first figure, formed by globules of the liquid; thus in figure 5 the globules seem to be generated simultaneously at the centre of vibration as indicated by the fans, and are thrown upon the quiescent portion of the liquid and point out the nodes.

Fig. 5.



This figure is of course only seen while the glass is being operated on by the bow. Lycopodium sinks in alcohol, but a film of minute particles remains on the surface, which

receives the impress of the figure, and from the great mobility of the liquid, minute changes are by its means detected. Analogous figures have been obtained from the secondary tones, the points of the star always increasing in number as before stated. Similar results have to a certain extent been obtained when vessels of porcelain, crockery-ware, and metal have been employed; this part of the subject will be resumed.

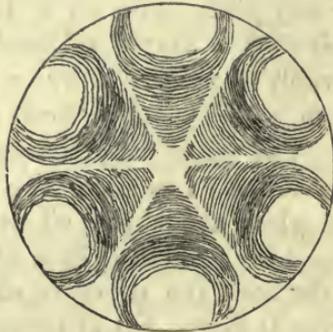
103. Another excellent method of ascertaining the number of nodes for any given note on a foot glass of any shape, is by employing a limpid oil such as linseed oil.\* This is incomparably superior to water when employed without lycopodium, as it does not vibrate and no splashing whatever results. The glass may be about half filled with the oil, care being taken in pouring the fluid to preserve the surface of the glass above the oil perfectly clean and dry. The note must be produced by a strong and rather sudden stroke of the bow, the pressure of the latter being increased in drawing it down, but at the same time the glass must not be shaken. The curved line under these circumstances will be distinctly and perfectly marked round the glass by the elevation of the waves. The fundamental note will yield a line of four undulations, while for the secondary tones the lines will consist respectively of six, eight, ten, and twelve undulations.

104. A figure can also be impressed upon the surface of mercury contained in a foot glass by means similar to those employed for water. A very slight coating of lycopodium being given to the mercurial surface, it was strongly

\* The peculiar whining tone produced by lowering the tension of a string during vibration is well known. This effect can be imitated by strongly vibrating a glass and suddenly pouring in water; the note is lowered in proportion as water is added, and the whining effect is produced by the note passing rapidly through a series of tones to that tone produced by the whole bulk of water added, and the glass still continues to vibrate for some time after the addition of the water. It is a curious fact that if oil instead of water be added to the vibrating glass the effect of that fluid is to stop vibration instantaneously, but if oil heated to about 250° be added to the vibrating glass, the note continues precisely as if water had been employed. The cohesive nature of oil will explain this, the effect of heat being to overcome the cohesion of its particles so as to enable them to glide more easily over each other, and consequently not to become attached to the surface of the glass. The effect of heat upon oils has been already shewn (23 et seq.) and this experiment is analogous.

breathed upon, and the sex-nodal division produced by the bow. In the process of vibration while the particles of dust were becoming somewhat agglutinated by the condensing breath, the motion of the mercury underneath arranged the film into a form analogous to that which the mercury had itself assumed; when vibration had ended and the mercury become quiescent, the film retained the figure which had been impressed upon it as in figure 6.

Fig. 6.



This figure assumes the appearance of having been caused by a repulsion of particles from each vibrating centre, shewing the inward bellying of the arc of the glass at each centre, and exposing the surface of the mercury by its vibratory action while at the nodes the powder is not disturbed. The point at which the bow is applied becomes a centre of vibration, and such, indeed, is always the case.

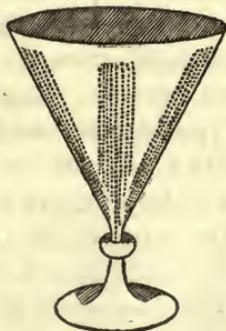
105. The nodes having thus been indicated by means of wires, mercury, water, and oil, endeavours were frequently made to ascertain their existence by means of a dry glass and lycopodium, as in the case of glass plates and other plane surfaces, and for a considerable time our patience has been tried unsuccessfully to accomplish on the surfaces of goblets that which is affected by such simple and easy means on plane surfaces.

106. It was obvious that the only favourable form of glass was the before mentioned conical vessel. Powder was lightly sifted upon the interior surface, and the glass then vibrated without the desired effect, the glass was vibrated and the powder sifted into it during vibration, but this was also unsuccessful, and after many such plans the following was resorted to with success.

107. The glass being clean and dry, and the powder also well dried, the *external* surface of the glass was covered with the powder by means of a fine sieve, the distribution being made equal all over the surface by giving the foot of the glass one or two gentle taps to get rid of accumulations in particular parts; the bow was then applied to the edge with the precaution of touching the glass at one and the same spot in all the oscillatory movements of the bow, and with an uniform pressure, so as to produce one note only; and in the first instance this note was the fundamental note E, first glass before mentioned.

108. When the glass began to vibrate, some of the heavier particles of the powder fell down from the external surface, but the lighter particles were, as we at first supposed, shaken from the surface, carried upwards, and hovering like a slight cloud over the vessel, fell into it and produced a regular quadripartite system on the inclined interior side of the vessel, the powder being collected up the nodal lines and an empty channel at the middle of each vibrating sector as in figure 7. This, however, is not the explanation of the effect as I shall presently shew.

Fig. 7.



109. The above figure was produced by the glass vibrating the note E, with the higher note B; the result was the formation of six distinct trains down the interior, each train being narrower than in the former instance, and more clearly separated from the intermediate spaces. The trains did not approach so near the apex as in the former instance.

The second glass yielded similar results; the lowest note affording four trains, and the higher note E flat eight trains,

thus in both instances the same in point of nodal arrangement as when mercury, oil, water, &c., were employed.

110. These results were obtained from a vast number of trials, all of which, except those now stated, were unsuccessful. It was, therefore, necessary to inquire in what consisted our want of success in so many trials, so as to obviate the causes of failure and be able to produce these effects at will. As our trials had been confined to the two conical glasses before mentioned, and having succeeded but twice, I supposed it possible that in consequence of the severe vibratory action to which these glasses had long been subjected they had lost, in great measure, their *nodal* properties, if I may so speak, their particles undergoing a certain change, which though producing notes by vibration, did so without a strict and spontaneous division into vibrating sectors.\* I, therefore, procured other glasses and on covering the exterior surface as before with lycopodium was gratified by the production of the trains every time I attempted their formation, and found that in proportion as the glass was new so were the trains well defined and perfect in their formation. They are produced by one bold stroke of the bow, and if not produced at once it is seldom of any avail to apply the bow a second time.

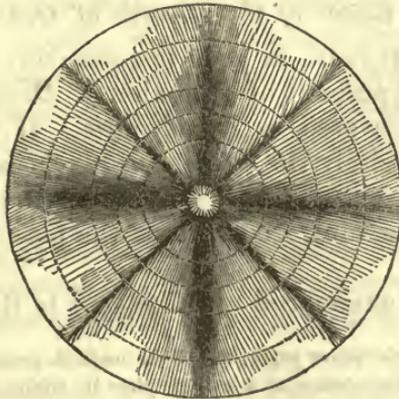
111. There was also another cause which in the former trials militated against success; the glass and powder were always kept quite dry, and after each trial the glass was well cleaned with a dry cloth and sometimes with soft leather; this produced a disturbance of electricity which interfered greatly with the success of the experiment which may be probably thus explained; the surface of the glass both exterior and interior acquired positive electricity from the cloth or leather, the latter being negative, the powder being sifted upon the exterior surface of the glass participated in electricity also in a positive state; its attempts, therefore, to enter the glass would be repelled, at least to a degree sufficient to prevent a symmetrical arrangement of figure. I, therefore, found this objection obviated by preparing the glass with powder, &c., and allowing it to remain several minutes at rest previous to the experiment.

\* It is known that the glass plates employed in Chladni's experiments by frequent use become incapable of producing the acoustical figures.

112. It has been already stated, that we were at first disposed to account for the transference of the powder from the exterior to the interior surface of the glass by considering the lighter particles of the powder to be shaken from the glass, carried upwards by the current, generated in the surrounding air, and so falling into the glass formed the trains on the nodes or quiescent portions, but in the latter and successful trials I soon found that this could not be, because the trains were formed in an instant, before even the bow was taken from the glass, and, indeed, it is easy to see how the trains are formed: as soon as the glass is vibrated the powder by a sort of centrifugal force rushes upwards over the edge, falls into the interior, where a partial vacuum is formed by the vibratory action, arranges itself first into a series of innumerable circles distinct from each other round the whole interior surface; they are then shaken off the vibrating parts of the glass, and accumulate in heaps or trains up the nodes, forming fan shaped figures for the lowest note, and lines for the upper notes, and all this is the result of a moment.

113. The fans and lines can be produced alternating in the same glass, provided the bow be so managed that during the first *half*-stroke the low note be produced, and the second half-stroke produce the high note. In such case a very pleasing figure results, which may be represented on a flat surface as in the following figure.

Fig. 8.\*



\* This figure was copied from the glass by my friend, D. C. Read, Esq., of Salisbury, whose Etchings are so well known in this country and appreciated on the continent.

It is, perhaps, unnecessary to remark that this figure is contained within an inverted cone, and the above mode of representation has been preferred as being better capable of shewing the details.

114. These figures are seen with best effect when conical vessels made of blue or green glass are employed, the contrast of the light coloured powder and the dark ground upon which the figures rest, appeal to the eye better than when white glass is employed.

115. All the experiments contained in the present paper depend greatly for their success upon the newness of the glass vessels, and the bows; the latter should have horse hair of the stoutest kind capable of great tension, and rosin should be applied sparingly; a few days work, however, renders the hair quite useless, and active vibration has similar effects on glass vessels also. An old bow and glass that has been much employed often prevent results which with new instruments are immediately obtained.\*

*Salisbury, March, 1836.*

### ARTICLE III.

*On a New Oxide similar to that of Donium.* BY HENRY S. BOASE, M. D., *Secretary of the Royal Geological Society of Cornwall.*

*To the Editor of the Records of General Science.*

SIR,—I have just read, in your last number, Mr. Richardson's interesting account of the oxide of *Donium*, which he has recently discovered in Davidsonite. If I am not mistaken, it is precisely the same substance as one which I have also found, and for which I had intended to propose the name of *Treenium*, from *Treene* the place where it was obtained.

I might have announced my discovery to the public, as I

\* Since the date of the above paper, one of the methods proposed (106) for obtaining the figures in the interior of a conical glass by sifting lycopodium into it, has been successfully adopted, the only precaution necessary to insure success, being the employment of new glasses and a good bow. By new glasses, I mean, such as have not been employed in vibratory experiments.—C. T.

did to several friends in private, in April last—but before making it known, I was desirous of completing the examination of the other ingredients of the mineral,—which, however, I have not as yet been able to accomplish to my satisfaction.

The substance under examination contains a considerable portion of alumina, or rather of a substance which is soluble in potassa and re-precipitated by muriate of ammonia, yielding crystals of alum with sulphuric acid and potassa. My attention was first called to the new substance, by observing that this alumina was unusually soluble in liquid ammonia; next the pale drab colour of the precipitated alumina, and its change from white to brown by exposure, seemed to indicate the presence of iron or manganese, which, however, I could not detect: and lastly, the fluctuating quantities of alumina obtained, led me to examine not only the liquid in which the alumina had been separated from potassa by muriate of ammonia, but also that in which the sulphate of alumina had been decomposed by excess of carbonate of ammonia, in both of which, there was a substance, which, dissolved in nitric acid and thrown down by ammonia, gave a precipitate rapidly changing from white to pale brown. I then suspected something extraordinary, and the addition of hydro-sulphuret of ammonia settled the question, by giving a dense green precipitate. Then recollecting how beautifully the crystals of alum were truncated in their angles and edges. I applied the same test, after having decomposed and re-dissolved in potassa, and with a similar result. Mr. Richardson does not allude to the fact, but my sulphuret soon changes from green to a yellowish white on exposure in a moist state, on the filter; and the alkaline solution has a bright green colour from holding some of the sulphuret in solution, which also slowly undergoes the same change with deposition of whitish flocculi. The green precipitate readily dissolves in nitric acid, with evolution of fumes of sulphuretted hydrogen: and when concentrated by a gentle heat, it has a yellowish colour.

With this solution, potassa and ammonia give white precipitates changing to yellow, the carbonates of the same

alkalies also give precipitates. The pure alkalies re-dissolve the recent precipitates, as do also the carbonates to a certain extent. Ferro-cyanate of potassa causes a slight whitish opalescence, but tincture of galls produces no change. A plate of zinc is soon covered with a slate coloured coating, which, however, does not increase, but is succeeded by drab flocculi, and if the solution be concentrated the whole is gelatinized, and in this state is not very soluble in acids. Muriate of gold does not occasion any alteration.

The oxide, or the nitrate, heated with chlorate of potassa, first becomes dark brown, and if the salt be in sufficient quantity, the residue on cooling is of a bright flesh colour. Well washed, an insoluble substance of same colour remains, which soon decomposes giving out bubbles, and returning to its brown colour; muriatic acid makes it white with evolution of chlorine, and nitric acid dissolves it, with extrication of gas. It is probably an oxide at a higher degree of oxidation than the brown.

Such is the result I had arrived at, when the oxide was laid aside for more circumstantial examination, after I had ascertained the nature of the other ingredients of the mineral, one of which, is an organic acid very perplexing in its composition. Should my oxide prove to be the same as Mr. Richardson's Donium, my name of Treenium must of course give place to his, as he first had the honour of making it public, and I trust that this brief note will insure to me, if not the honour, at least, the credit of also having discovered Donium.

The alumina of my substance has probably been derived from decomposing granite, and I am sanguine that some of the peculiarly red granites of the Land's End district, will be found to contain this oxide; and, perhaps, a re-examination of many minerals will shew that alumina has been mistaken for this substance.

By inserting this notice you will much oblige, Sir,

Your obedient servant,

HENRY S. BOASE.

*Penzance, 11th June, 1836.*

ARTICLE IV.

*Description of a New Hygrometer; illustrated by experiments and a comparison of its results with Sir John Leslie's, and the Dew-point Hygrometers, by JOHN ABRAHAM MASON, M.D., Member of the Royal College of Surgeons, Edinburgh; Extraordinary Member of the Royal Medical Society, Edinburgh, &c.*

It has long been a subject of complaint among all practical meteorologists, that we have no Hygrometer which combines the advantages of being simple in its construction, easy of application, and unerring in its results.—Persons unaccustomed to scientific pursuits are unwilling to devote their time and attention to complex instruments, and it has occurred to me that good service might be rendered to Meteorology, by devising some method of ascertaining the Hygrometrical condition of the atmosphere, which will combine simplicity with accuracy.

I am fully persuaded that moisture has a much greater share in developing the effects of climate upon the human constitution than most physicians imagine; and I believe that the following reasons have hitherto tended to impede its investigation with that precision which it merits:

1st. The uncertainty and deterioration of all Hygrometers made of Hygroscopic substances, and their only indicating relative differences.

2nd. That all the Dew-point Hygrometers occupy more time than most people are able to bestow upon them; and that some tact is required in their application.

3rd. That ether of sufficient strength cannot be obtained in every climate, and other chemical means of producing cold cannot be always at hand.

4th. That Sir J. Leslie's Hygrometer by evaporation is considered by many to offer erroneous results, being influenced by radiation, currents of air, &c., and that much calculation is required before the absolute quantity of humidity can be ascertained.

5th. That the degrees of humidity indicated by the different kinds of Hygrometers cannot be easily reduced to a common standard, so as to be compared with each other.

Now, the instrument which I propose is at once free from

all these objections, and is so simple and easily constructed, that by mere inspection the results can be ascertained with as little difficulty as attends the common thermometer.

The form of the instrument is portable; it may be readily used in every climate and under all circumstances; and its graduation depends upon no arbitrary or disputed determinations of wet and dry; it is liable to no deterioration from use, age, or accidental circumstances; it acts with an unerring fluid; and above all things, if the thermometers be carefully selected, so as to indicate the same degree under similar circumstances, it is impossible with moderate care to obtain erroneous results; so that the comparative variations of humidity, from the point of saturation, being exhibited in degrees of the thermometric scale, are at once referred to a known standard of comparison which every person can understand and appreciate.

The only trouble which attends the use of this instrument is the renewal of the silk on the moistened bulb once a month; and the addition of a fresh supply of well boiled or distilled water from time to time as it may be necessary.

#### *Description of the Instrument.*

Upon the outer margin of a stand *M* is fixed an upright rod of brass *f* supporting by a semi-circular clyp the scale *JJ*, (the clyp is seen in shadow at *o*, and is  $\frac{3}{4}$  of an inch in diameter, thus removing the scale *JJ*  $\frac{3}{4}$  of an inch from the brass support *f*.) In the middle of the scale *JJ* a space is left to receive a glass tube *A* on the principle of the bird fountain, having on each side of it two thermometers *bb* of equal range from  $0^{\circ}$  to  $120^{\circ}$  Fahrenheit, firmly attached to the scale already described. The bulbs *dd* of both thermometers are of equal size, and covered with white persian silk; round the stem of one, a thread of floss silk *e* is attached, which terminates in the cup of the fountain *c*.

This fountain is easily removed and re-fixed at pleasure by turning the screw *h* which allows the support *g* to move with ease in the groove *i* bringing the support *g* to the contracted part of the fountain.

The upper part of the fountain is kept in its place by a double clyp *kk*. The fountain is hermetically sealed at *p*;

the size of the orifice in the cup *c* which is oval, is  $\frac{2}{8}$  of an inch in length by  $\frac{1}{8}$  in breadth.

When not in use, a box or brass case covers the whole instrument, and screws on the stand *M* at *nn*. The scale is made of ivory and ought to be as slight as possible.\*

The principle which Dr. Hutton proposed I considered the best :—

1st. As by it the temperature of the shade could be ascertained at the same time along with its hygrometric condition.

2nd. Because it would indicate the relative dryness of the atmosphere ; and,

3rdly. That the Dew-point could be easily ascertained from the data afforded by the instrument ; whence the absolute quantity of aqueous vapour could also be estimated.

I was led to the construction of my instrument by the following observations in the *Encyclopædia Britannica*, which I accidentally saw in the winter of 1834, during my detention at Portsmouth, by the S. W. gales, when on my way to Madeira ; “ a more scientific instrument has been constructed on the following principle, viz. :—that as evaporation produces cold, the effect of that process on a thermometer will indicate the rate and amount of evaporation, and consequently the relative moisture of the air. The more dry the air is the greater will be the evaporation ; and the greater also the cold or the depression of the mercury in the thermometer.”—“ The general doctrine is that the dryness of the air, under all circumstances is precisely indicated by the depression of temperature produced on a humid surface which has been freely exposed to its action.”

Not possessing either Professor Daniell's, or Sir J. Leslie's Hygrometers, I was led by these remarks to the construction of the instrument now proposed, being desirous of making a minute series of observations during my residence in Madeira, for the purpose of determining the true character of its climate ; the ample details of which will be given in

\* The instrument is made in the above form by Mr. Cary, Optician, 181, Strand ; and by Mr. Squire, Operative Chemist, 277, Oxford Street, London, to pack in a neat travelling case, containing spare fountain, silk, &c., with appropriate tables for the use of the instrument ; also by Mr. William McDowall, Philosophical Instrument Maker, 13, Infirmary Street, Edinburgh. Much care is requisite in the construction of the Hygrometer to ensure accuracy ; the above named makers may be depended upon as affording accurate instruments.

a volume on the Medical Topography, climate, physical structure, and past and present condition of that island, now preparing for the press by my friend, Mr. Blewitt, and myself; being convinced that medical practitioners do not yet possess that precise knowledge of the climate of Madeira which is necessary to enable them to decide on the particular classes of disease to which it is adapted:—I was also convinced that an instrument which would at any time indicate with facility and precision the actual state of the air in regard to humidity or dryness, would be a valuable acquisition to science. My first idea was that the instrument would only serve to indicate the relative humidity of the atmosphere, or its distance from the point of saturation; but after considering all the laws of evaporation, and taking a general view of my own experiments on the subject, I have been able to find the dew-point by very simple calculation, and also to trace out the relation between this instrument and Sir J. Leslie's. Considering that we ought ever to bear in mind, that nature is constant in her operations, and that the same causes invariably produce the same effects, it hence became easy not only to obtain all the indications that I required from the instrument itself, but also to ascertain by it the results given both by the dew-point Hygrometers and by that of Sir J. Leslie, with perfect accuracy and invariable certainty; so that it may be truly regarded as supplying the place, if not of superseding altogether, the more complex instruments which have preceded it. I shall subsequently enter more fully into the consideration of the important fact, that any errors which may arise in taking the temperature of the shade, will in my Hygrometer give an excess of moisture, while those of the dew-point Hygrometers will indicate an equal excess of dryness; so that when both instruments are used, if very accurate data be required in ascertaining the absolute weight of vapour in a given quantity of atmospheric air, the mean of both instruments will insure the truth.

I will here state all the objections which have been advanced against the principle of Hygrometers by evaporation which Dr. Hutton first proposed, the chief of which have been grounded upon the belief that the air dissolved the vapour contained in it in a chemical manner, the same as

water dissolves a saline substance, consequently all the formulæ for calculating the weight of vapour in a given space have been made on this supposition, whereas if the beautiful laws developed by the illustrious Dalton be applied to this instrument, I think every objection can be fully and satisfactorily answered.

Objections which have been advanced at various times against the Hygrometer by evaporation.

1st. That evaporation is considerably increased by currents of air. Thus the instrument would be affected by the wind increasing the evaporation, and consequently depressing the temperature, which would indicate a fallacious dryness.

2nd. That evaporation takes place with greater rapidity in sun-shine than in shade, also indicating a fallacious dryness.

3rd. That the two thermometers are more or less influenced by reflected light and heat, &c.

4th. That evaporation is influenced by the density of the atmosphere; it being greater or less according to the height of the mercury in the barometer.

5th. That it is almost impossible to take the heat of the air to any degree of nicety, without the observation being affected by the power of radiation; and if a radiant caloric be allowed to interfere, the condition of calculation fails.

6th. That more time is required, than could be bestowed by an ordinary observer, in order to make a correct observation, as time must be allowed for the water to acquire the same temperature as the atmosphere.

1st. *Objection.* In answering the first objection it will be necessary to enumerate some of the laws of evaporation, which from being vaguely understood have always operated to the disadvantage of hygrometers by evaporation. I need not here enter into any minute analysis of the composition of the atmosphere, as it would be foreign to my present purpose.

The two following laws will be sufficient to elucidate the question.

The first established by Mr. Dalton, "that gases, dry atmosphere air included, act as vacua with regard to vapour,

and that where they happen to be mixed together, they exist as independent atmospheres."

The second, that the *final* tension of vapour given off in the process of evaporation is determined not by the temperature of the evaporating surface, but by the elasticity of the aqueous atmosphere already existing; which law I will endeavour to establish in my next paper.

If we take the trouble to inquire how the error has arisen with regard to this objection, I am endeavouring to remove, we shall find that evaporation has been considered to be essentially promoted by the application or presence of heat, and the agitation of the ærial medium.

Mr. Dalton when speaking of this subject observes, "that air when calm and still, merely obstructs the progress of evaporation; but when in motion it increases its effect in direct proportion to its velocity by removing the vapour as it forms." He has fixed the extremes that are likely to occur in ordinary circumstances at 120 and 189 grains per minute, from a vessel of six inches diameter, at a temperature of  $212^{\circ}$ , giving 79 grains increase per minute, for the effects of a strong wind. This law would be perfectly applicable in a dry air, but will by no means hold in one containing almost its full charge of humidity.

The true law, I conceive to be this, that evaporation takes place from a humid surface in direct proportion to the temperature and velocity of the air, diminished by the force of the vapour already existing in the atmosphere; this I shall prove by direct experiment. The agitation of the air has hitherto been conceived to perform the principle part in reducing the temperature of an evaporating surface, but I hope to prove, that it is the dryness of the air on which we shall find the effect alone to depend.

For example, if air be perfectly saturated with humidity, neither the increased temperature of the humid surface equally with that of the surrounding air, nor the strongest agitation of the medium can produce further evaporation or depression of temperature; under those circumstances, both thermometers would indicate the same degree, and show that the air was absolutely saturated with humidity; these conditions would continue until one of the two follow-

ing circumstances occurred: either the temperature of the medium must increase so as to render it capable of receiving a further supply of vapour, which would be at once shown by the depression of the mercury in the moistened bulb, or the air must be rendered less humid by a sudden depression of temperature, or increased density of the atmosphere, which would condense the vapour already existing in the medium, and cause precipitation in the form of rain or dew, when the temperature would be again raised to a certain degree by the heat given out from the water, passing from a state of vapour to its fluid condition, it would then admit of a further portion of vapour, and the mercury in the moistened bulb would descend as before.

Under either of the above conditions, were the air in motion, the rapidity of evaporation and consequent depression of temperature would be found exactly equal to the velocity of the current, diminished by the force of vapour already existing in the atmosphere.

From the above remarks it will be seen that the "*celerity* of evaporation has been mistaken for its intensity, and the coldness induced on the evaporating surface has been viewed as the accumulated effect of a rapid dissipation of moisture; whereas the fact is simply, that the quantity of particles will be carried away in proportion to the velocity of the wind: consequently a humid surface will be much more rapidly dried; but it does not follow as a consequence of the rapid dissipation of moisture that the temperature of the evaporating surface should be proportionally depressed, for in a free atmosphere, as Sir J. Leslie has proved, vaporization proceeds with unabated energy, while the corresponding depression of temperature must advance by a relaxing progression; since otherwise, the accession of an accelerated movement might push it to any extent, but the reduced temperature caused by this process under given circumstances has a certain limit beyond which it cannot pass."\*

The chief objection; therefore, against this instrument, advanced by Mr. Daniell himself, is perfectly without foundation, viz., "that the temperature of evaporation is no longer that constant quantity which it is supposed to be if dependant only upon the temperature of the air, and is

\* Leslie on Heat.

liable to *fluctuations* with every change of place and every breath of wind." In the last place, I may add, in order to prevent any misconception of my views: 1st. that the moistened bulb will cool down to a certain point dependent upon the dryness of the atmosphere, and there its temperature will remain stationary. 2nd. The rapidity of a current will hasten the term of equilibrium; but the degree of cold induced will be found still the same.

I will now endeavour to prove by direct experiment, the law which I wish to establish in answer to this first objection, viz., that evaporation takes place from a humid surface in direct proportion to the temperature and velocity of the air, diminished by the force of vapour already existing in the atmosphere; and that under given circumstances, the depression of temperature induced by evaporation must have a certain limit beyond which it cannot pass; and that this depression of temperature, does not bear the same proportion to the rapidity with which a humid surface becomes perfectly dry.

In order to prove, that, under given circumstances, the depression of temperature induced by evaporation has a certain limit beyond which it cannot pass; I placed two hygrometers of similar construction upon a table in the middle of a large room; they each indicated three degrees of dryness.

The one I subjected to the strongest current I could produce by a large pair of double bellows, which had previously acquired the temperature of the apartment, and found by repeated trials that I could only reduce the temperature 0.5 of a degree below the other. The next thing to be proved was, whether this depression bore a proportionate progressive increase by equal increments of dryness. To establish this fact, I waited for an opportunity to repeat the experiment when the hygrometer indicated six degrees of dryness. The results of several trials were, that the temperature was depressed, just 1 degree beyond which it would not pass; at 9 degrees it was depressed 1.5; after repeated trials I found this invariably the case; being convinced of the facts, I made the following table from 0 to 26 degrees, the greatest depression of temperature I had ever witnessed in a strong *Leste*, during my residence in Madeira; in

order to discover whether my inferences would be confirmed by natural phenomena.

Table showing the number of degrees which it is necessary to subtract from the depression of temperature, produced by a humid surface when exposed to a strong current of air, in order to reduce the number of degrees to what the hygrometer would indicate, under the same circumstances, provided the atmosphere was perfectly calm.

Degrees of dryness.	Excess of refrigeration produced by a strong current of air.	Degrees of dryness.	Excess of refrigeration produced by a strong current of air.	Degrees of dryness.	Excess of refrigeration produced by a strong current of air.	Degrees of dryness.	Excess of refrigeration produced by a strong current of air.
1	0·166	8	1·333	15	2·500	22	3·666
2	0·333	9	1·500	16	2·666	23	3·833
3	0·500	10	1·666	17	2·833	24	4·000
4	0·666	11	1·833	18	3·000	25	4·166
5	0·833	12	2·000	19	3·166	26	4·333
6	1·000	13	2·166	20	3·333		
7	1·166	14	2·333	21	3·500		

I shall have occasion to describe the character of the *Leste* or dry wind of Madeira in the work already referred to; and shall there give a table, the data of which will prove that this progressive increase holds good in the maximum depression, I have seen the hygrometer indicate: for instance, at 9 A. M., the hygrometer indicated 20 degrees of dryness; the remarks are "quite calm, sky clear, without clouds;" at 11 A. M., the hygrometer indicated 24, the remarks are "strong wind." Then on referring to the above table for the necessary corrections to be made for a strong wind, opposite the degree of dryness 24 will be seen 4°·000, which subtract from 24°=20 the degree of dryness indicated two hours previously, when the atmosphere was calm. Having proved that the depression of temperature has a certain and constant limit, under given circumstances, beyond which it will not pass, I may proceed to consider the second objection.

*Second objection.* That evaporation takes place with greater rapidity in sun-shine than in shade. The difference in the quantity of water converted into vapour in sun-shine and in shade, I have not yet been able to ascertain by experiment,

but I have proved, which is sufficient for our purpose, that the relative difference indicated between the dry and moistened bulb of a thermometer is equal; or in other words, that the refrigerating process is the same under given states of dryness or humidity: whether the thermometer is in the shade or exposed to the direct rays of the sun, provided the instrument be suspended in free space at some distance from the ground.

This I have sufficiently proved to be the case, and can with confidence assert that the relation between two hygrometers of the above construction is constantly equal in sunshine and in shade. I may instance two observations to elucidate the matter, one under the ordinary state of the atmosphere, the other during a *Leste* or dry wind. The hygrometer in the shade stood as follows:

Temperature of the dry bulb 75°.

Temperature of the moistened bulb 68°, 7° dryness.

Hygrometer in the sun.

Temperature of the dry bulb 83°.

Temperature of the moistened bulb 76°, 7° dryness.

The thermometer on the ground indicated 142°.

During a strong *Leste* the hygrometer in the shade stood as follows:—

Temperature of the dry bulb 86°.

Temperature of moistened bulb 66° 20.

Hygrometer in the sun.

Temperature of the dry bulb 96°.

Temperature of the moistened bulb 72 and  $96^\circ - 72^\circ = 24^\circ - 4^\circ$  correction for strong wind = 20; thermometer on the ground indicated 124°. Thus the influence of the sun affects each thermometer equally, and the temperature produced by evaporation is the same in both cases, being regulated by the elasticity of the aqueous vapour already existing in the atmosphere. Mr. Daniell makes the same remark on this subject with regard to his dew-point hygrometer. I find in looking over my observations on this subject, that the maximum point of variation in the moistened bulb for the same day at Madeira appears to be about 2°; frequently it remains stationary; but hitherto, I have not been able to trace the cause of this variation; I may, however, remark, that this is greatest at the commencement of the

rainy season; from which we might infer, that when the temperature of the dry bulb remains the same, the moistened one rising one or two degrees, rain may be expected, or the contrary. Supposing the moistened bulb sinks 2 degrees more than ordinary, during the same day, it would indicate a continuance of fair weather.

*Third objection.* That the two thermometers are more or less affected by reflected light.

By covering the two bulbs with the same substance any relative difference will be prevented, as both bulbs will be equally affected by radiant and reflected light and heat.

It appears from many experiments I have made on the subject, that radiant heat from the sun, does not influence the process of refrigeration, as the temperature of both bulbs rises equally from that cause.

But in stating my opinion, that the hygrometer now proposed is not affected by *radiant heat* from the sun, I wish it to be distinctly understood, that this is only the case when the instrument is suspended in free space, at some distance from the ground, and apart from bodies giving off radiant caloric; for when the hygrometer is subjected to the influence of *radiant caloric* given off from *heated bodies*, its indications are no more to be depended upon as furnishing accurate results than those afforded either by Sir John Leslie's or the dew-point hygrometers; the indications afforded by my hygrometer being erroneous in proportion to its proximity to the source of error.

However, I can with confidence assert, that the instrument may be used under circumstances in which it would be impossible to obtain *correct data*, either with Sir John Leslie's or the dew-point hygrometers.

All those instruments should unquestionably be used as much as possible in the *shade*, but from repeated and varied experiments, I am convinced, that the hygrometer now proposed, will be less affected by this source of error, than any other at present in the hands of the meteorologist.

*Fourth objection.* That evaporation is influenced by the density of the atmosphere, being greater or less, according to the height of the mercury in the barometer, consequently, that corrections would be required to rectify this error.

This objection I consider without foundation, being in-

compatible with the theory of Dalton, namely, "that the quantity of vapour, contained in a given space, is independent of the presence or density of any other elastic fluid with which it forms no intimate combination; or that the maximum quantity of vapour which can exist in a given space is the same, at the same temperature, as it would be, did that space contain nothing else." Nothing can be more obvious, than that steam *in vacuo* has no concern with external pressure; and it is as well known, that the maximum force or density of steam in air has nothing to do with the density of that air, being the same as in *vacuo*.

From Professor Daniell's experiments, the dew-point is not at all affected by the density or rarefaction of the air, consequently, the tension of the existing vapour is the same as in *vacuo*; and the refrigeration of the moistened bulb, being entirely regulated by that tension, no barometrical correction will be required; neither Mr. Dalton nor Professor Daniell have ever thought of making corrections for the dew-point.

The careful experiments of Le Roy, Dalton, Gay Lussac, Daniell, and many others, have completely settled this point and placed it for ever beyond a doubt. I have also verified this by experiment, in finding the dew-point by my hygrometer.

Since, then, air acts only mechanically over the process of evaporation; the only difference that atmospheric pressure can make, will be in the time required for the moistened bulb to cool down to its ultimate limits, evaporation taking place almost instantaneously in *vacuo*; while it is impeded in proportion to the density of the air, the rapidity of evaporation, according to Leslie, being inversely proportional to the height of the barometer. As the limits to the process of refrigeration are always constant in the moistened bulb while in action, a correct observation may at any time be made by mere inspection.

*Fifth objection.* That it is almost impossible to take the heat of the air to any degree of nicety without the observation being affected by the power of radiation, and if radiant caloric be allowed to interfere, the conditions of calculation fail.

This is perfectly true both with respect to this hygro-

meter and Professor Daniell's as far as accurate calculation is concerned, but as regards the degree of dryness on the thermometric scale, or distance from saturation, it is undoubtedly superior; as in professor Daniell's instrument the two bulbs are not under similar circumstances, for in the latter hygrometer the thermometer which indicates the temperature of the air must be in the shade, and guarded from all the sources of error to which it is liable, whereas, as I have proved with respect to mine, those sources of error are of less moment, precisely the same indications being given, providing radiant caloric from surrounding objects be not allowed to interfere.

*Sixth objection.* That more time is required than could be bestowed by an ordinary observer in order to make a correct observation, as time must be allowed for the water to acquire the temperature of the atmosphere.

This objection is fully obviated by the method I have adopted, namely, by keeping the ball constantly moist by means of capillary attraction; and no more time is required than would be necessary to make similar observations on two ordinary thermometers.

Having answered every objection, I will for the present stop here, and in my next paper state the view I take of the manner in which the temperature of the moistened bulb is reduced, and also the method to be adopted for the purpose of obtaining the dew-point; the mode of comparing it with the hygrometer of Sir J. Leslie, and the various uses to which it may be applied, both in meteorology and in different manufactures.

I am, Sir, your obedient servant,

JOHN A. MASON.

18, Claremont Place, Pentonville,  
December 1st, 1835.

(To be continued.)

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ARTICLE V.

*Catalogue of Plants collected at Bombay.* BY  
JOHN GRAHAM, ESQ.

We believe this to be the first attempt at communicating any information with regard to the botanical productions of this beautiful part of the western peninsula of Hindostan.

The catalogue constitutes the gleanings of a few occasional minutes snatched by our excellent friend (with whom we have spent many a pleasant hour in botanizing amid the sylvan recesses of India) from the ingrossing avocations of his official duties. He has set an example which those who possess more spare time would do well to imitate.—EDIT.

1. *Alpinia nutans*.

2. *Achyranthes aspera*. A common weed.

3. *Asclepias gigantea*. Very common throughout India. The natives apply the milky acrid juice to sores.

4. *Asclepias acida*. This is a rare plant; I found it last August (1834) on the plains to the south of Aurungabad; also in the neighbourhood of Poona.

5. *Asclepias annulare*.

6. *Asclepias formosissima*. I have only seen this species in gardens, but, I believe, it is a native of India.

7. *Asclepias odoratissima*. This too I have only seen in gardens, and very rare.

8. *Amaryllis Zeylonica*. A very beautiful plant; I do not think it is to be found within this neighbourhood.

9. *Asparagus falcatus*. Large bushes of this shrubby species are common in the Deccan; it requires support and is generally found overtopping some other shrub; it is rather a pretty plant.

10. *Aloe littoralis*. The fibres of its long leaves are extremely tough and might be used in making cord, if not cloth; however, I am not aware of its being applied to any economical uses.

11. *Anacardium occidentale*. Cashew nut; common in Salsette and on the island of Bombay, &c. The apples are seldom used, indeed they are not worth eating.

12. *Adenantha pavonia*.\*

13. *Adenantha aculeata*.

14. *Averhoa bilimbi*.

15. *Averhoa carambola*. Both species are common in gardens, and the fruit is used for making tarts. The fruit of bilimbi grows from the thick branches and often from the stem of the tree in a singular manner, like the jack fruit.

\* This elegant flower (termed the *peacock flower*) forms a prominent part of the bouquet with which the Musselmans present Europeans on Sundays.—EDIT.

The carambola is called *kurmul* by the natives, a word which signifies sour or sharp tasted.

16. *Argemone Mexicana*. A common weed, if not a native, it is, at least, completely naturalized.

17. *Alangium 6-petalum*. Grows on Elephanta.

18. *Anona squamosa*. Custard apple, very common throughout India. The fruit is used as an article of food by the natives in times of scarcity; it is produced in great abundance with the slightest care; the tree seems to grow indifferently on all soils and situations.

19. *Anona reticulata*. Bullock's heart, so named from the shape of the fruit, which is also eaten, though it is inferior to the custard apple. The flowers have a very sweet smell, something like the finest flavoured pears. This species is not nearly so common as the other. It is generally to be found planted near temples along with the other species. They call them *ram vhoool* and *ceta vhoool*, in honour of a heathen god and goddess; vhoool means flower.\*

20. *Adansonia digitata*. This tree appears to be naturalized. Several of them grow on Bombay Island, throughout the Concan and in Guzurat. I do not think any use is made of the fruit; the tree assumes a very fantastic shape, the trunk very short and rapidly tapering; it attains a great size.†

21. *Abrus precatorius*. A climber common in the hedges and jungles; when the pods open and display its red bead like fruit, it looks very pretty. The natives use the seeds for weights, and call them *Gooneh*.

22. *Artemisia Indica*.

23. *Aristolochia Indica*. This is a rare plant, with dingy looking flowers and leaves. I have found it on Malabar hill and Cross Island in the harbour. Humboldt tells us, the South Americans use the flowers of some of their gigantic species for hats.

24. *Artocarpus incisa*. Bread fruit tree. I only know one tree on the island, it grows well and produces fruit, of

\* The author states in a letter to me that "the properties of Indian plants are little known, and no dependence whatever can be placed on native names. In fact very few have any place in their nomenclature. They are *Jungle ka vhoool*, i.e. wild flowers."—EDIT.

† There is a fine specimen of this tree in Caranja Island. See Records, vol. i., 335.—EDIT.

some of which I have eaten. In times of scarcity it would be an invaluable tree, and as the soil and climate appear to suit it well, it is a pity that it has not been commonly planted. Its congener the jack fruit (*A. integrifolia*) is in common use among the natives, who call it *Plumus*, and the wood of the tree is more used than any other for making household furniture. The tree attains a large size in Malabar; I have seen a single fruit larger than the largest turnip at home. When growing on the stem of the tree it has something the appearance of a hedgehog stuck to it.

25. *Amaranthus tricolor, tristis, oleraceus*, varieties, I suspect; *bajee* is the native name, red, green, and variegated. They are extensively cultivated and eaten like spinage.

26. *Arum campanulatum*. Native name *soorun*. The root somewhat resembles a pine apple, but it is globular. It is used by the natives instead of yams; I have tasted it; it is rather coarse.

27. *Arum esculentum*. Much cultivated by the natives who make use of the tubers in their curries, &c.

28. *Arum polyphyllum*. Very common, springing up on waste land during the rains.

29. *Acalypha Indica*.

30. *Areca Catechu*. A very graceful looking tree extensively cultivated for the nuts (betel) which are chewed by the natives.

31. *Andropogon schoenanthus*. Sweet lemon grass, grown in flower pots.

32. *A. Ischaemum*.

33. *A. Nardus*.

34. *Adiantum lunulatum*. A fern covering old walls during the rains.

35. *Avidcennia tomentosa*. Very common in salt marshes. I have seen it as large as a middle sized tree; it adorns the banks of creeks and rivers, growing in the water as well as out of it.

36. *Acanthus ilicifolius*. Sea holly. Looks pretty when in flower (dark blue colour); grows common among the *Avidcennia* plants.

37. *Artabotrys odoratissimus*. I have only seen it in gardens; it is a pretty scandent evergreen plant, with very

sweet smelling but insignificant looking flowers, as all the Annonaceae have. Decandolle calls it *Unona uncinata*.

38. *Aegiceras majus* or candel. Found common in salt marshes; it has pretty dark green leaves with white flowers.

39. *Argyreia cuneata* Sprengel. A shrub with very beautiful blue bell looking flowers. When near any support it is scandent and sends out long slender branches. Roxburgh refers it to genus *Lettsomia*. I have only found it on a range of hills about 24 miles west of Poona near Wurgaum. It is grown as an ornamental shrub in the gardens at Poona, but I have never met with it here.

40. *Agave America*. I have only seen it in gardens at Seroor and Aurungabad.

41. *Agrostis linearis*. A common grass.

42. *Anthericum tuberosum*. Springs up during the rains on rocky waste land.

43. *Boerhaavia diffusa*.

44. *Boerhaavia erecta*. Found about 30 miles N. E. from Poona. Stems woody, as thick as a man's finger.

45. *Basella alba* and *rubra*. Varieties cultivated as root herbs; the leaves are thick and succulent, and afford an excellent substitute for cabbage.

46. *Bromelia ananas*. Pine apple.

47. *Bambusa arundinacea*. Common and well known Bamboo.

48. *Bryophyllum calycinum*. Growing in cocoa-nut groves; rather pretty when in flower; grown in flower pots as an ornamental plant.

49. *B. uhinia speciosa*.

50. *B. „ candida*.

51. *B. „ variegata*.

52. *B. „ parviflora*.

} Trees with pretty flowers  
} particularly the *variegata*.

53. *Bergera Konigii*. Cultivated for its leaves which the natives use in curries. The native doctors use the bark and roots as a stimulant.

54. *B. integerrima*. Found near Panwell on the main land.

55. *Bassia longifolia*. A common tree. The intoxicating spirit called mowra is distilled from the flower. Oil is also expressed from the seeds. It is a very common and useful tree. The oil obtained from the seeds is extensively used for adulterating glue.

56. *Bignonia 4-locularis*. Common in the jungles, and somewhat resembling the ash. The white flowers rising from the ends of the branches look showy at a distance, but cannot bear inspection.

57. *Bignonia spathacea*.

58. *Bignonia radicans*. I have only found these two in gardens; both have pretty flowers, particularly the latter; it is a shrub of very slow growth, and was brought from China, I believe.

(*To be continued.*)

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#### ARTICLE VI.

*Experiments on the heat or cold produced by dissolving salts in water.* BY THOMAS THOMSON, M. D., F. R. S. L. & E., &c., *Regius Professor of Chemistry in the University of Glasgow.*

1. 300 grains of crystallized carbonate of soda in powder, were thrown into 1000 grains of water of the temperature  $59^{\circ}$  in a tumbler, and the mixture was stirred till the salt was dissolved; the thermometer sunk to  $43^{\circ}$  or 16 degrees.

The water of crystallization in 300 grains of carbonate of soda is  $187\frac{1}{2}$  grains; which is  $\frac{1}{7}$  of 1300 grains, the whole of the liquid and salt included. Now, the water of crystallization becoming liquid would absorb  $140^{\circ}$  of heat. Hence the temperature ought to have sunk  $\frac{1}{7}$  of 140 or  $20^{\circ}$ . But the fall was only  $16^{\circ}$ ; the difference is owing to the quantity of heat given out by the glass tumbler, which of course would prevent the temperature from sinking so low as it otherwise would have done.

300 grains of anhydrous carbonate of soda in powder, were thrown into 1000 grains of water of the temperature  $57^{\circ}\cdot 5$ , and stirred with a thermometer till the temperature ceased to rise. The thermometer rose from  $57^{\circ}\cdot 5$  to  $79^{\circ}\cdot 5$  or  $22^{\circ}$ . In another experiment from  $61^{\circ}$  to  $82^{\circ}\cdot 5$  or  $21^{\circ}\cdot 5$ . There remained undissolved 7·7 grains of salt. The water of crystallization seems to be absorbed by this salt in the first place; hence the reason of the rise of temperature. This water amounts to  $182\frac{1}{2}$  grains or about  $\frac{1}{7}$  of the salt and water. Hence, the rise of temperature should be  $\frac{1}{7}$  of 140 or  $20^{\circ}$ . It exceeds this quantity a very little; the reason

of which may be, that the bulb of the thermometer being at the bottom of the vessel where the salt actually dissolved, probably the temperature in that spot might have been rather higher than at the surface of the liquid.

The specific gravity of anhydrous carbonate of soda is 2.640.

The specific gravity of a saturated solution of carbonate of soda at 80° is 1.2291.

It is composed of water . . . . .	1000
Anhydrous salt . . . . .	292.3
	1292.3

The mean specific gravity of such a mixture is 1.1647. But the specific gravity of the solution is 1.2291. It is, therefore, a good deal denser than the means. This will explain in part the reason why the temperature is greater than it ought to be from theory.

2. 300 grains of crystallized sulphate of soda in powder, were thrown into 1000 grains of water of the temperature 57°.5, and the liquid was stirred about with a thermometer till the whole salt was dissolved. A longer time elapsed before the sulphate dissolved than was requisite for the solution of the carbonate of soda. The thermometer sunk to 45°.5 or 12°.

300 grains of anhydrous sulphate of soda in fine powder, were thrown into 1000 grains of water of the temperature 61°.5, the mixture was stirred about with a thermometer. The temperature rose to 65°.5. or 4°. This temperature continued unaltered for nearly half an hour, showing that the salt was giving out heat during the whole of that time.

The quantity of salt dissolved was 165.8 grains. The quantity remaining solid was therefore 134.2 grains.

The specific gravity of anhydrous sulphate of soda is 2.640.

The specific gravity of a saturated solution of sulphate of soda at 61°.5 is 1.1549.

Now the mean specific gravity of a mixture of 1000 grains water of 61°.5 and 165.8 grains of anhydrous sulphate of soda is 1.0959. The solution, therefore, is a good deal denser than the mean.

3. 300 grains of crystallized sulphate of magnesia in powder were thrown into 1000 grains of water of the tem-

perature  $56^{\circ}\cdot 5$ , and stirred with a thermometer; the solution was rapid but incomplete. The thermometer sunk from  $56^{\circ}\cdot 5$  to  $51^{\circ}$  or  $5^{\circ}\frac{1}{2}$ .

4. 300 grains of crystallized proto-sulphate of iron in powder, were thrown into 1000 grains of water of the temperature  $58^{\circ}$ , and the mixture was stirred till the salt dissolved. The thermometer sunk from  $58^{\circ}$  to  $53^{\circ}\cdot 5$  or  $5^{\circ}\frac{1}{2}$ . So that the cold evolved by the solution of sulphate of magnesia and proto-sulphate of iron is sensibly the same.

The quantities of water of crystallization in 300 grains of each of these salts are as follows :

	grains.
Carbonate of soda . . . .	187·50
Sulphate of soda . . . .	166·66
Sulphate of magnesia . . .	153·65
Proto-sulphate of iron . .	135·96

Now, the ratios of these numbers to each other are very nearly as the numbers  $37\frac{1}{2}$ ,  $33\frac{1}{3}$ ,  $30\frac{3}{4}$ ,  $27\frac{1}{5}$ .

While the cold produced by the solution of each salt was  $16^{\circ}$ ,  $12^{\circ}$ ,  $5^{\circ}\frac{1}{2}$ ,  $5^{\circ}\frac{1}{2}$ .

We see that these two ratios are not the same or even analogous to each other. It is obvious from this that the mere knowledge of the water of crystallization, and the solubility of a salt, is not sufficient to enable us to foretell the degree of cold that will be induced by its solution in water. A great deal depends upon the rapidity of the solution. Hence, it happens that more cold is produced by dissolving salts in dilute acids; because by this method the rapidity of the solution is very much increased.

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#### ARTICLE VII.

*On some Astronomical Methods of Observation.* BY WILLIAM GALBRAITH, A. M., *Teacher of Mathematics, Edinburgh.*

##### I.—ON THE OBLIQUITY OF THE ECLIPTIC.

To trace the various methods of astronomical observation used by the ancients, would be a task too laborious and irksome for our present purpose. It would not, however, be uninteresting to notice a few of their processes and instruments which they most generally employed. Among the latter the *gnomon* constructed in various ways appeared to be that in which most confidence was placed.

The rudest example of the gnomon was an upright pole, placed perpendicularly to the horizontal plane by means of a plumb line, though there are instances of some of them constructed of masonry of considerable heights, but these could not properly be called *instruments*. The altitudes of the heavenly bodies were from these calculated by comparing the length of their shadows with their heights. In modern mathematical language, the height of the gnomon divided by the length of its shadow, gives the natural tangent of the altitude of the celestial body, such as the sun, whence by means of a table of natural tangents the angular measure of that altitude becomes known in some conventional measure, such as degrees. Thus let the height of the gnomon be 5 feet, and the length of its shadow 10 feet, then  $\frac{5}{10}$  or 0.5 being found in a table of natural tangents will give the angle equal to about  $26^{\circ} 30'$ , the altitude of the sun at that time.

This method was found to be inconvenient, because the length of the shadow was required to be measured each time an observation was made. It, therefore, occurred to the ancient astronomers to form an instrument of moderate dimensions on similar principles, like the artizan's square, having the horizontal side divided into equal parts as it was at first, and afterwards into the natural tangents called by the Arabians *shadows*, to the radius, and by this means the angle of elevation became known in degrees and parts of a degree by inspection, though not to any great accuracy.

This gave place in its turn to the quadrant, divided into degrees and parts of a degree by means of a radius turning round its centre, in which were placed fine pins or sight vanes. It was with such instruments as these that Eratosthenes and Ptolemy attempted the measurement of the figure and magnitude of the earth, and the determination of the obliquity of the ecliptic. Ptolemy states that the distance between the tropics in his time was found by such an instrument to be  $\frac{11}{8}$  of the whole circumference, that is  $\frac{11}{8}$  of  $360^{\circ} = 47^{\circ} 42' 40''$ , and the half of this or  $23^{\circ} 51' 20''$  constitutes what is called the obliquity of the ecliptic.

The accuracy of observations made with the quadrant could not be great till the invention of the telescope and the vernier or reading microscope. The quadrant though

a good instrument with these appendages, and was long so used, has, at last, given place almost universally to the circle which by means of verniers reading round the whole circumference destroy by mechanical means, probably, the small incidental errors inseparable from materials and workmanship however excellent both may be. "With all the care that could be employed, errors to the amount of 20" or 30" were known to exist in the observations of some of the continental observatories, and even to the amount of from 5" to 10" in those of Greenwich." Indeed, Troughton has been heard to affirm that a well divided circle of a single foot in diameter is more to be depended upon than a fixed quadrant of the largest construction. In a series of four observations made with the six inch circles of Kater as constructed by Robinson, I have never found, under favourable circumstances, the errors to exceed ten or fifteen seconds. Now, in the preface to the first volume of the Greenwich observations, published by Maskelyne in 1776, he makes the following remarks: "The sun and moon and some of the principal fixed stars are constantly observed on the meridian every day when the weather will permit; and the exactness of the instruments is so great, and their rectifications so nice, that the place of any heavenly body may always be found by them within ten seconds of a degree both in longitude and latitude, and generally much nearer." He then possessed a great mural quadrant of eight feet radius, by Bird, and we, therefore, see that our small circles of a few inches in diameter are nearly as accurate as the old quadrants of as many feet, and they approach much nearer to perfection than we had any reason to anticipate. Such small portable circles are consequently very valuable to the amateur astronomer, as well as the scientific traveller, since in the hands of a skilful observer, they furnish results highly useful for the improvement of geography, astronomy, and navigation, while at the same time their moderate price enables many to become purchasers.

In a letter from Captain Kater of the 25th of February, 1831, he remarks: "the size I recommend, and which I use is only 3 inches in diameter, and in the latest construction has only a vertical circle which can, however, be placed in the plane of any two objects so as to take the angle between them, the whole contained in a box 7 inches

long,  $4\frac{1}{2}$  inches wide, and 3 deep, so that it really deserves the name I originally gave, that of a pocket azimuth and altitude circle. With this little circle I can get, in one evening, my latitude to within 5" of the truth by the pole star." Such are literally the expressions of the late Captain Kater, the inventor of this instrument, and the advantages of it to scientific travellers are very obvious.

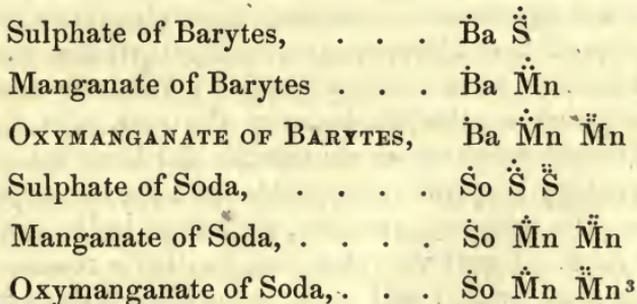
(*To be continued.*)

ARTICLE VIII.

*On a difficulty in Isomorphism, and in the received constitution of the Oxygen Salts; in a Letter to Professor Mitscherlich, of Berlin, from THOMAS CLARK, M. D., Professor of Chemistry in Marischal College, Aberdeen.*

(*Concluded from vol. iii. page 443.*)

II.—Adopting the alternative of retaining the received views of the constitution of the Oxymanganate of Barytes, and assimilating thereto our views of the constitution of the Sulphate of Soda, consistency compels us to adopt the following formulas:—



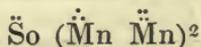
In this list, the alterations on the formulas are limited to the salts of Soda. Those alterations have given rise to the following difficulties:—

1. The basis of all the oxygen-salts of soda is assumed to be an unknown oxide of sodium, retaining half as much oxygen as is in soda.
2. The sulphate of soda is assumed to contain, not sulphuric acid, but another undiscovered acid having in combination additional oxygen, and by a like assumption, man-

ganate of soda contains, not manganic acid, but the oxymanganic.

3. The oxymanganate of soda is assumed to contain, not oxymanganic acid, but another acid, composed of manganese 4 atoms, and oxygen 15 atoms.

Here, again, it may be supposed that material difficulties may be avoided by retaining, without alteration, the first formula for the oxymanganate of soda:—



But this would involve us in the new inconsistency of assuming the soda-manganate to have, as a constituent, the same acid as the oxymanganate either of soda or of barytes, while we admit that the barytic-manganate is constituted by a different acid.

I do not know whether you, or other chemists, may see any way of reconciling with the constitution commonly assigned to the oxygen-salts, the notion that sulphate of soda and oxymanganate of barytes are analogous in constitution. I profess I can see none. The difficulties already pointed out, as consequences of admitting that analogy, have occurred while our attention was limited to those two, and four or five other salts; but how would such difficulties be multiplied and aggravated, were we resolutely to trace the consequences of that admission, throughout all the wide and varied field of chemical combination? Wherefore, all idea of analogy of constitution between the two salts in question, I would renounce as chimerical, did I not believe that such analogy is quite reconcilable with the constitution of oxygen-salts and oxygen-acids, according to the other and better view. I say *better view*, and I will give reasons; but, wishing to be brief, I will confine my observations to what may be called internal evidence, arising from a consideration of the constitution of the oxygen-salts, according to both views, as modified by the known results of analysis.

Δ being employed to represent an atom of any metal that may be conceived to be in the basis of any oxygen-salt, the following formulas will exemplify the constitution of sulphates, assumed to contain bases oxidized in different degrees, according to the view commonly taken of such salts:—

## Instances.

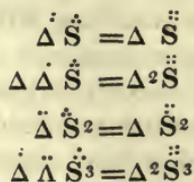
- I.  $\Delta \overset{\cdot}{\underset{\cdot}{S}}$  { Protosulphate of tin; of iron; of manganese.  
Persulphate of mercury—of copper.
- II.  $\Delta \overset{\cdot}{\Delta} \overset{\cdot}{\underset{\cdot}{S}}$  Protosulphate of mercury—of copper.
- III.  $\overset{\cdot}{\Delta} \overset{\cdot}{\Delta} \overset{\cdot}{\underset{\cdot}{S}}^2$  Persulphate of tin.
- IV.  $\overset{\cdot}{\Delta} \overset{\cdot}{\Delta} \overset{\cdot}{\underset{\cdot}{S}}^3$  { Persulphate of iron—corresponding sul-  
phate of manganese—sulphate of alumina  
—of chrome.

These four descriptions of sulphates contain each one atom of oxide. The first and second contain each one atom of acid; but, what demands most attention, the third contains *two* atoms of acid, and the fourth, *three* atoms of acid. But, admitting that each of these four descriptions of sulphates contains one atom of oxide, and that the first and second contain each one atom of acid, analogy would lead us to expect that the third and fourth sorts would also contain one atom of acid in each. Sulphurous acid, on being farther oxidized, so as to become sulphuric acid, does not, in consequence of having acquired more oxygen, combine with more potash, in order to form a neutral salt, and, indeed, hypo-sulphuric acid, which is undoubtedly more oxidized than the sulphurous, combines, for the same sulphur, with only half as much potash. Chemists, guided, as they may conceive, only by the result of analysis, may, it is true, choose to regard, as a rule of combination, that any oxide, the basis of a salt, requires, for neutrallization, as many atoms of acid, as itself contains of atoms of oxygen. But we are too apt to regard, as the result of analysis what is merely our own arbitrary expression of that result. The objection to our admitting, as a law of nature, that oxides combine with acids according to such a rule, lies in this: that not only does such a rule imply that oxides, in their combinations, observe a law different from what bodies not oxides observe, but it implies that one class of oxides observe a rule of combination, different from another class of oxides. This will at once appear on considering the following Table, where  $\Delta$  and  $X$  stand for any two oxidizable metals, and where the combination of the oxides of each metal is represented as taking place, reciprocally in the same number of atoms of each oxide as the other combining oxide contains of atoms

of Oxygen. The table gives first the compound of the two protoxides ( $\dot{\Delta} \dot{x}$ ).

	$\dot{X}$	$\ddot{X}$	$\dot{X} \ddot{X}$	$\dot{\dot{X}}$
$\dot{\Delta}$	$\dot{\Delta} \dot{X}$	$\dot{\Delta}^2 \ddot{X}$	$\dot{\Delta}^3 \dot{X} \ddot{X}$	$\dot{\Delta}^3 \dot{\dot{X}}$
$\ddot{\Delta}$	$\ddot{\Delta} \dot{X}^2$	$\ddot{\Delta} \ddot{X}$	$\ddot{\Delta}^3 (\ddot{X} \ddot{X})^2$	$\ddot{\Delta}^3 \dot{\dot{X}}^2$
$\dot{\Delta} \ddot{\Delta}$	$\dot{\Delta} \ddot{\Delta} \dot{X}^3$	$(\dot{\Delta} \ddot{\Delta})^2 \ddot{X}^3$	$\dot{\Delta} \ddot{\Delta} \dot{X} \ddot{X}$	$\dot{\Delta} \ddot{\Delta} \dot{\dot{X}}$

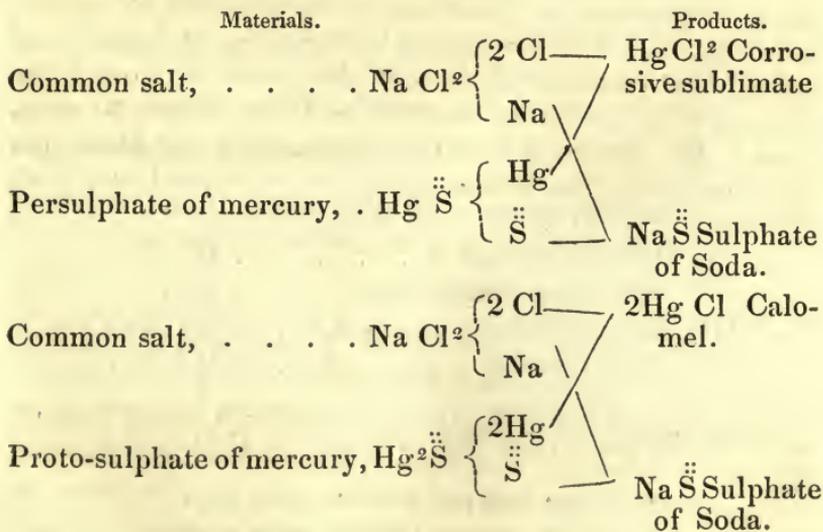
A little study of the structure of this table, and consideration of its contents, will evince, that, consistently with the rule according to which it is framed, any two oxides, in combining together so as to form neutral compounds—which may be supposed the simplest sort of combination that can take place between two oxides—ought to combine *in such proportions only that each oxide would contain an equal quantity of Oxygen*. How remote this is from the fact, it were superfluous to say. Impossible, therefore, to be general in its application, the supposed rule can only be regarded as partial. But partiality is unlike a law of Nature, and indeed the partiality disappears whenever we regard the Oxygen-salts as having metals, and not oxides, for their bases. The general formulas of the neutral Sulphates are then transformed, as follows:—



When taking this altered view of the Sulphates, a striking fact is brought into light. So much of the Oxygen as the common view regards as belonging to  $\Delta$ , always, in neutral salts, occurs exactly in the supplementary proportion necessary to make up the acid-radical,  $\dot{\dot{S}}$ . Hence, ac-

According to the new view, the anomaly of one class of oxides combining according to a rule different from other oxides, and from other bodies, disappears; for, according to this view, when Oil of Vitriol, regarded as an Hydrogen acid ( $H^2 \ddot{S}$ ), acts on an oxide, it is not a simple combination that takes place, but a double decomposition, resulting in a neutral salt and water, precisely as takes place when Hydro-chloric acid acts on oxides. While, therefore, on regarding Oxygen salts as having metals for their bases instead of oxides, the anomalous aspect of such of them as are formed from oxides of high degrees of oxidation disappears, we do not need, in taking this view, to seek any new supposition to stand upon. Nor can it escape your observation, that, regarded in this view, all the oxygen is in a state of unity; whereas the former view presented it broken asunder, like a sphere, into two irregular parts, which, when examined apart, seemed neither of them symmetrical, but which, being joined again, conceal all that before appeared irregular, attesting at once the violence that had rent them asunder and the unity of the artist's design.

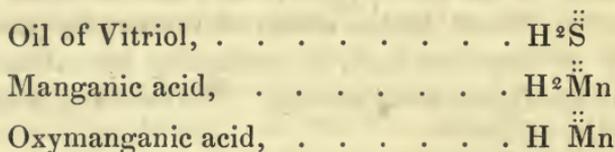
Such unity of all the oxygen in any neutral salt, is remarkably confirmed by the action of the two Sulphates of Mercury on common Salt, in producing by sublimation, Corrosive Sublimate and Calomel. The following diagrams indicate the actions—



These undeniable actions appear to me to demand evidence, such as never yet has been adduced, in proof of the usual explanation that, in undergoing mutual decomposition by another neutral salt, every neutral Sulphate divides its own Oxygen so as to leave, with the metal it contained, one-fourth, while transferring, to the new Sulphate produced, the remaining three fourths.

To avoid becoming unseasonably tedious by insisting on such details, I shall draw them to a close, trusting that enough has been stated to establish, that no Chemist is obliged to reject any view, otherwise well founded, merely because that view is inconsistent with the doctrine of Oxygen salts having oxides for their bases, provided he perceive that the view in question is not inconsistent with the doctrine of Oxygen salts having metals for their bases. Accordingly, I proceed to show that the suggested analogy of Oxymanganate of Barytes and the waterless Sulphate of Soda, although inconsistent with the former doctrine, is quite consistent with the latter.

Regarded as Hydrogen acids, the acids of the Salts we have been more particularly considering would be



I am not sure that Chemists have taken much notice of the varying proportions of Hydrogen in its acids, so far as that element re-places the metallic bases of neutral salts. But, of the following four Hydrogen acids, the Hydrogen varies, without any doubt, in the first and second, and, with much probability, in the third and fourth—

- I. Hydrochloric Acid, . . . . . H Cl
- II. Sulphuretted Hydrogen, . . . . H<sup>2</sup> S
- III. Hydro-ferricyanic Acid,\* . . . H<sup>3</sup> (Fe Cy<sup>6</sup>)
- IV. Hydro-ferrocyanic Acid,† . . . H<sup>4</sup> (Fe Cy<sup>6</sup>)

Sulphuretted Hydrogen, you will observe, has here, in respect of Hydrogen, the same relation to Hydrochloric acid

\* Acid that forms with Potash the red Prussiate.

† Acid that forms with Potash the yellow Prussiate.

that I have supposed, Oil of Vitriol to have to Oxymanganic acid, assuming both of these to be Hydrogen acids.

Regarded as having metals for their bases, the salts themselves would be constituted, as follows—

Sulphate of Barytes, . . . . .	Ba $\ddot{S}$
Manganate of Barytes, . . . . .	Ba $\ddot{Mn}$
Oxymanganate of Barytes, . . . . .	Ba $\ddot{Mn}^2$
Sulphate of Soda . . . . .	So $\ddot{S}^2$
Manganate of Soda, . . . . .	So $\ddot{Mn}^2$
Oxymanganate of Soda, . . . . .	So $\ddot{Mn}^4$

Remembering the relation of the acids, as just now explained—remembering that in the salts, constituted as this table sets forth, Barium has to Sodium the same relation as Tin, in its proto-compounds, has to Tin, in its per-compounds—I cannot, in the view here presented, perceive any difficulty requiring elucidation, nor any obstacle to forbid our admitting, as analogous in constitution, the salts that have called for all this consideration, in consequence of their being alike in form. These, represented as analogous, are as follows—

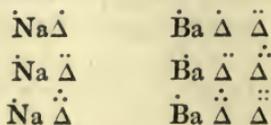
Oxymanganate of Barytes, . . . . .	Ba $\ddot{Mn}^2$
Waterless Sulphate of Soda, . . . . .	So $\ddot{S}^2$
Waterless Seleniate of Soda, . . . . .	So $\ddot{Se}^2$
Sulphate of Silver, . . . . .	Sv $\ddot{S}^2$
Seleniate of Silver, . . . . .	Sv $\ddot{Se}^2$

(Silver, in all that went before, being represented by Ag, at the usual atomic weight, but here by Sv, at double that weight.)

Throughout the foregoing observations, I desire to be understood, as regarding the suggested analogy of oxymanganate of barytes and the other salts in question, not as a main proof, nor indeed as one of the proofs, of the doctrine of oxygen-salts having metals for their bases, but only as a refutation of the rival doctrine, that such salts have oxides for their bases.

Thus, in its consequences, threatening to shake what chemists have been accustomed to consider as most fixed, the difficulty in isomorphism that I have pressed upon your attention, is not, like some former difficulties, one of mere detail, rectified, perhaps, by adverting to the water of crystallization, and, when rectified, leaving unaffected all the other details, and all former views of chemistry. Here, on the contrary, is a difficulty concerning a point, upon which, when granted, the world of chemical doctrine may be moved. Proportional to the importance of such a point at issue, will be the caution of chemists in scrutinizing the stability of the evidence. On a single point, indeed, however well established, few men will be disposed to rest all the consequences that the one at issue may be destined to bear. Even Archimedes, it may be suspected, had he, in answer to the enthusiasm of his wish, obtained that one stable point he desired, would, in the moment for action, have sighed for another. Content, therefore, with depicting to chemists the consequences of this difficulty, I leave the issue to be determined, as it can only be, by some future instance, equally unequivocal, of coincident form and constitution in compounds of sodium or silver, compared with compounds of barium, or strontium, or lead, or calcium, and, perhaps, I might add other metals. Such coincidence, in respect of constitution, will accord, it may be supposed, either with the received atoms of sodium and silver, or with those atoms doubled. That the coincidence shall prove according to the received atoms of those metals, is rendered little probable by the fact, that, according to that standard, many coincidences in constitution are already known, without any coincidence in form having been yet observed; whilst, according to those atoms doubled, scarce any crystalline compounds of entirely coincident constitution are as yet known.\* If observation, which must be the final arbiter,

\* Supposing that the present atomic weight of sodium should be doubled, the following formulas would represent, according to the received constitution of oxygen-salts, and to the *present* atomic weights, some salts of soda and of barytes that might prove of coincident form:—



shall determine one coincidence more to accord with the doubled atoms of sodium and silver; then, for aught I can see, the doctrine of oxygen-salts having oxides for their bases must at once be abandoned; but, if observation shall determine any coincidence to accord with the received atoms of sodium and silver; then, if we admit coincidence in form, we must also admit discrepancy of constitution, for though barium substitute these metals in compounds, without affecting the form, it must be *in the proportion, both of atom for atom, and of one atom for two atoms*; then, too, must we seek for an explanation of all known coincidences, however striking in form and in constitution, in the emptiness of some such phrase as “a random-concurrence in the chance-disposition of atoms;” and, then, must all the specious fabric of isomorphism, stable though it seems, vanish like a dream.

Sir, I am,  
Yours, with sincere esteem,  
THOMAS CLARK.

Marischal College,  
Aberdeen, April 1, 1836.

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#### ARTICLE IX.

*Examination of the water of the North Well at Scarborough.*

BY ROBERT D. THOMSON, M. D.

I am not aware that any recent analysis of this water has been published. A book was written on the subject, about the beginning of last century; but it is scarcely necessary to say, that, the results given in that work are completely at variance with those which I obtained.

In the specimen which I examined, the water was transparent and colourless; a few brownish red flocks were, however, deposited at the bottom of the bottle in which the water was contained. Its specific gravity was 1.003354.

The presence of sulphuric and muriatic acids and of lime was indicated by muriate of barytes, nitrate of silver, and oxalate of ammonia; litmus paper was not acted on.

1. 1000 grains of the water were placed in a phial which was laid in an inclined position on the sand bath until the liquid was evaporated to dryness. The residue weighed

3.37 grains. Again 500 grains were treated in the same manner; the remainder amounted to 1.68. Now  $1.68 \times 2 = 3.37$ , precisely the same result as in the first trial.

2. 1000 grains of the water afforded by means of nitrate of silver .418 grains chlorine.

3. A similar quantity yielded 2.07 grains sulphate of barytes = .4028 grains sulphuric acid.

4. To a third portion oxalate of ammonia was added, by which means the lime was precipitated. It amounted to .964 grains carbonate of lime, or .269 grains oxide.

5 To obtain the magnesia and soda 1000 grains were weighed out. The lime was precipitated by oxalate of ammonia. The liquid separated from the precipitate was placed in a platinum capsule and reduced to dryness. A few drops of sulphuric acid were added to convert the magnesia and soda into soluble sulphates, which were evaporated to dryness, and heated carefully to redness in order to drive off the excess of acid. The weight amounted to 1.3 grains. This residuum was then dissolved in water, the magnesia precipitated by boiling with carbonate of soda. This carbonate when heated gave .5028 magnesia. The soda, therefore, amounted to 0.0100 grains.

The iron being held in solution by carbonic acid was all precipitated before the water reached me, so that I was unable to estimate its quantity. The proportion, however, precipitated was not considerable.

According to this analysis the solid contents in the imperial gallon are,

Sulphate of magnesia . . .	105.94
Sulphate of lime . . . .	47.64
Chloride of calcium . . .	38.00
Common salt . . . . .	7.23
	<hr/>
	198.81

#### ARTICLE X.

#### *The Art of Dyeing.*

(Continued from vol. iii. page 455.)

#### YELLOW FROM QUERCITRON AND ALUM MORDANT.

To obtain a light quercitron-yellow, the calico is mordanted in the manner described under berry-yellow. The same

method is also to be adopted in procuring a dark yellow. The mordanted cloth is then cleared by immersing it in running water. The boiled powder is very rich in dye-stuff. To obtain the dark yellow, for 14 lbs. of mordanted cloth it is only necessary to employ 3 lbs. of quercitron powder, while, for 14 lbs. of cloth 7 lbs. of the fibrous matter which remains after boiling are required. The powder also gives a clearer yellow.

*Dyeing.*—On account of the sharp nature of the fibre of the quercitron which sticks to the cloth, and is not easily separated, it is necessary, when one is about to dye with the fibrous part, to place it in a linen bag, and boil it several times with water. This decoction should then be mixed with water, and heated to  $65^{\circ}\frac{3}{4}$ . The cloth well moistened and mordanted should then be dyed at a temperature between  $110^{\circ}\frac{3}{4}$  and  $122^{\circ}$ . When the solution is formed from 15 lbs. quercitron bark and 1 lb. lime the yellow is formed very pure. The addition of bran answers still better, as it does not precipitate the colouring matter. The best proportion is 2 lbs. quercitron bark to 3 lbs. bran. If the boiled powder is employed, 2 lbs. quercitron powder are used with 6 lbs. bran. Clearing is not required in this colour.

*Properties of quercitron-yellow.*—This colour is not very permanent when exposed to the air and light, but it is more permanent than many other yellow colours.

*Boiling soap-suds* consisting of 1 lb. soap in 200 lbs. water is coloured yellow without deteriorating the colour.

*Solution of potash* makes brownish spots which vinegar makes white.

*Ammonia, lime water, and vinegar* produce no change on the colour.

*Lime juice* changes the colour into sulphur-yellow; ammonia destroys the colour again, and restores the original one.

*Tin mordant*, No. 1, printed in a strong solution changes the colour into sulphur-yellow.

*Solution of chloride of lime* only renders the colour brownish. If a piece of dyed calico be placed in the solution for a quarter of an hour, a dark brown colour is produced passing into nankeen; so that in this space of time it does not become white.

## YELLOW FROM YELLOW WOOD AND ALUM MORDANT.

This yellow colour is very fleeting. Its want of permanence can be well exhibited by dyeing a piece of calico with Persian berries and another with yellow wood. The former retains its deep colour, while the latter fades to a straw tint.

## VIOLET FROM ALKANET AND ALUM MORDANT.

Bright alkanet violet is produced with the alum mordant, No. 3. Dark violet with No. 1.

The mordanted cloth is cleared by immersing it in running water, and then in warm water.

*Dyeing.*—Alkanet-root has its dye-stuff, only in the bark; therefore, much is required for a dark colour; as 4 lbs. mordanted cloth to 10 lbs. alkanet root. The colouring matter dissolves in water only by means of spirit of wine. But it is important to use as little spirit as possible. The 10 lbs. of alkanet are therefore placed in 5 different vessels (1, 2, 3, 4, 5.) So much spirit (of 80°) is poured on the first as covers it. After 12 hours the red solution which has been formed should be poured off, and re-placed by fresh spirit. The latter should be decanted after 12 hours, and poured upon the roots in the vessel, No. 2. After 12 hours it will also be saturated with colouring matter. It should be decanted and mixed with the first red solution, and employed for dyeing. Fresh spirit should then be poured on No. 2, and this is again poured on No. 3, and thus in succession until the roots are deprived of colouring matter.

The solution of colouring matter in spirit described, should then be diluted with water, well mixed, and the mordanted cloth dyed in this. The water must be completely free from lime and gypsum, otherwise a great loss of colouring matter will be sustained by precipitation. The heat should be pushed very slowly to boiling, otherwise the spirit, which contains the colouring matter in solution, would be too rapidly volatilized.

Boiling soap-suds give the colour a lilac shade.

*Properties of alkanet violet.*—These are very remarkable. It is not altered by *lime water, ammonia, vinegar, lime juice, tin mordants*, nor by chloride of lime; and the blue spot produced by potash will even after 24 hours be removed

by vinegar. Air and soap-suds produce scarcely any effect. Boiling in solution of 300 lbs. water and 2 lbs. soap does not deteriorate the colour; the shades become bluer and clearer than before.

#### VIOLET BLUE FROM LOGWOOD AND ALUM MORDANT.

For bright colours, the alum mordant, No. 3, is used, or the alum solution mixed with soda in the manner described. The dark violet blue should be produced with alum mordant, No. 2. It is necessary to allow the mordanted cloth to hang up some days before the dyeing takes place. The mordanted cloth is cleared in running water.

In order that the colour may not pass into black, bran should be added to the solution. The bran is first boiled with logwood and a little water; then more water is added, and the heat increased and raised to boiling.

For 10 lbs. of mordanted cloth, 2 lbs. logwood and 2 lbs. bran are required to produce a dark logwood blue. Hot soap-suds heightens the colour, and makes it more permanent to acids.

*Properties.*—Towards light, air and soap-suds, logwood blue shews much permanence.

*Potash* makes brown spots which are removed by vinegar.

*Ammonia* does not change the dye, but dissolves a portion; and makes it paler.

*Lime water* effects no alteration.

*Lime juice* makes bright red spots which ammonia completely dissolves.

*Tin mordant*, No. 1, printed upon it forms a lilac.

*Tin mordant*, No. 2, printed on it discharges a clear violet.

*Chloride of lime* discharges a yellow like nankeen.

#### IRON MORDANTS.

Next to the alum mordants, iron mordants are of most importance to the dyer. Their affinity for the cotton fibre is, if possible, stronger than that of the alum mordants. The iron mordants bear the same relation to dark colours that the alum mordants do to light ones.

For this purpose, sulphuric and acetic acid mordants are employed.

## IRON ALUM.

Iron alum contains no alum, but derives this name from its crystalline form which resembles that of alum, and in consequence of the similarity of its chemical constitution with alum. When the alumina is taken out of the alum, and oxide of iron substituted for it, we obtain iron alum. It may be formed by mixing 78 lbs of red oxide of iron, 117 lbs sulphuric acid combined with it, both dissolved in water, and 87 lbs sulphate of potash added while boiling, and then allowing the iron alum to crystallize.

Iron alum, made on a large scale, has a clear amethyst colour; subsequently it becomes covered with a yellowish white crust which does not injure it. It is very soluble in water. The solution is yellow coloured and undergoes decomposition by boiling, while oxide of iron separates.

The solution of iron alum is employed by itself for the production of a great many colourless grounds, variously shaded according to the strength of the solution. This solution acts differently from the alum; for the stronger the solution, the more iron alum the calico takes up. A weak solution of 1-lb iron alum in 60 water with quercitron, gives a pale straw colour, while with 10 lbs iron alum and 60 water, the colour produced is comparatively very dark. In order to obtain such shades of an equable nature, it is necessary to impregnate the calico with the mordant, press it and rinse it as described. Unless very carefully dried, more of the mordant collects on the edges and faults of the calico than on other places, and produces inequalities. This is prevented by rinsing it before drying.

Solutions of iron alum may be kept for a long time without losing their properties. The air does not produce any injurious effect, as occurs with the solution of sulphate of iron.

*Acetate of iron.*—When vinegar or pyroligneous acid is poured upon heated iron, and left in contact with it for a month, a mordant of acetate of iron is formed, which when thickened with gum or starch is very useful for printing; for mordanting colourless grounds it does not answer so well.

This mordant is most readily produced by decomposing iron alum with sugar of lead. The two following formulæ are in most general use.

*Iron mordant, No. 1.*—Dissolve 20 lbs iron alum in 80 lbs of warm water, add 20 lbs sugar of lead, and agitate the mixture until all the sugar of lead is completely dissolved. This mordant when thickened with starch is employed especially for dark rust yellow; in block printing it gives also with prussiate of potash a clear dark chemical blue. After some days this mordant becomes turbid, and oxide of iron precipitates. It is, therefore, proper not to prepare more than can be used in the course of 1 or 2 days. As the mordant clears quickly, there is no difficulty in forming it on the instant by keeping prepared solutions of 20 lbs iron alum in 40 lbs water, and of 20 lbs sugar of lead in 40 lbs water. These can be added to each other when the mordant is required.

When vinegar is added instead of the water, no change takes place and no oxide of iron is precipitated. Such a mordant answers very well in place of nitrate of iron as an addition in block printing.

*Iron mordant No. 2.*—Dissolve 10 lbs of iron alum in 80 lbs of warm water, add 10 lbs sugar of lead, and agitate until all the sugar of lead is completely dissolved.

This mordant is employed for dark colourless grounds; the cloth should be dyed in the manner before described, and dried as quickly as possible. This mordant is also subject to the same changes as the iron mordant No. 1. It may be preserved by the same means, viz., vinegar adding only half the quantity of water.

Calico takes up from a solution of oxide of iron and acetic acid more iron than from a sulphuric acid solution, when also the proportion of oxide of iron is equal in both.

If two solutions are made of 1 lb iron alum in 20 lbs water, and to one of them 1 lb sugar of lead is added, and both be employed as mordants, while two equal pieces of calico are placed in contact with them for  $\frac{1}{4}$  of an hour, then rinsed and dyed in tannin colours are obtained of different shades. From which it is obvious that the oxide of iron can be taken up in greater proportion from the acetic acid solution formed by the addition of sugar of lead, than from the sulphuric acid solution; it also appears that the acetic acid iron mordant is much more rapidly exhausted of its proportion of iron, and therefore, must always be employed in a weaker state for this purpose. It is, therefore, neces-

sary when it is required to give a number of pieces of calico an equal mordanting, to add after each impregnation with the mordant a definite quantity of fresh mordant, or for the sake of great accuracy, to divide the mordant into as many portions as there are pieces of calico to be mordanted and impregnate each by itself therein. The remainder should be again collected and rinsed together.

#### BROWN FROM QUERCITRON AND IRON MORDANT.

Light quercitron brown is formed with the solution of iron alum, No. 1. Dark quercitron is produced with the acetate of iron mordant, No. 2. The mordanted cloth is purified by passing it through a cow-dung bath. To produce the dark quercitron brown, 3 lbs. of quercitron powder are employed for 12 lbs of mordanted cloth.

For the purpose of dyeing it, the decoction of quercitron bark is employed, being made lukewarm by the addition of cold water. The dyeing is prolonged by gradually raising the temperature to boiling. The addition of lime is unnecessary. Hot soap-suds do not improve the colour.

*Properties of quercitron brown.*—This colour is very permanent; light and air alter it very little, and soap-suds by continued action only make it a little paler, while the solution itself becomes yellow.

*Solution of potash* forms reddish brown spots which are removed by vinegar.

*Ammonia and vinegar* produce no alteration.

*Lime water* forms a scarcely perceptible brown spot which vinegar removes.

*Tin mordants*, No. 1 and 2 printed in strong solution, produce (especially No. 1) a pure yellow, and may be used as dischargers.

*Solution of chloride of lime* acts similar to lime-water, rendering the colour brown, but not destroying it.

*Remark.*—Persian berries give a similar colour with similar properties, only that the yellow which the zinc mordants produce is more lively and saturated.

#### BROWN FROM OAK BARK AND IRON MORDANT.

Light brown from oak bark is produced with the solution of iron alum; dark brown with the acetate of iron mor-

dant. The mordanted cloth is purified by passing it through a cow-dung bath. To form a dark brown from oak bark 6 lbs of mordanted cloth are to be employed for 12 lbs of oak bark.

*Dyeing.*—When the oak bark is very finely pulverized a number of small chips (as in the quercitron bark) stick to the cloth. We should, therefore, employ a cool decoction for dyeing, and heat the solution gradually to the boiling point. Hot soap-suds effect no improvement of the colour.

*Properties of oak bark brown.*—This colour is very permanent in the air, light and soap-suds. When boiled with soap-suds the colour becomes first darker, then brighter.

*Solution of potash* makes reddish brown spots which vinegar does not completely remove.

*Ammonia and vinegar* produce no alteration.

*Lime water* forms a scarcely perceptible brown spot, which vinegar removes.

*Lime juice* forms a grayish white spots, which ammonia converts into dark brown.

*Tin mordants*, No. 1 and 2, printed in a strong solution, take the colour completely away and form a bright nut brown.

*Solution of chloride of lime* printed upon it acts in a similar manner to the lime water, not destroying the colour, but rendering it strongly brown.

*Remark.*—Willow bark (especially the basket willow) gives similar colours with the same properties.

#### YELLOW BROWN FROM YELLOW WOOD AND IRON MORDANT.

Light yellow brown from yellow wood is formed with the solution of iron alum, No. 1. Dark yellow brown from acetate of iron mordant, No. 2.

When the cloth is passed through a hot cow-dung bath, the dark colour becomes blacker. The cloth is purified by rinsing it in running water.

To produce a dark shade 8 lbs of mordanted cloth are employed with 3 to 5 lbs of yellow wood.\*

*Properties of brown from yellow wood.*—In light and air this colour fades somewhat, while it acquires a bright but not disagreeable colour.

\* With this dyestuff dark colours can only be obtained by adding a great excess.

When boiled for  $\frac{1}{2}$  of an hour in soap-suds consisting of 1 lb soap to 200 lbs water, it loses much of its colour and acquires a light yellow brown shade.

*Solution of potash* forms reddish brown spots, which are completely removed by vinegar.

*Ammonia* dissolves the dye and becomes yellowish.

*Lime water and vinegar* have no injurious action.

*Lime juice* forms light yellow spots, which after being moistened with ammonia become greenish.

*Tin mordants*, No. 1 and 2, printed upon it, form a pure yellow.

*Solution of chloride of lime* printed upon it, forms a dirty brown.

#### BROWN FROM TANNIN AND IRON MORDANT.

Light tannin brown is formed with the solution of alum, No. 1; dark tannin brown with the acetate of iron mordant, No. 2. The colour is purified by passing it through a cow-dung bath.

To form dark tannin brown 13 lbs of mordanted cloth are employed with 5 lbs of tannin. The dyeing is performed by gradually raising the heat to boiling. Soap-suds do not improve the colour. The bright tannin brown becomes darker by being passed through hot soap-suds.

*Properties of tannin brown.*—Tannin colours which are fixed by iron mordants are on the whole very permanent in air and light, and stand washing very well.

*Lime water and ammonia* do not alter the dark tannin brown.

*Solution of potash* produces a reddish brown colour, which is completely removed by vinegar.

*Lime juice* makes yellowish white spots which are re-dissolved by ammonia.

Vinegar has no injurious action.

*Tin mordants*, No. 1 and 2, when printed on it form a sulphur yellow, and cannot, therefore, be employed to discharge it.

*Solution of chloride of lime* produces no change on the dark tannin brown.

#### BLACK FROM LOGWOOD AND IRON MORDANTS.

Light logwood black, which is a peculiar gray, like all black colours becomes gray by dilution. It is formed

with the solution of iron alum, No. 1. Dark logwood black is produced with the acetate of iron mordant, No. 2. Logwood colours do not stand stronger iron mordants.

The mordanted cloth, as with all colours which have iron for their base, is purified in the cow-dung bath. To form the dark logwood black, 4 lbs. of mordanted cloth are required for 1 lb. of logwood.

*Dyeing*.—For this purpose a decoction of logwood is employed, which is rendered lukewarm by the addition of cold water. The cloth should be dyed in this, but the solution should not be boiled.

The black acquires, by hot soap-suds, a deep velvet lustre.

*Properties of logwood black*.—This colour, when it is formed with not too much iron mordant, is very permanent, while light, air and washing with soap have but little action on it.

*Lime water, ammonia and vinegar* have no injurious action.

*Lime juice* produces a slight spot which ammonia completely removes.

*Solution of potash* changes the logwood black most. It makes a brownish yellow spot which completely disappears by immersion in vinegar.

*Tin mordants*, No. 1 and 2, printed on it in a stronger solution discharge a violet colour.

(To be continued.)

## ARTICLE XI.

### ANALYSES OF BOOKS.

*Narrative of an Excursion to the Lake Amsanctus and to Mount Vultur in Apulia.* By CHARLES DAUBENY, M.D. F.R.S., &c.  
(Read to the Ashmolean Society.)

THE localities to which this tour refers are situated in the midst of volcanoes, which have been comparatively in recent action. The author has devoted much time to the careful investigation of the chemical products of volcanoes, and on the present occasion these have not been forgot. He compares Lagoni with Amsanctus. "At the Lagoni we see pools of water in a state of absolute ebullition, from the quantity of steam which is constantly rising through them, and which imparts to them a temperature exceeding 180° of Fahrenheit. This steam seems to carry up with it boracic acid and sal ammoniac; the former in sufficient quantities to make it worth while to evapo-

rate the water through which it passes in order to collect it; the liquid being conducted into shallow troughs, where it is mixed with soda, by which addition crystals of borax are obtained from it as the aqueous portion escapes.

“Now, when we compare together the effects produced by the disengagement of steam and sulphuretted hydrogen, owing almost unquestionably to a volcanic cause, in the instance before us and in that of the Lake Amsanctus, we are naturally led to apply the same explanation to those immense deposits of sulphur which occur on the western side of Sicily. If any doubt should exist as to the fact of their having been so produced, it may be removed by reflecting, that we know of no instance of sulphur being sublimed in uncombined form by volcanic action, and that seems scarcely possible for such an event to occur, without the combustion of the sulphur taking place the instant of its coming into contact with atmospheric air.

“Hence it may be inferred, that the whole of this vast deposit in Sicily has been occasioned by a decomposition of sulphuretted hydrogen gas, such as has taken place on a smaller scale at Lake Amsanctus, and has there impregnated the surrounding rocks with crystals of the same material.

“In Sicily too we meet with all the combinations which sulphuric acid is capable of forming with the earths present—in the beds of sulphate of lime, of strontian, of barytes, of magnesia, that occur—there also we see in the immediate neighbourhood warm springs impregnated with sulphuretted hydrogen—memorials, as it were, of the cause, which had produced these deposits.

“May we not also be led to conjecture, that the gypsum so commonly present in the tertiary clay of Volterra and the maremnæ of Tuscany, has been produced by the same process, especially when we find this clay associated, as it frequently is, with beds or nests of sulphur.

“Thus the formation of the blue clay, in Sicily and in the maremnæ of Tuscany, would have taken place, not only on the same epoch, which is generally admitted to have been the case, but under the same physical conditions, one as the other, and a volcanic action similar to that going on at the Lago d’Ansanto and at Monte Cerboli would likewise have given rise to the deposits, which the former contain, in common with the rocks found immediately round the spots, where the above operations are at present proceeding.

“But there is another circumstance also worth noticing, although the inference to which it seems to point will scarcely receive admission until further proof can be adduced in support of it. I allude to the association of salt springs with gypsum and sulphur both in Tuscany and Sicily. Their occurrence in such localities as these might induce us to conjecture, that the same volcanic action, which produced the sulphuric salts, and volatilized the sulphur, has been instrumental also in separating the salts from its solution in water, and thus serve to explain, in these instances at least, the puzzling fact, that rock-salt is found associated, as is so commonly the case, with beds of gypsum. The connexion between the above phenomena may perhaps be seen more clearly by the following tabular view:

Volcanoes give out . . . . .	}	sulphuretted hydrogen, sal ammoniac, boracic acid, muriatic acid, steam ;
and cause . . . . .		deposits of sulphur, of sulphuric salts, of muriatic salts, &c.
Moffettes, connected geographically with volcanoes either now in action, or extinct, give out . . .	}	the same principles,
and cause . . . . .		deposits of sulphur and of sulphuric salts.
Many tertiary clays, some of which are connected geographically with volcanoes, contain . . . . .	}	beds of sulphur, of earthy sulphates, and of common salt.
Most salt formations are associated with . . . . .		beds of gypsum,
some with . . . . .		sulphur,
others with . . . . .		sal ammoniac."

The traveller it would appear is not only in bodily danger from subterraneous convulsions but also from the ignorance and superstition of the peasantry.

"Almost the last scientific traveller before myself, who made Mount Vultur the object of his researches, was the celebrated Italian geologist, Brocchi, who, burdened, not only with a hammer, but likewise with a barometer and other philosophical instruments, proceeded some fifteen years ago to ascend the mountain.

"The peasants, at a loss to conjecture the nature of his objects and his manipulations, set him down as a magician, but not knowing whether he might have come to do them good or harm, contented themselves at first with watching him, very attentively, but at a respectful distance. Unfortunately, in the very midst of his observations, the heavens lowered, the wind betokened an impending tempest, and drops of rain began to descend.

"The peasants regarded all this as the first-fruits of his incantations, and awaited with silent dismay the result; but when they saw a furious thunder-storm invading their crops, and demolishing their hopes of a coming vintage; whilst the philosopher continued inspecting with redoubled attention those instruments of his craft, which had been regarded by them before with so much suspicion, fear gave way to indignation, and they rushed forwards in a body with the full intent of demolishing the mysterious author of all this mischief.

"Fortunately for Brocchi, he had with him three or four resolute gens d'armes, as his escort; and these, with their muskets and bayonets, contrived to keep the unarmed peasants at bay long enough to enable him to escape, or his zeal in exploring the secrets of volcanic action might have been as fatal to him, as it was of old to Empedocles.\*"

\* According to a recent tourist in Ireland, a similar adventure lately befell a botanist herboring in the mountains of Connemara during the time the cholera was raging.

He was nearly murdered by the country people, in consequence of being mis-

II.—*The Equilibrium of Population and Sustenance demonstrated.* By CHARLES LOUDON, M. D. Leamington.

IN this pamphlet the author discusses the importance of insisting on the prolongation of the period of lactation as an auxiliary in balancing the population and the means of subsistence. He observes, "at present on the continent the average of children to a marriage is nearly 4·5. Reasoning on the average age of marriage and the population, as given by the last two census, the number in England is also 4·5. If, by prolonging the time of suckling, the average were reduced to 4, it is evident that population would remain stationary, because one half of our numbers die between birth and the 24th year; and the average age of females marrying is very near to that period. In France, the average age of marrying is 26 years; that of men being 29 and of women 24." Admitting this, and "child-bearing to terminate at 44, the average of time between each child will be 54 months or  $4\frac{1}{2}$  years." How long should lactation be prolonged to keep population in check? "Admit in each instance the 9 months of gestation, the ten months of lactation, and one-tenth of the remaining 35 months as an equivalent for the present increase of population, and the period will be  $13\frac{1}{2}$  months. To this, however, must be added an allowance of 6 weeks for the chances of impregnation during the 3 months and the 6 weeks, on the supposition, that in every 3 instances of lactation, impregnation takes place once. Thus, the entire time will be 15 months, or, in other words *one-third longer than the present period.* This extension of lactation must necessarily increase the 4·5 years between each child approximately to 5, and, consequently, reduce the 4·5 children in a family to 4. It has been already seen, that one half of our numbers die under the age of marriage for females; the result will then be, that there will remain only a representative for father, and a representative for mother on an average in every family in the country."

The author then proceeds to shew the capability of the United Kingdom for supporting an almost unlimited population. There is one defect in his calculations, however, viz., that they are founded upon assumed data, not upon statistical returns. There cannot be a doubt that the plan proposed would be an important auxiliary in checking population, which, it is only matter of fact to say with Malthus has always a tendency to increase beyond the means of subsistence, and can only be checked by moral restraint, that is, by the exercise of common sense, or vice and misery.

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III.—*Observations on the present state of Naval Architecture in Great Britain, &c.* By J. C. BEAMISH. Cork, 1836.

THE object of this publication is to draw the attention of naval architects to the proper construction of ships with due regard to the

taken for a French doctor sent by government to inoculate the people with this dreaded plague; and the tin box, in which he carried his plants, was regarded by them as the case, in which his stock of infection was kept, in readiness to disseminate wherever he went. See Angler in Ireland, vol. i. p. 189.

principles of hydrostatics. "A merchant ship is generally designed to carry a great quantity of weight in proportion to its length and breadth, that is, to its general buoyance, and this great weight is stowed nearly throughout the length instead of being contracted in the middle of the vessel, where its effects to cause pitching and scending would be much less. Therefore, the double disadvantage of great weight, and that weight being stowed in the extremities, which are subject to be very unequally supported in a head sea, requires that the bows of many merchant ships should have nearly as much area as can be given to prevent them, when loaded, from pitching deeply and dangerously in a head sea, and if the breadth of the stern and fulness of the quarter be in proportion to the area of the bow, motion of scending or plunging the stern into the water will be also avoided."

In a country like ours, where prosperity and navigation are so intimately allied, every hint from practical men, in reference to the construction of ships, should be carefully treasured up.

IV.—*On the Theory of Ratio and Proportion, as treated by Euclid, including an inquiry into the nature of quantity.*  
By the Rev. BADEN POWELL, M.A., F.R.S., &c.

THIS forms a communication, by the author (one of the most active and promising members of the University of Oxford) to the Ashmolean Society. The author's object in this tract is to defend Euclid from the charges of inconsistency which have been brought against him by Sir John Leslie and others, in consequence of the introduction of the doctrine of ratio and proportion as part of his system of geometry. Most of the best writers of geometry (as Legendre) omit this part in their elementary systems, and most teachers in this country pass over the 5th book, and adopting the doctrine of proportionals from algebra, proceed to apply it to the theorems of the 6th book. Professor Powell treats the subject in detail, stating the objections which have been urged against Euclid, and presenting answers to these objections. He begins with a general statement of the question; he then proceeds to the consideration of Euclid's method, or the doctrine of commensurables and incommensurables. He shews that Euclid, in his earlier books, does not even imply the idea of incommensurability. Neither is this introduced in the 5th and 6th books, and it is not till we arrive at the 10th that this edition in geometrical magnitudes, expressed by numerical measures, is broached. In the 11th and 12th books all reference to this distinction is dropped, recurrence being made to the principles of the 5th book. It is again, however, resumed in the 13th book, and is applied to various properties. The author observes, "that much of the confusion of ideas which has arisen on these subjects, has been occasioned by not observing that when we say two lines are incommensurable, the phrase is, in fact, elliptical, and we ought always to consider as understood, if not expressed, that two lines *if referred to numbers* are incommensurable. The deficiency of exact comparison in such cases is not in the *geometrical* relation of the quantities,

but in the powers and capabilities of our *numerical* system to express them. Mr. Powell then proceeds to discuss the views of the earlier geometers and of later mathematicians. He points out the misapprehension under which they all labour, from the common mistake of considering that definitions describe the thing defined instead of fixing the meaning of terms. He shews that the mistake must be corrected before reasoning can be admitted on the subject. The nature of abstract quantity is next ably treated of, and the paper concluded in the same philosophic spirit which pervades it throughout.

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V.—*The Transactions of the Linnean Society of London.* Vol. XVII, part 3rd, 1836.

The number of communications in this portion of the transactions amounts to 12, most of which are important.

BOTANY.

*Remarks on some British Ferns.* By MR. DAVID DON, Lib. L.S.

THE object of this paper is to determine how far some species of ferns recently added to the British Flora, merit the rank which has been assigned to them.

*Aspidium dumetorum*, of Smith he has ascertained to be merely a diseased state of *A. dilatatum*, which is shown by the sudden termination of the costae, and by the partial decay of the other segments.

*Nephrodium rigidum* turns out to be the same with the plant of Swartz. It differs from *N. dilatatum* and *N. spinulosum*, in having larger and more crowded sori, and a broader and more depressed indusium. The fronds are lanceolate and both the stipes and rachis are copiously clothed with long narrow ramentaceous scales, as in *aspidium aculeatum*. In *dilatatum* and *spinulosum* the rachis is nearly naked, and the stipes is furnished with fewer and broader scales. From *N. felix mas* it is distinguished by its more delicate fronds, having the *pinnulae* pinnatifid and a more scaly rachis.

*Asplenium filix foemina* is observed in the shape of two different varieties, but neither of them are entitled to be regarded as a distinct form.

*Cystea dentata* or *Polypodium dentatum* of Dickson, who first distinguished it from *fragilis*, inhabits Clova, and appears peculiar to the Scottish Alps.

*Cystea regia*. Contrary to the opinion of Hooker, Mr. Don considers this plant distinct from *alpina*, being characterised by its more compact frond, by its shorter, broader and cuneiform segments, by the still more important characters of its more copious sori, and of its narrower and tapering indusium. In the *Alpina* the segments are linear and the sori much fewer, being mostly solitary on the lobes, and the indusium broader, truncate, and not taper pointed. No British station now exists for this plant.

(To be continued.)

## ARTICLE XII.

## SCIENTIFIC INTELLIGENCE, &amp;c.

I.—*Pharmacy.*

1. *Peucedanin*.—Schlatter has obtained this substance from the root of the *Peucedanum officinale*. It is prepared by digesting the root in alcohol of 80 per cent; distilling and allowing the residue to stand. The mother liquor is then poured from the crystals, and the latter washed with cold spirits; it is then allowed to crystallize from its solution in alcohol in which it is dissolved at a boiling temperature. The crystals are colourless, transparent fine needles, destitute of taste and smell, but when dissolved in alcohol the taste is aromatic and sharp. They melt at  $140^{\circ}$  without losing weight. By increasing the heat they become green, and then grayish white. They are insoluble in cold water, and melt in boiling water without dissolving. They are little soluble in cold alcohol of 80 per cent., but easily soluble at the temperature of  $140^{\circ}$ . The solution is precipitated by water. Soluble in fat and volatile oils; decomposed by concentrated acids; not dissolved by dilute acids. Peucedanin is soluble in alkalies from which it is precipitated by acids. Assisted by heat it dissolves in carbonate of potash and ammonia, from which it crystallizes on cooling. Its solution in alcohol is precipitated by bin-acetate of lead, chloride of tin and sulphate of copper, but not by sulphate of iron.—(*Ann. der Pharm.* I. 201. *Jahresbericht*, 1835.)

2. *Cusparin*. It has been ascertained by Saladin that when at a common temperature, the infusion of the bark of the *Angustura vera*, is treated with absolute alcohol, and set aside to evaporate, at  $16^{\circ}$  crystals separate, which are *cusparin*. It is quite neutral, its crystals appear to be tetrahedral. It melts at a low temperature with a loss of 23.09 per cent. of weight; cold water dissolves a half, and boiling water 1 per cent. It is dissolved by absolute acid, and by alkalies, and precipitated by infusion of galls.—(*Jahresbericht*, 1835, 323.)

3. *Cubebin*. Monheim has lately analyzed cubeb. The powder was first treated with cold ether, and then with alcohol; the alcoholic extract treated with water and the residue dissolved in boiling water; on cooling a white substance separated from the solution, and afterwards by spontaneous evaporation another substance which he terms *cubebin*. It possesses a greenish yellow colour, an acrid and fatty taste; melts at  $68^{\circ}$ , boils at  $86^{\circ}$ , and disappears leaving a little charcoal. Cubebin is soluble in acetic acid, alcohol, ether, and almond oil. Not dissolved by oil of turpentine, potash ley, nor sulphuric acid. It is reddened by the action of nitric acid.

4. *Hyoscyamin*. Geiger has separated this substance from extract of hyoscyamus, by means of ether and alcohol. It crystallizes in splendid needles, has no smell, but an acrid tobacco like taste, an alkaline re-action and is difficultly soluble in water, but less so than a tropin. It is soluble in alcohol and ether, and affords neutral soluble salts. It dilates the pupil like atropin.—(*Jahresbericht*, 1835, 262).

5. *Colchicin*. The same chemist has also extracted an alkaloid from *Colchicum autumnale*, which was taken by Pelletier and Caventou for veratrin, but which Geiger found to be distinct. Colchicin crystallizes in fine needles, when procured by the method of Mein, has a bitter acid taste, and does not produce sneezing like veratrin. It forms with acids partly crystallizable salts. By nitric acid it is first made dark violet, then indigo blue, passing through green into yellow. *Veratrin*, by the same means, becomes first red and then yellow; but by sulphuric acid first yellow, then red, and lastly dark violet, while the *colchicin* is only coloured brownish yellow.—(*Ib.*)

6. *Aconitin*. Geiger has also obtained a bitter alkaloid from the *aconitum* which is not volatile and not crystallizable, but a white granular or colourless and transparent mass. It has an alkaline re-action, and forms neutral salts, which do not crystallize. It possesses some action upon the pupil.—(*Ib.*)

7. *Daturin*. A similar alkaloid has been obtained from the seeds of the *Datura stramonium*. It crystallizes in fine splendid prisms, possesses a bitter tobacco like taste, an alkaline re-action, difficultly soluble in cold water, more soluble in hot water, from which it precipitates in crystals during cooling. Alcohol and ether dissolve it. It dilates the pupil more powerfully than *atropin*. Hence it appears, that *atropin*, *hyoscyamin*, *daturin*, *aconitin*, and, according to Geiger, also *solanin*, possess this remarkable property. Bley found that it affords salts which are soluble in water, alcohol, and ether.—(*Ib.* 269.)

8. *Digitalin*. Lancelot gives the following process for obtaining this base from *digitalis*. From the aqueous extract of *digitalis*, an alcoholic extract is prepared by means of absolute alcohol. This is dissolved in water, filtered and mixed with dilute muriatic acid, which precipitates a yellow flocky substance, which is impure *digitalin*. It should be washed with water until the acid re-action disappears, dried, dissolved in alcohol, the solution treated with blood charcoal (*blut laugen kohle*) till it is quite colourless, and then allowed to evaporate spontaneously, by which means a fatty substance separates at the surface, and the bottom of the vessel is covered with a crystalline substance, which is *digitalin*. It is colourless, has an acrid taste, is unchangeable in the air, has an alkaline re-action, insoluble in water, soluble in alcohol.—(*Ib.* 270.)

9. *Apirin*. Bizio has obtained this alkaloid from the nut of the *Cocos lapidea*. He gives it this name because the solution of its salts while hot become turbid. It is obtained by digesting the bruised nut with water and muriatic acid, precipitating the filtered solution by ammonia, washing and drying the precipitate. It is white like starch, has no taste, but after remaining in contact with the tongue for some time it produces an impression. It has not an alkaline re-action; it is dissolved by 500 parts of cold water. On heating this solution it becomes turbid, which is removed on cooling. By dry distillation it carbonizes without melting, and the vapour smells like burned hemp. It is readily dissolved by acids. If the solution is saturated, it becomes turbid by a slight increase of tem-

perature. The portion which separates is the salt. *Tartrate of apirin* deposits by the application of heat small tetrahedral crystals. The acetic acid salt, also precipitated in a crystalline state. Apirin is precipitated by disacetate of lead but not by tan.

10. *Fraxinin*. Was obtained by Keller from the bark of the ash in the form of six-sided prisms, which are readily soluble in water and alcohol. Buchner has given it the name of *Fraxinin*. It is prepared in the same way as *salicin*.

11. *Veratrin*. This alkaloid may be prepared by treating the alcoholic extract with sulphuric acid, and the solution with the incinerated matter of blood, by which means the veratrin will be precipitated with the alkali. From one French pound, about 72 grains can be procured. This should be dissolved in dilute sulphuric acid, and nitric acid poured into the solution as long as a black precipitate falls. The solution should be filtered, then precipitated with a very dilute solution of potash; the precipitate should then be well washed and again dissolved in anhydrous alcohol. On evaporation a yellowish mass remains. This contains besides veratrin, 1st a new crystallizable base. 2nd, An uncrystallizable base. 3rd, A neutral substance. To separate them the mass should be boiled with water which leaves the veratrin and nonbasic matter undissolved; ether dissolves the veratrin and leaves the latter. The veratrin should then be combined with sulphuric acid and crystallized in long needles, by spontaneous evaporation.

The composition of veratrin has been found to be carbon 71.48. Azote 5.43. Hydrogen 7.67. Oxygen 16.42. The atom has been fixed at 36.44 by one party, and at 34.18 by another. So that we may estimate its atom at 35.25.

12. *Lobeline*. Has been extracted from the leaves of the *Lobelia inflata*, by Mr. M. S. Calhoun, by treating them with water acidulated by muriatic acid, concentrating the liquid, and agitating with alcohol which separates the earthy matter, and dissolves the active principle. By evaporating the alcohol, the lobeline is obtained. It is a soft almost fluid substance resembling nicotine. Its taste is acrid. It is very soluble in alcohol; scarcely so in ether. Charcoal scarcely removes the colouring matter. It forms salts with acids; the nitrate is dilequescent, and so are the sulphate and muriate.—(*Philadelphia Journal*, January 1834.

13. *Powder of Cubebs*. Monheim finds in 1000 parts of cubebs.

Cerumen . . . . .	30
Green volatile oil . . . . .	25
Yellow „ . . . . .	10
Cubebin . . . . .	45
Resinous balsam . . . . .	15
Chloride of sodium . . . . .	10
Extractive . . . . .	60
Lignin . . . . .	650
Loss . . . . .	155

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1000

The cubebin appears to be identical with piperin. It melts at 20° and at 86° begins to boil.—*Journ. de Pharm.* xx. 403.)

II.—*Westminster Medical Society, March 26, 1836.*

DR. ADDISON in the Chair.—DR. ROBERT D. THOMSON exhibited several specimens of vegetable concentrated extracts and infusions, which were prepared by careful attention to the nature of the elements which entered into their composition. At the request of the President, he made a few observations upon the specimens submitted to the inspection of the Society. He remarked, that in regard to extracts, it is an important object to reject all those parts which have no action upon the animal system. For example, in the *extract of conium*, the only constituents which can possess any remedial agency, are *extractive* or *tannic acid* (both of which names are applied to substances possessing identical properties), and the oxide of Concin. But tannic acid readily decomposes under particular circumstances, as by exposure to the air, when saturated with moisture, and is then converted into a different substance, *ulmin* or *ulmic acid*, possessing different properties. The principle *concin*, in which the narcotic properties of the *conium maculatum* reside, is a colourless oil, but when exposed to the atmosphere, it is changed into a green resin which imparts its colour to the extract. Hence, a good green colour is a test of the genuine nature of this extract, if proceeding from the proper colouring matter, and hence a spurious extract is too often introduced into apothecaries' shops, which is coloured by means of salts of copper. The specimens of this extract before the Society, possessed the green colour of the oxide of concin in perfection, and after having been kept for two years, was as perfect as when first formed, and not a trace of copper could be detected by appropriate tests. It was double the strength of the common extract of the shops, a dose consisting of two grains and a half. The specimen of *extract of hyoscyamus*, also exhibited, had been preserved for a similar period, without being in the slightest degree impaired, either in appearance, chemical properties, or narcotic effects. Solubility is a test of the purity of this extract. Its dose is two grains and a half.

Dr. THOMSON called the attention of the Society to a new preparation, the *kinate of quinin* from the *cinchona cordifolia*, in which the quinin, instead of being in combination with sulphuric acid, as in the form in which that medicine is usually administered, is retained in union with its native acid, and presented in the same form in which it exists in bark. The aromatic and astringent principles of the bark are also retained in this preparation, so that it actually comprises in a concentrated form all the active principles of the bark. It is pale yellow, quite soluble in water, and in doses of five grains forms a more powerful remedial agent than sulphate of quinin. Dr. T. observed, that in favour of the employment of this remedy, he might state that in the course of his experience in China, he had found bark much more effective in the cure of intermittent fever than sulphate of quinin, showing that the astringent principles of bark possessed a powerful influence on the human constitution. The grea

objection, however, to the use of the bark, is its bulky form. This objection is remedied by the kinate of quinin, which, from numerous trials, has been proved to possess fully the properties of bark.

Dr. WEBSTER observed, that the opinion given by Dr. Thomson of the superiority of bark over sulphate of quinin, was in accordance with his own experience, and he was glad to hear such good authority in corroboration of his own observations.

Dr. FERGUSON remarked, that he was astonished that Dr. Thomson should say, that the great objection to the use of bark was its bulk, when a more weighty objection was its frequent adulteration. In the West Indies, where his experience lay, the bark was almost always adulterated, and seemed mixed with something like brick-dust, whereas sulphate of quinin was comparatively pure.

Dr. WEBSTER and Dr. ADDISON, in correcting the last speaker, showed that the sulphate of quinin might be, and was as much liable to adulteration as bark, and that Dr. Thomson's remarks referred to those substances in a state of purity, and proved that bark is a more powerful curative agent than sulphate of quinin.

Dr. THOMSON then observed, that the infusions of cusparia and senna submitted to the meeting, were seven times the strength of the common infusions, and that in order to render them of equal strength, it was only necessary to add to one part of infusion, seven of water. In answer to a question as to their permanence, he stated that they and all the other infusions used in this country, had been preserved for several months without any deterioration whatever.

A well-written and comprehensive paper on worms was then read by Dr. Peregrine.—(*Lancet*, vol. ii., 1835-36.)

### III.—*Oxide of Zinc, an Antidote to Belladonna.*

IN July 1835, a mare aged nine years, belonging to the Marquis de Spinetto, accidentally swallowed half an ounce of the extract of Belladonna. Washes, camphorated spirit of wine, &c., were immediately applied without success. The nervous system was greatly affected; the animal being attacked with palpitations and difficult respiration. In such a pressing case, Dr. Chiovitti prescribed 3 ounces of oxide of zinc to be taken at 4 times in the day. In 12 hours the symptoms had disappeared. On the 5th day they again shewed themselves, but were removed by the administration of a scruple of the oxide.—*Jour. de Chim. Med.*, ii., 258.

### IV.—*Volatile Oil of Spiraea Ulmaria.*

WHEN the flowers of the *meadow sweet* are distilled with water, the aromatic water again introduced into an alembic, and distilled till about a fifth passes over into the receiver—the product is *hydro-spiroilic acid*. It is denser than water, very soluble in alcohol and ether, of a clear yellow colour, inflammable, hot taste, boils at 185°, congeals at 14°; few of its salts are soluble; nitric acid converts it into *spiroilic acid*. The *spiroilide of copper* is formed by agitating

an aqueous solution of the oil with the hydrate of copper; chlorine decomposes the spiroilides, and forms chlorides of *spiroile*—the latter being the basis of the acid. *Spiroilic acid*, when carefully prepared by taking care not to add an excess of nitric acid, forms a crystalline mass; or may be sublimed in close vessels. By exposure to the air it becomes yellow; dissolves readily in alcohol and ether. It consists of carbon 51.58, hydrogen 3.50, oxygen 44.92. *Hydrospiroilic acid* consists of carbon 66.92, hydrogen 5.35, oxygen 27.73. The *spiroilide of copper* consists of carbon 51.71, hydrogen 3.51, oxygen 22.51, copper 22.27. The subject has been largely investigated by Pagenstecher and Lowig.—*Journ. de Pharm., April, 1836.*

#### V.—*New Hydrate of Carbonate of Magnesia.*

DR. FRITZSCHE observed a deposition of two different salts, in a large quantity of a concentrated solution of carbonate of magnesia in carbonic acid water, which had remained for a winter in a glass vessel. One of these salts consisted of small needles grouped together, which gave by analysis,  $Mg O, CO_2 + 3 HO$ . The other salt consisted of plates, and lost some of its water of crystallization by exposure to the air. Its constituents are magnesia 23.70, carbonic acid 25.39, water 50.91; and its formula  $Mg O, CO_2 + 5 HO$ .—*Poggendorff Annalen xxxvii., 304.*

#### VI.—*Mode of preparing Azote.*

ONE of the easiest methods of preparing this gas, is to pass a current of chlorine through liquid ammonia. The ammonia is decomposed and muriatic acid is formed. Mr. Emmet has suggested an equally simple method. He fuses in a retort nitrate of ammonia with some pieces of zinc. The metal decomposes the nitric acid; azote and ammonia are disengaged, and when received over water the latter is absorbed. Mr. Emmet employs, for this experiment, a small cylinder of zinc, attached to a wire which passes through the tubulure of the retort. By raising or lowering the latter in the nitrate, the escape of the gas may be regulated.—*Silliman's American Journal, Feb., 1835.*

#### VII.—*Ice a Non-conductor of Electricity.*

IN the 4th series of his electrical researches, Dr. Faraday states that on investigating his new law of electric conduction, "he was suddenly stopped by finding that ice was a non-conductor of electricity." He found that a thickness of  $\frac{5}{16}$ ths of an inch of ice scarcely allowed the electricity to pass at all. Professor Bache, of Pennsylvania University, in commenting upon this observation, (*Journal of the Franklin Institute, March, 1836*) states that the non-conducting power of ice was well known to Dr. Franklin and his associates. In a series of letters to Collinson of London, written in 1747 and 48, Franklin observes "a dry cake of ice or an icicle held between two in a circle likewise prevents the shock, which we would not expect, as water conducts it so perfectly well." Watson

and Bergman had previously considered ice a conductor, although afterwards the latter with Achard and Ermann confirmed the accuracy of Franklin. The remark of Faraday, that the "assumption of conducting power by bodies as soon as they pass from the solid to the liquid state, offers a new and extraordinary character, the existence of which, as far as I know, has not before been suspected," is superseded by the experience of Franklin related in a letter to Cadwallader Colden of New York, dated April 23rd, 1752. "I do not remember," says he, "any experiment by which it appeared that highly rectified spirit will not conduct: perhaps you have made such. This I know, that wax, rosin, brimstone, and even glass, commonly reputed electrics *per se*, will, when in a fluid state, conduct pretty well—glass will do it when only red hot." Again, "a certain quantity of heat will make some bodies good conductors that will not otherwise conduct. Thus wax rendered fluid and glass softened by heat, will both of them conduct. And water, though naturally a good conductor, will not conduct well when frozen into ice."

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#### VIII.—*Residuum of Fired Gunpowder.*

LIEUT. BRADDOCK has examined the residue remaining after the explosion of gunpowder. The specimen examined was obtained from the Madras powder mills, and was collected from an 8 inch iron mortar after firing off two ounce charges: the composition of the powder being, saltpetre, 75, charcoal,  $13\frac{1}{3}$ , sulphur  $11\frac{2}{3}$ . The constituents of the residue were, sulphate of potash, 46.83, carbonate of potash, 26.73, nitrate of potash, 15.34, sulphuret of potassium, 6.65, sulphuretted hydrogen, 1.52, unconsumed charcoal, 6.60, and earthy matter 2.—*Madras Journal of Literature and Science*, Jan., 1836.

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#### IX.—*Creosote as an Antiseptic.*

HUNEFELD has ascertained that the addition of a small quantity of creosote not only prevents solutions of sugar and pastes from fermenting, (10 to 15 days in June,) but likewise reserves healthy and diabetec urine, in corked vessels, completely unchanged. Hence it may be inferred that the presence of smoke in the neighbourhood of a fermenting tun will be prejudicial to the operations of the brewer.—*Pharmac. Centralblatt*, May 1836.

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#### X.—*Lithotritry employed in Persia in Ancient Times.*

IN the Khamas and Hadschar, an Arabic medical work, whose date is unknown, under the description of the diamond, is the following sentence: "One of its properties is that of its grinding down stones in the bladder in the following way: a diamond of the size of a grain of corn is surrounded with a cover of copper, and fastened with mastic or lack; this is brought in contact with the stone and rubbed against it until it is broken to pieces, and the small portions discharged with the urine."—*Brandes' Pharmac. Zeitung*, No. 4, 1835.

XI.—*Calcaire grossier on the Garonne.*

This formation corresponding with the lower beds of the Paris basin and the London clay, begins to appear over the chalk below Blaye between Talmont and Mortagne, and extends along the Dordogne to Libourne, and along the Garonne to Marmande. 1. The lowest beds consist of a sandy limestone containing small quartz. In it are observed *Orbitolites plana* a fragment of *echini* and *milliolites*. 2. Above the former reposes a hard limestone resembling oolite, containing *milliolites* and besides *Cerithium lapidum* and *mutabile*. *Ampullaria acuta*, *Echinolampas stellifera*, &c. Then follow five other beds which are all rich in fossils, especially *Scutella nummularia*, *centicularis*, *polygona*; *Marginalis*; *Fibularia scutata*, *affinis*; *Echinus elegans*, *gacheti*; *Echinolampas affinis*, *ovalis*; *Spatangus acuminatus*, *grignonensis*, &c.

M. Desmoulin, as we are informed by Dufrenoy (Ann. des Mines, vi. 430.) is engaged with the conchology of the department of Gironde, which, when finished will be very important. The *calcaire grossier* is found almost exclusively on the right bank of the Garonne; however, between Langon and Castres, it occurs on the left bank. The rock at St. Macaire and Virelade, is very rich in fossils. Dufrenoy enumerates above 50 shells.

Near Reole the *calcaire grossier* is covered by fresh water limestone. The *calcaire grossier* is represented near Bordeaux, by sands which resemble the sand of the plastic clay. The same limestone is observed at Landes, on the Adour, at St. Justin and Dax, where numerous fossils shells have been collected. Dufrenoy gives the names of 45, as *Ostrea sinuata*, *Modeola cordata*, *Cyprina islandica*, *Conus deperditus*, *Natica patula*, *Cassis plicatula*, *Strombus giganteus*.

XII.—*Note to Dr. Boase's Paper, inserted at page 20.*

I regret that the additional observations, sent by Dr. Boase, arrived too late for insertion in the present number. He has been so good as favour me with a few grains of the substance which he suspects to be new. I have subjected it to a hasty examination, and have obtained the following results.

It possesses a resinous appearance—is not attracted by the magnet. When heated over the spirit lamp it decrepitates violently, becomes almost black, and leaves a pale yellow powder which is not altered *per se* or on charcoal before the blow-pipe. With carbonate of soda it fuses into a pale green opaque bead, which becomes buff or pale yellow on cooling. It dissolves readily in muriatic acid, leaving a quantity of gelatinous silica. Caustic ammonia added to the solution produces a yellow flocky precipitate, which is not re-dissolved by a great excess of the alkali. This precipitate being thrown on a filter, and the liquid which passed through evaporated, a yellow flocky precipitate appeared, resembling exactly that originally thrown down by the caustic ammonia, and strongly resembling a mixture of alumina and iron—not soluble in a strong solution of carbonate of ammonia.—EDIT.

XIII.—*Silk Worms.*

Professor Livini found the composition of the shells of the silk-worm to be in 45 parts :

Water, ammonia and empyreumatic oil . . . . .	19.630
Carbonic acid . . . . .	7.030
Carbonaceous matter . . . . .	16.910
Silicate of potash . . . . .	0.480
Sulphate of potash . . . . .	0.060
Carbonate of potash . . . . .	0.080
Carbonate of lime . . . . .	0.366
Phosphate of lime . . . . .	0.080
Peroxide of iron . . . . .	0.175
Carbonate of magnesia . . . . .	0.072
	44.883

Gas contained in the cocoon consists of

Oxygen . . . . .	15
Carbonic acid . . . . .	10
Azote . . . . .	75
	100

Liquor secreted by the silk worm moth, contains

Animal resin . . . . .	1.000
Uric acid . . . . .	2.000
Impure matter . . . . .	0.800
Phosphate, urate of ammonia, } acetate of potash and acetic acid }	1.20
	5.000

*Memorie del Accad. di Torino, xxxvi. 30.*

XIV.—*British Association for the Advancement of Science.*

We understand that the next meeting of the British Association will be held at Bristol during the week commencing on Monday, the 22nd August, and that the Members of the General Committee will convene on the Saturday preceding.

XV.—*Notice of New Books.*

The Rev. Professor Henslow, of Cambridge, has engaged in a New Work, to be called THE BOTANIST. It will be conducted by Mr. Maund, the author of the Botanic Garden, and is to combine all interesting points of the science, with popular and practical information. No. 1 will appear on the first of August.

Early in August will appear the First Number of "THE NATURALIST," illustrative of the Animal, Vegetable, and Mineral Kingdoms (to be continued Monthly) with highly finished coloured Engravings, and illustrated with Wood Cuts. Conducted by B. Maund, F. L. S., and William Holl, F. G. S., assisted by several eminent Scientific Men.

OBSERVATIONS on Temperature, Solar Radiation, and Humidity, made on the 15th of May, 1836, at 18, Claremont Place, Pentonville, London. By JOHN ABRAHAM MASON, M.D., &c. (Observations made every 15 minutes.)

Time.	Dr. Mason's Hygrometer in the shade.			Thermometer in the open air, in the direct rays of the sun.	REMARKS.
	Temp. of dry bulb	Temp. of wet bulb	Degrees of dryness observed.		
10 A.M.	62°	55°	7°	69°	Clear and bright.
$\frac{1}{4}$ past	62	55	7	69	Ditto.
$\frac{1}{2}$ do.	62	55	7	74	Ditto.
$\frac{3}{4}$ do.	62	55	7	77	Ditto.
11 A.M.	62	54	8	80	Ditto.
$\frac{1}{4}$ past	62	55	8	80	Ditto.
$\frac{1}{2}$ do.	61·5	54·5	7	81·5	Ditto.
$\frac{3}{4}$ do.	61	54	7	82	Ditto.
Noon.	62	54	8	83	Ditto.
$\frac{1}{4}$ past	62	54	8	83	Ditto.
$\frac{1}{2}$ do.	62·5	54·5	8	84	Ditto.
$\frac{3}{4}$ do.	62·5	54·5	8	84	Ditto.
1 P.M.	62·5	54·5	8	85	Ditto.
$\frac{1}{4}$ past	63·5	55	8·5	83·5	Rather dull, few passing clouds obscuring the sun.
$\frac{1}{2}$ do.	63	54·5	8·5	83	Ditto.
$\frac{3}{4}$ do.	63	54·5	8·5	85	Very clear and bright.
2 P.M.	64·5	55	9·5	82	Commencement of eclipse at Greenwich, 51m. 12s. after 1 P.M.
$\frac{1}{4}$ past	65	55	10	78	Limb of the moon appears near the centre of the sun.
$\frac{1}{2}$ do.	64	55	9	74	Rather obscured by light passing clouds, limb of moon appears at the centre of the sun.
$\frac{3}{4}$ do.	64	55	9	71	Rather obscured by light passing clouds, limb of moon appears at the centre of the sun.
3 P.M.	63·5	54·5	9	69	Sun very bright; about $\frac{1}{3}$ of sun visible.
$\frac{1}{4}$ past	63	55	8	67	Clear, no clouds about the sun.
$\frac{1}{2}$ do.	63	55	8	66	Greatest obscuration 19m. 6s. past 3 P.M.
$\frac{3}{4}$ do.	62·5	55	7·5	68	Sun bright; limb of the moon appears near the centre of the sun.
4 P.M.	62·5	55	7·5	68	Sun appears about $\frac{1}{4}$ obscured; clear.
$\frac{1}{4}$ past.	63	55	8	68	Very small portion of the moon visible on the sun. Termination 39m. 6s. past 4 P.M.
$\frac{1}{2}$ do.	63	55·5	7·5	66	
$\frac{3}{4}$ do.	63	55·5	7·5	68	
5 P.M.	62·5	55·5	7	67·5	Sun clear, clouds in the zenith.
$\frac{1}{4}$ past	62·5	55·5	7	66	Fine, slight breeze.
$\frac{1}{2}$ do.	62·5	55	7·5	66·5	Ditto.
$\frac{3}{4}$ do.	62·5	55	7·5	66	Ditto.
6 P.M.	62·5	55	7·5	66·5	Ditto.
Mean.	62·697	54·833	7·864	74·560	

May 17th, from 10 A.M. to 6 P.M.

Mean Temperature, . . . . .	65·182
Mean of Wet Bulb, . . . . .	57·061
Mean of degrees of dryness observed, . . . . .	8·121
Mean of Solar Radiation, . . . . .	77·182
Mean Power of Radiation on the 15th . . . . .	74·560

2·622 for the day.

Mean Power of Radiation from $\frac{1}{2}$ past 1 P.M. to $\frac{1}{2}$ past 4 P.M. on the 17th, . . . . .	79·307
Mean Ditto for the 15th, . . . . .	72·692
Minus produced by the eclipse, . . . . .	7·615

OBSERVATIONS on Temperature, Solar Radiation, and Humidity, made on the 17th of May, 1836, at 18, Claremont Place, Pentonville, London; to determine what effect the passage of the moon over the disk of the sun had in preventing Solar Radiation, &c. By JOHN ABRAHAM MASON, M.D., &c. (Observations made every 15 minutes.)

Time.	Dr. Mason's Hygrometer in the shade.			Thermometer in the open air, in the direct rays of the sun	REMARKS.
	Temp. of dry bulb	Temp. of wet bulb	Degrees of dryness observed.		
9 A.M.	62°	55°	7°	68°	Fine and clear, no detached clouds, sky blue and gray.
¼ past	62	55	7	69	Ditto.
½ do.	62	55	7	69.5	Ditto.
¾ do.	62	55	7	71.5	Ditto.
10 A.M.	62	55	7	75	Calm, ditto.
¼ past	62	55	7	75	Gentle breeze, slightly dull.
½ do.	62.5	55	7.5	75	Ditto.
¾ do.	63	55	8	75	Ditto.
11 A.M.	63	55	8	75.5	Ditto.
¼ past	63.5	55	8.5	77	Very clear.
½ do.	64	55.5	8.5	78	Less wind.
¾ do.	64.5	56	8.5	78	Ditto.
Noon.	64.5	56	8.5	78	Wind strong.
¼ past	65	56.5	8.5	79	Ditto.
½ do.	65	56.5	8.5	80	Ditto.
¾ do.	65	56.5	8.5	81	Ditto.
1 P.M.	65.5	57	8.5	83	Ditto.
¼ past	65.5	57	8.5	80	More calm, slight clouds about sun.
½ do.	65.5	57	8.5	83	Clear, wind strong.
¾ do.	66	57.5	8.5	80	Rather dull.
2 P.M.	66	58	8	79	Ditto.
¼ past	66.5	58	8.5	81	Clear.
½ do.	66	58	8	81	Ditto.
¾ do.	66	58	8	79	Rather cloudy.
3 P.M.	66.5	58.5	8	80.5	Gentle breeze.
¼ past	66.5	58.5	8	80	Wind very strong.
½ do.	66.5	58.5	8	80	Ditto.
¾ do.	66.5	58.5	8	79	Ditto.
4 P.M.	66.5	58.5	8	77	Ditto.
¼ past	66.5	58.5	8	76.5	Ditto, fine.
½ do.	66.5	58.5	8	75	Gentle breeze, clear.
¾ do.	66.5	58.5	8	74	Ditto.
5 P.M.	66	58.5	7.5	74	Ditto.
¼ past	66	57.5	8.5	71.5	Ditto.
½ do.	65.5	57.5	8	69.5	Ditto.
¾ do.	65.5	57.5	8	70	Ditto.
6 P.M.	65	57	8	68	Ditto.
¼ past	65	57	8	67	Cirrus and fibrous clouds.
½ do.	65	57	8	66	Wind increasing.
¾ do.	64	56.5	7.5	65	Ditto.
7 P.M.	63.5	56.5	7	64	Ditto.
¼ past	62.5	55.5	7	63	Ditto.
½ do.	62	55	7	62	Sun below the houses, Thermometer in the shade.

Note.—Sun rises 4h. 12m. Sun sets 7h. 49m.

# Meteorological Journal, for May.

Kept at the Manse of the Parish of Abbey St. Bathans, Berwickshire. Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea.  
By the Rev. JOHN WALLACE.

DATE.	THERMOMETER.				HYGROMETER (Leslie's.)				BAROMETER. At 32° of Fahrenheit.				Rain in Inches Weekly.	Direction of Wind at X. A. M.	REMARKS.
	IX. A. M.	X. A. M.	III. P. M.	X. P. M.	IX. A. M.	X. A. M.	III. P. M.	X. P. M.	IX. A. M.	X. A. M.	III. P. M.	X. P. M.			
1893.															
Full M. Su	40	41	43	38	0	2	29-436	29-446	29-531	29-686		N. W.	Gentle breeze, heavy masses of cumulous clouds floating, evening calm.		
Mo.	42	43	43	39	1	7	29-805	29-817	29-822	29-852		N. E.	Gentle breeze, cumulous clouds floating over a blue sky, evg. calm and cloudy.		
Tu.	43	43	45	40	1	6	29-805	29-793	29-746	29-700		N. E.	Brisk wind, A. M. cloudy with some showers; P. M. gradually overcast, in the evening some rain.		
We.	4	42	41	42	0	2	29-600	29-596	29-524	29-555	0-543	N. N. E.	Gentle breeze, thick fog.		
Th.	4	42	42	40	0	2	29-581	29-618	29-626	29-685		N. N. E.	Calm, A. M. cirri abundant, masses of white cloud floating below, P. M. cloudless		
Fr.	6	45	46	51	3	5	29-811	29-828	29-861	29-916		N. by W.	Very gentle breeze, nearly cloudless.		
Sa.	7	42	44	49	3	8	29-343	29-302	29-921		N. N. W.	Gentle breeze, A. M. particularly cloudy; P. M. cloudless.			
Su.	47	48	52	56	3	9	29-878	29-885	29-825	29-875		N. N. W.	Calm, blue sky overspread with cirri.		
Mo.	10	51	52	58	4	7	29-773	29-752	29-736	29-733		N. E.	Calm, A. M. clear, P. M. light haze over spreading the sky cirri prevalent in the evg.		
Tu.	10	57	60	64	5	3	29-680	29-685	29-566	29-581		E. S. E.	Gentle breeze, thin hazy clouds veiling the sky, dark hazey clouds in the evng.		
We.	11	58	62	65	4	12	29-484	29-515	29-477	29-505	0-000	SW by W.	Gentle breeze, clear blue sky with scattered patches of cirri and cirrostrati.		
Th.	12	52	55	62	4	8	29-481	29-497	29-530	29-640		S. W.	Gentle wind, A. M. cloudy, P. M. lowering, evening clear.		
Fr.	13	56	57	58	4	20	29-687	29-694	29-698	29-835		E. S. E.	Calm, A. M. cloudy, P. M. gradually clearing, evening cloudless.		
Sa.	14	53	55	58	4	19	30-040	30-041	30-013	30-007		W.	A. M. gentle breeze, P. M. wind increasing, evg. strong gale, cloudless all day.		
New M Su.	16	57	61	68	4	15	29-965	29-984	29-963	29-935		NW by W	A. M. gentle breeze, P. M. calm, sky thinly veiled with cirros. & cirroc., aur. bor. in		
Tu.	17	66	71	74	4	40	30-012	29-942	29-960	29-962		E.	Very calm, cirroc. & cirros. prevalent, fog in the evng. [evg. with clear sky.		
We.	18	55	59	63	4	27	29-907	29-902	29-865	29-875	0-116	E.	Very gentle breeze, A. M. hazy clouds prevalent, P. M. clear, depos. copious [in the evening.		
Th.	19	55	58	62	4	11	29-769	29-769	29-644	29-603		N. W.	Very calm, hazy, patches of cumulous cloud floating.		
Fr.	20	49	49	53	4	5	29-586	29-589	29-537	29-537		E. N. E.	Very calm, A. M. foggy, P. M. cloudy.		
Sa.	21	44	46	50	4	3	29-463	29-467	29-400	29-545		E.	Calm, A. M. rain, P. M. overcast.		
Su.	22	46	45	46	4	4	29-605	29-630	29-711		E. N. E.	Calm, cloudy, with occasional sun-shine.			
Mo.	23	44	45	45	4	25	29-611	29-605	29-630	29-711		N.	Calm, masses of cumuli floating on a blue sky.		
Tu.	24	44	46	49	4	20	29-725	29-735	29-749	29-746		N.	Calm, A. M. cumuli, P. M. cirrocumuli prevalent, in the evening soft clouds.		
We.	25	50	51	52	4	16	29-810	29-810	29-816	29-852	0-234	S. E. by E.	Calm and nearly cloudless, slight deposition in the evening.		
Th.	26	52	53	57	4	3	29-885	29-885	29-863	29-885		N. E. by E.	Calm, A. M. a few masses of cumulus, P. M. clear and cloudless.		
Fr.	27	59	60	61	4	18	29-903	29-909	29-901	29-922		N. E. by E.	A. M. gentle breeze, P. M. calm, all day cloudless.		
Sa.	28	53	53	57	4	41	29-955	29-962	29-969	29-980		N. E. by E.	Calm, A. M. cloudless, P. M. cirrostratus prevalent, passing into cirrocumulus.		
Su.	29	55	57	60	4	3	29-985	29-978	29-969	29-969		N. E.	Gentle wind, cirros., cymoid, cirroc., hazy clouds floating below, evg. calm.		
Full M. M.	30	51	52	53	4	17	29-917	29-917	29-883	29-853	0-000	N. E.	Gentle wind, A. M. cloudless, P. M. thin cirrostratus in S. W., evening calm.		
Tu.	31	53	54	55	4	7	29-775	29-770	29-712	29-633		N. E.			
Mean.	50-6	52-1	54-8	44	23	25	29-767	29-763	29-748	29-776	0-893				

Means Therm. 48° } Mean temperature of spring water 46° }  
 for the Hygr. 18° } Mean point of deposition 40°-7  
 Month { Bar. 29-779 in. } Moisture in a cubic inch of air = -001812 grs.

# RECORDS OF GENERAL SCIENCE.

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## ARTICLE I.

### *Biographical Account of Sir Humphry Davy, Bart.*

(Continued from page 12.)

It is worthy of remark, that Davy during a part of these laborious investigations, appears to have been in a feverish state, the prelude to a severe attack of illness, and his great apprehension was that he should die before he had published his discoveries. He was afterwards of opinion, that his disease was typhus fever caught during a visit to Newgate. Dr. Babington considered it the result of his hard work. It was severe and protracted. He took to his bed on the 23rd of November, and was only convalescent nine weeks after. Fortunately, no permanent evil followed from his illness, for he was able to begin his lectures in March following, with his wonted vigour and success.

During six years his exertions were most fruitful. The pages of the Philosophical Transactions testify this. Twelve papers from his pen were published by the Royal Society in that short space of time, all of them pregnant with important additions to chemistry. In 1810, he proved that the constitution of muriatic acid, contrary to the received opinions and in consonance with the deductions of Scheele, was a compound of chlorine and hydrogen. Davy's explanation was speedily received by all chemists, with the exception of one or two, who were compelled by the

force of reasoning superior to what they appeared susceptible of, to assent to the true theory. He discovered also telluretted hydrogen and sesqui-phosphuret of hydrogen about the same time, and broached the now generally received theory of the composition of fluoric acid. He also demonstrated the composition of boracic acid. In short, he was indefatigable in his exertions; "except when I resolve to be *idle* for health's sake," he observes in a letter to his mother, "I devote every moment to labours which I hope will not be wholly ineffectual in benefiting society, and which will not be wholly inglorious for my country hereafter; and the feeling of this is the *reward* which will continue to keep me employed."

When he was appointed to the Royal Institution he gave up the medical profession, but in consequence of the moderateness of his income there, he began to think of graduating, and actually entered his name at Cambridge, and kept some terms there with the view of engaging in medical practice as a physician. But his love of science was superior to the prospect of fortune, and he accordingly gave up entirely this idea. In 1810 and 1811, he was twice invited to give lectures at Dublin. His popularity was then at its height; his audiences were overflowing; his success was complete.

On the 5th of December, 1811, the Dublin society presented him with £750 as a remuneration for his labours. At the same time the degree of LL.D. was conferred on him by Trinity College, Dublin.

During all these flattering attentions he never forgot his relations, and the letters which he wrote to his brother and mother, which have just been published, do honour to his heart. But it would be erroneous to conclude, that he had no failings, and that these successes did not in some measure carry evil along with them. The splendid atomic doctrine of Dalton was treated by him at first slightly. The evidence of Drs. Thomson and Wollaston is on record to prove this. Why should Dr. Davy attempt to call in question the correctness of their statements? He says, that his brother "never thought lightly of any of his (Dr. Dalton's) views." How the art that he ridiculed Dalton's views can be reconciled with this statement it is not

easy to divine. Dr. Davy, before writing as he has done, would have shewn more prudence by consulting the living evidences of the facts. The memory of his brother can acquire no fame by insinuations against the characters of his contemporaries. His claims to distinction rest on more solid grounds, and it is matter of regret that his biographer should strain his subject, even so far as to attempt to make us believe that the philosopher was perfect.

But that Davy was not very partial to Dalton's views is obvious from the circumstance of his having published a note in which he claimed the discovery of the atomic theory for Higgins, when that gentleman had shewn him his work upon the Phlogistic Theory. It is generally admitted that Higgins' work was one of great ingenuity, but that he had any idea of the atomic theory of Dalton cannot be admitted. What the consequence would have been had the observation of Davy been allowed to pass unnoticed, is scarcely doubtful. His note bore the marks of an attempt in the opinion of some, to crush the ingenious Dalton by the superior weight of name and situation, and his influence was decided. The just claims of Dalton were supported, however, at the risk of jealousy, in the pages of the *Annals of Philosophy*, with an earnestness and effect which were irresistible, and at the present day we question if any one will deny that the claims advanced in the note of Davy were unfounded. It is true, that he afterwards did justice to Dalton as evinced in the quotations from his lectures published by his brother, but still his observations are registered against him.

On the 11th of April, 1812, Mr. Davy was married to Mrs. Apreece, a lady of fortune and daughter of Mr. Kerr. He had previously received the honour of Knighthood. What were the permanent effects of this union upon his happiness do not appear from his brother's life of him, but his own expressions speak most favourably of his improved condition, at a period soon after marriage, "She is a noble creature," he says, "and every day adds to my contentment by the powers of her understanding, and her amiable and delightful tones of feeling."

In November, 1812, while engaged in making experiments upon the chloride of azote, he was severely wounded

by the explosion of a small portion. The accident happened at Tunbridge, in Mr. Children's laboratory. The consequences were that severe inflammation attacked the eye, and the conjunctiva and cornea were obliged to be punctured. His complete recovery was suspended till April following, yet while labouring under imperfect vision and often violent pain, he found opportunities of working upon fluorine and ascertained that it expelled oxygen from most compounds.

In October, 1813, he crossed the channel with Lady Davy, to Morlaix, on a continental tour, "He was accompanied," says Dr. Davy, "by Mr. Faraday (who has since so honourably distinguished himself in original research) as his assistant in experiments and in writing, and provided with a commodious portable apparatus for instituting such inquiries as he had in contemplation." He spent about two months in Paris, occupied between the calls of society and science.

During this short period he assisted in adding another substance to the supporters of combustion, viz., iodine. The discoverer of this useful agent was M. Courtois, a saltpetre manufacturer of Paris. But to Davy and Gay Lussac, chemistry owes the developement of its properties. Davy's experiments were first published, but Gay Lussac appears to have been engaged with his experiments at the same time.

Nothing else of importance occurred during his residence in the French capital, with the exception of his cultivating an acquaintance with the philosophers of the time. His opinions of some of the leaders are interesting.

"Cuvier had even in his address and manner the character of a superior man. I should say of him that he is the most distinguished man of talents I have known; but I doubt if he is entitled to the appellation of a man of genius.

"Gay Lussac was quick, lively, ingenious, and profound, with great activity of mind and great facility of manipulation, I should place him at the head of the living chemists of France.

"La Place when a minister of Napoleon was rather formal and grand in manner, with an air of protection rather than of courtesy. This was in 1813. When I saw

him again in 1820, he was become mild and gentlemanlike." In 1813, "on my speaking to him of the atomic theory in chemistry, and expressing my belief that the science would ultimately be referred to mathematical laws similar to those which he had so profoundly and successfully established with respect to the mechanical properties of matter, he treated my idea in a tone bordering on contempt, as if angry that any results in chemistry could even in their future possibilities be compared with his own labours. When I dined with him in 1820, he discussed the same opinion with acumen and candour, and allowed all the merit of John Dalton."

From Paris, Sir Humphry proceeded to the South of France by Auvergne, where he examined the extinct volcanoes of that mountainous country. From thence he went to Montpellier—Nice—Turin and Genoa, where he made some experiments on the torpedo and on iodine. At Florence, he entered upon the study of the nature of the diamond, and of the different varieties of carbon. The results of this investigation were published in the Philosophical Transactions for 1817; his inference was, that diamond is merely crystallized carbon, as Mr. Tennant had formerly shewn.

In April he advanced to Rome, where he remained nearly a month, and then passed on to Naples.

In the course of the same year, he visited Switzerland, and returned to Italy, where he remained till April 1815, when he returned to London. In the Philosophical Transactions for that year, three papers from his pen were published, the results of his studies in Italy. In a letter to his mother, he says,—“We have had a very agreeable and instructive journey, and Lady Davy agrees with me, in thinking that England is the only country to *live* in, however agreeable it may be to *see* other countries.”

On his return from the continent, he began to investigate the properties of fire damp, for the purpose of determining whether any method could be discovered of preventing the dreadful accidents, which were so often occurring in coal mines at that period. His attention was directed to this subject first, “in August 1813, in consequence of a letter from the Rev. Dr. Grey.”

He published the results of his experiments in the Philosophical Transactions, in a series of papers, which rapidly succeeded each other, and afterwards collated them into a work entitled "On a safety lamp for preventing explosions in mines, houses lighted by gas, spirit warehouses and magazines, in shops, &c.; with some remarks on flame."

The important discovery which resulted from these investigations, was begun by an accurate inquiry into the properties of carburetted hydrogen or fire damp—although most of them had been well studied by Dalton, Thomson, and Henry; he found that it required a large quantity of atmospheric air for explosion; that it was the least readily combustible of all the inflammable gases, or required the highest temperature, and that its expansive effect from heat, was less than that of other gases. He observed, that mixing one part of carbonic acid, or fixed air, with seven parts of an explosive mixture of fire damp or one part of azote with six parts, their powers of explosion were destroyed. He observed also, that in exploding a mixture in a glass tube of one fourth of an inch in diameter, and a foot long, more than a second was required before the flame reached from one end to the other, and that in a tube of one seventh of an inch in diameter, explosive mixtures could not be fired when they were opened in the atmosphere, and that metallic tubes prevented explosion better than glass tubes. "In reasoning upon these various phenomena," he observes, "it occurred to me, as a *considerable* heat was required for the inflammation of the fire damp, and as it produced in burning a comparatively *small degree* of heat, that the effect of carbonic acid and azote, and of the surfaces of small tubes in preventing its explosion depended upon their cooling powers, upon their lowering the temperature of the exploding mixture so much, that it was no longer sufficient for its continuous inflammation." "This idea, which was confirmed by various obvious considerations, led to an immediate result, the possibility of constructing a lamp, in which the cooling powers of the azote or carbonic acid, formed by the combustion or the cooling powers of the apertures through which the air entered and made its exit, should prevent the communication of explosion." The consequence was, the invention of the safety lamp—a cage

of wire gauze, which, however strongly the flame may burn, its passage beyond the wire is prevented, it is supposed by its cooling power. This was a most important present to the lowly miner, and formed a vast addition to the interests of humanity. Numerous objections were brought forward on the publication of this discovery, some of which were correct, while others were groundless, but all of them contributed to stimulate the inventor to improve his instrument as completely as possible, and do not deserve the titles of "carping" and "disingenuity," and other unworthy terms, which have been applied to them by Dr. Davy, and which confer any thing but credit on his work. It is now admitted that the lamp of Davy is completely effective in principle, and that all the accidents which have occurred since its introduction have been produced by the carelessness of the workmen. On the 11th of October, 1811, he was presented with a service of plate, of the value of £1200 by the associated coal owners of Newcastle and its neighbourhood. He received also a splendid gilt vase from the late Emperor Alexander of Russia.

Notwithstanding that the whole of the credit of this discovery was given to Sir Humphry, there is strong reasons to suspect that Dr. Clanny and Mr. Stevenson anticipated him in the principles upon which it was constructed, as appears from the Report of a select Committee of the House of Commons appointed to investigate the subject. "The principle of its construction," says the Report, "appears to have been practically known to the witnesses Clanny and Stevenson previously to the period when Davy brought his wonderful mind to bear upon the subject." Davy even admitted privately in the case of Stevenson that this gentleman deserved some credit; but, unfortunately, such opinion was never stated in public. In a letter to Mr. Lambton (now Lord Durham) he says, "The general impression of the scientific men in London, which is confirmed by what I heard at Newcastle, is, that Stevenson had some loose idea floating in his mind, which he had unsuccessfully attempted to put in practice till after my labours were made known; then he made something like a safe lamp, except that it is not *safe*." If Davy had shown a disposition to give *any* credit to those who were labouring

in the same field, the memory of this invention would have remained unsullied ; but justice demands that facts should be candidly stated, and that the biographer of Davy should omit none even of the features of the transactions\*.

In 1817, Humphry communicated two papers to the Royal Society ; one “ On the fallacy of experiments in which water is said to have been formed by the decomposition of Chlorine ;” and the other on “ New experiments on some of the combinations of phosphorus.

Between 1815 and 1818, he made several journies to the North of England and Scotland, partly in connexion with his investigations “ relative to fire damp, but chiefly for the sake of fishing and shooting,” and one of these excursions extended as far as the Orkney Islands. Several extracts from his note books, written about this period, exhibit his opinions upon general subjects—“ When young shoots grow on a rotten branch,” he observes, “ the only way to save them is to detach them. Analogy---Rotten aristocracies and governments, and young and vigorous life amongst the people.” “ Persons of very exalted talents and virtues may be said to derive their patent of nobility directly from God ; their titles are not registered in perishable court calendars, but written in the great histories of Nature or of man.” These and other occasional productions are only interesting in so far as they show the general bent of his mind, which appears to have been to view general subjects in a kind of poetical light ; not to grasp them with a firm gripe, but to allow them to glide away in a smooth and harmonious manner. Thus he discusses the effects of the national debt, as being praiseworthy, without even a passing thought of its baneful influence on the country, and disapproves of the Walcheren expedition, merely because the scene of its exploits was a foggy country. He speaks of the advantage of having philosophers in the cabinet, instead of “ empty-headed declaimers and empty-pursed cadets from the aristocracy ;” and yet adds, “ Any philosopher would have warned government against the importation of corn, which is now weighing down the country by a diminished circulation.”

\* We understand that the documents concerning safety lamps, in favour of the claims of Dr. Clanny, are in the hands of Mr. Pease, M.P.

His opinions in reference to religion may be sufficiently gathered from his notes; and in his last work, "Consolations in Travel," he considered that "the only pure foundation of natural religion is instinctive feeling; that there is a sense in regard to religion as there is in regard to colour, sound, tastes, or smells; or as in regard to the propensity for society, and the ties of kindred and family; that they who have the taste or instinctive feeling will be religious, obeying the impulse of their natures, and see a supreme intelligence governing the universe by fixed laws, and will worship this intelligence in its power and goodness displayed in all the works of creation; whilst those (if any such, there be) who are destitute of the feeling can no more acquire a sentiment of religion, than a blind man can a notion of colours, or the deaf of sounds; and that, consequently, like the brute animal, their desires must be very much bounded by the present, and will be low and grovelling---no hopes beyond the grave; no aspirations after immortality; no fervent, however humble, longings after the perfecting of their nature, the exaltation of intellect, the purifying of sense, and, in brief, the acquirement of a glorified nature, such as we imagine belongs to angelic beings and to the spirits of the just made perfect." The term instinct here indicated as the cause of religion, is one which has been too much employed to mar inquiry, and refers to some unknown secondary cause. But what Davy meant by instinctive religion, was, merely, that certain individuals are constituted with benevolent dispositions, the result undoubtedly of peculiar organic developement, and are, consequently, more capacitated for cultivating the finer sensibilities of our nature, and exhibiting gratitude suitable to the author of our being. His answer to those who would annihilate the human mind after the termination of the present earthly scene is well conceived—

"If matter cannot be destroyed,  
The living mind can never die."

In May, 1818, he left England on his second journey to the Continent. He passed through Flanders, into Germany, and arrived at Vienna about the 13th of June. He then proceeded to Venice, and crossed the Appenines to

Rome. Soon after he arrived he wrote a paper "On the formation of mists in particular situations," which was contributed to the Royal Society. He now began his researches at Naples on the Herculaneum MSS., for the purpose of applying his chemical skill to the enrolling of which he had particularly visited Naples. The result of this investigation was published in the *Philosophical Transactions* for 1821. In this paper he proves in a satisfactory manner that the MSS. at Herculaneum had not suffered from fire, but from the slow and intimate action of the elements, by which they had been converted into matter similar to peat and Bovey coal. He succeeded in unfolding them by means of chlorine and ether, and a graduated temperature, without injuring the work. The jealousy of the Museum defeated the utility of these operations to literature, and rendered them of more service to science.

At the same time he attended carefully to action going on in the volcanoes among which he was resident. The deductions from his observations were embodied in the theory which he formed, viz: that volcanic action depends on the decomposition of water, by the agency of the inflammable bases of the earths and alkalies existing in the earth. But the nature of the matter ejected, and of the gases exhaled from volcanoes, caused him to discard it, and latterly to explain volcanic action by referring it to the supposition of the nucleus of our globe being in a liquid ignited state, and liable to break out through the crust of solid matter which envelopes it. The former, though the less probable hypothesis, has never had but a limited number of supporters, but has been well defended by Dr. Daubeny of Oxford. Sir Humphry Davy left Naples early in Spring, and arrived in London in June, 1819. In the course of the same month the President of the Royal Society, Sir Joseph Banks, died. Davy immediately came forward as a candidate for the honour of succeeding him. He was almost unanimously elected, and for seven years afterwards he was successively re-elected without any opposition.

He appears to have exerted himself to support science by his patronage, by giving entertainments, and having open meetings at his own house, after the plan followed by his distinguished predecessor, who, notwithstanding Davy's

slighting mention of him, occupied the important station of president more successively, and with greater benefit to science than any other individual who ever held it since the time of Newton. It would be invidious to point out the individuals who were encouraged and supported in the prosecution of science by Sir Joseph Banks, but that some of our most eminent men of science were under deep obligations to him is sufficiently well known. It is also certain that the evening meetings at his house continued till his death, while those of Davy were discontinued in 1826. What his motives were for discontinuing them do not appear. In August, 1819, he made an excursion to Scotland, and in October of the same year, directed his attention to the experimental results of Oersted. The first notice of this is contained in a letter to his brother, dated December, "I have ascertained" he observes "(repeating some vague experiments of Oersted) that the *voltaic pile* is a powerful magnet, *i. e.* that by the union of the + and — electricities, magnetism is produced in the same combinations as heat. I am deeply occupied with this, which promises to explain so much for the theory of the earth: do not say any thing on the subject. I hope in two or three days to be able to give you the whole details of which you will immediately perceive the importance." His knowledge of the experiments of Oersted, was first derived at second hand, by a letter from a friend at Geneva.

This important discovery, which consists in the fact that when the extremities of a voltaic pile or battery are united by a perfect conductor, as a metallic wire, and the compass is brought near it, the needle is attracted by the wire and may be made to deviate from its natural direction. This Davy verified, and inferred that the uniting wire must have become magnetic, which he found correct. He found the same magnetic power to be exhibited by the Leyden battery, and conjectured that the magnetism of the earth might be owing to electrical currents. In July, 1821, he published in the Philosophical Transactions, his "Farther Researches on the Magnetic Phenomena produced by Electricity," &c. In this paper he points out some remarkable facts in reference to electrical conduction; he shews that

the higher the intensity of the electricity, the less difficulty it has in passing through bad conductors; that the conducting power of metallic bodies, is lower in some inverse ratio as the temperature is higher, and that the generation of heat by electricity, in the case of metals, is nearly inversely as their conducting powers.

After the close of the session of the society in 1821, he repaired to Ireland, to enjoy his favourite pastime, a country which he never entered without feeling his spirits rise, partly from the kindness of heart, which he always experienced there, and partly from the original and diverting nature of the people.

On his return to London, he occupied himself with scientific research, the results of which were published in the Transactions, under the title of "On the electrical Phenomena exhibited in vacuo." His principal inferences from his experiments are, that electricity and magnetism can exist in the most complete vacuum, their attractions and repulsions in vacuo, being exercised much in the same manner as in the atmosphere, while in reference to heat and light his conclusion was, that they cannot exist in vacuo. During the winter he visited Penzance, where he obtained a hearty welcome, and was honoured by a public dinner at which fifty gentlemen sat down.

During 1822, he communicated to the society a paper "on the state of water and aeriform matter in cavities found in certain crystals." He found the air in these crystals to resemble azote, and the water to be nearly pure. In the course of the summer, he re-visited Scotland, where he spent some time in fishing and shooting amid the highland mountains. At Christmas he visited Wales, and investigated the nature of the effluvia emitted from the great copper works in the neighbourhood of Swansea.

In the spring of 1823, he read his paper to the society, "On a new Phenomenon of Electro Magnetism." The phenomenon referred to those rotatory actions now so familiar to all who have paid any attention to the recent progress of electrical science. He concludes the paper by an act of justice to Dr. Wollaston, pointing out how the discovery of the rotations of the electro magnet wire

round its axis by the approach of a magnet, realised by the ingenuity of Mr. Faraday, had been anticipated and even attempted by Dr. Wollaston in the laboratory of the Royal Institution.

(To be continued.)

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## ARTICLE II.

*On the Formation of Sulphuric Acid.* By THOMAS THOMSON, M. D. F. R. S. L. and E. &c. Regius Professor of Chemistry in the University of Glasgow.

It is well known that sulphuric acid is manufactured in this country by the combustion of sulphur. The sulphurous acid formed is passed into large leaden chambers, where it comes in contact with nitric acid and a small quantity of water; the fumes of the nitric acid being sent into the leaden chamber at the same time with the sulphurous acid. Now, whenever any sloping part occurs in the leaden chamber at some height above the floor which is covered with water, there is a deposit of a white saline matter.

This saline matter is in small scales. It has an excessively acid taste. When exposed to the air it gradually runs into a liquid, which is pure sulphuric acid. When thrown into water a violent effervescence takes place, nitrous gas is given off in abundance, and a solution of sulphuric acid remains. This saline matter has been repeatedly examined. Davy considered it to be a compound of nitric acid and sulphurous acid. Dr. Henry examined it some years ago, and concluded from his experiments that it is a compound of hyponitrous acid and sulphurous acid.

By the kindness of Mr. Tennant I have had repeated opportunities of examining this matter in a state of great purity. I have subjected it to various experiments, and have been led to form a different opinion from that entertained by Dr. Henry of its composition. How far the experiments which I shall detail warrant that opinion, I leave to practical chemists to determine. The analysis is not quite satisfactory, because we cannot determine experimentally the quantity of water present.

1. When a quantity of the saline matter is mixed with water in a retort, a strong effervescence takes place, and nitrous gas escapes in torrents. The whole dissolves in the water, with the exception of a small quantity of white matter, the weight of which varies in different specimens. This white matter when dried is a tasteless powder, insoluble in water. When heated it takes fire, and burns with a blue flame, while some sulphur sublimes. What remains is pure sulphate of lead. These phenomena characterize *sulphite of lead*. Hence, it is evident, that the saline matter from the leaden chambers contains *sulphite of lead*. From 550 grains of saline matter I obtained 8.43 grains of sulphite of lead, or about 1.53 per cent. In another experiment 160 grains of the saline matter yielded 1.4 grains of sulphite of lead, or somewhat under one per cent. These two experiments show the two extremes; in all the others the quantity was intermediate.

2. 58 grains of the saline matter were heated in a small retort. The solid matter became partially liquid and fumes of nitrous acid made their appearance. On increasing the heat an effervescence took place, and gas passed rapidly. It was yellow like nitric acid fumes, and like that acid acted on mercury, which prevented me from collecting the gas. When the effervescence stopped, a colourless liquid remained with a small deposit of sulphite of lead at the bottom of the retort. This liquid was colourless, but it effervesced violently giving out nitrous fumes when mixed with water. It remained, therefore, the same mixture or compound as the original saline matter.

3. When the saline matter is triturated with carbonate of ammonia, combination takes place without any sensible decomposition.

4. It was triturated with a quantity of bi-carbonate of potash in powder, which from previous experiments was judged capable of just saturating the uncombined acids. Fumes of nitric acid were given off till the whole became quite dry. The trituration being continued the mixture softened into a white paste, which was left exposed to the air for some hours. On examining this residue, it was found to consist chiefly of a mixture of sulphate of potash and carbonate of potash with a very little nitrate; the nitric acid had been almost all dissipated during the trituration.

5. 160 grains of the dry saline matter were put into a retort mixed with water and the deutoxide of azote collected as it was extricated. The quantity of this gas evolved, supposing the thermometer at 60° and the barometer at 30 inches was 59·35 cubic inches.

The liquid in the retort being freed from the sulphite of lead, was found to be a solution of sulphuric acid in water, without any trace of nitric or sulphurous acid. This sulphuric acid being obtained partly in the state of sulphate of soda, and partly of sulphate of barytes, amounted to 132·24 grains=105·79 grains of sulphurous acid.

The weight of the nitrous gas obtained was 19·17 equivalent to 34·5 grains of nitric acid.

The constituents obtained were,

Sulphurous acid . . . . .	105·79
Nitric acid . . . . .	34·50
Sulphite of lead . . . . .	1·40
	<hr/>
	141·69
Loss . . . . .	18·31
	<hr/>
	160·

This loss must be water. The constituents then are very nearly

1 atom nitric acid . . . . .	6·75
5 atoms sulphurous acid . . . . .	20·00
3 atoms water . . . . .	3·375
	<hr/>
	30·125

That the acid present is nitric and not hyponitrous I infer from the phenomena of the distillation of the saline matter. And from our knowledge of the fact that nitric acid is actually introduced into the leaden chambers along with sulphurous acid, and there being nothing present to convert it into hyponitrous.

There is no evidence from the analysis that the whole acid of sulphur was in the state of sulphurous acid. I am induced from the proportions found to suspect that  $\frac{2}{3}$ ths of it was in the state of sulphuric acid, and  $\frac{1}{3}$ ths in that of sulphurous acid. On that supposition it is easy to see how the atom of nitric acid, by giving out 3 atoms of oxygen,

converts the 3 atoms of sulphurous into sulphuric acid, while the acid thus decomposed makes its escape in the form of deutoxide of azote.

The preceding analysis was repeated with very nearly the same result. If the supposition of the saline matter containing  $\frac{2}{5}$ ths of sulphuric and  $\frac{3}{5}$ ths of sulphurous acid be admitted, then the constitution of the portion examined must have been

Sulphurous acid . . .	63·87
Sulphuric acid . . .	52·90
Nitric acid . . .	34·50
Sulphite of lead . . .	1·40
Water . . .	7·33
	<hr/>
	160·00

This approaches pretty closely to

3 atoms sulphurous acid . . .	12
2 atoms sulphuric acid . . .	10
1 atom nitric acid . . .	6·75
1 atom water . . .	1·125
	<hr/>
	29·875

Probably the water was in combination with the sulphuric acid.

### ARTICLE III.

*Description of a New Hygrometer; illustrated by experiments and a comparison of its results with Sir John Leslie's, and the Dew-point Hygrometers, by JOHN ABRAHAM MASON, M.D., Member of the Royal College of Surgeons, Edinburgh; Extraordinary Member of the Royal Medical Society, Edinburgh, &c.*

I WILL resume the subject of my former paper by entering into a consideration of the general Law which regulates the action of my Hygrometer.

1. The *final* tension of the vapour given off in the process of evaporation is determined, not by the temperature of the evaporating surface; but by the elasticity of the aqueous atmosphere already existing: the quantity of vapour given off depending upon temperature, while the refrigeration produced by that process depends entirely

upon the tension of the pre-existing atmosphere of aqueous vapour.

When the Hygrometer stands at zero ; that is, when the wet and dry thermometers indicate the same temperature, it may be inferred, whatever the temperature may be, that the atmosphere is completely saturated with moisture ; since water, at the temperature of the surrounding medium, does not evaporate at all when the atmosphere is completely saturated ; and the quantity by weight, a cubic foot will contain, at any given temperature, may be at once ascertained by reference to appropriate tables on the subject : but supposing the atmosphere not completely saturated, an increase of temperature above the point of saturation will allow a certain addition of aqueous vapour ; and evaporation will commence and continue in proportion as the temperature of the air is above that at which saturation would take place : this process must continue either till the temperature of the whole surrounding medium becomes reduced to that which corresponds with the tension of the pre-existing vapour, or the atmosphere becomes fully saturated at the increased temperature, when evaporation would cease, and both bulbs indicate the same temperature, showing that the medium was fully saturated as before.

But, supposing the atmosphere not saturated, or a certain superabundant heat to be maintained, evaporation will go on in proportion to the increase of temperature ; but the refrigeration produced by that process will be governed by the tension of the aqueous atmosphere already existing ; that temperature being attained, refrigeration will cease ; although evaporation continues unimpeded.

The temperature of the moistened bulb will then remain stationary : the heat necessary for converting the moisture applied to the bulb into a state of vapour, being derived immediately from the surrounding medium ; the temperature of the atmosphere regulating the quantity of water raised in vapour, while the tension of the pre-existing atmosphere of vapour determines the limits of refrigeration.

2. There appears to be a diversity of opinion with respect to the source of the heat which is expended in converting a portion of moisture into vapour.

Leslie concluded, that it was supplied entirely by the surrounding medium; Dr. Anderson asserts that it is derived entirely from the water itself; while there are others who maintain that it is at first derived from the water, until refrigeration has attained its maximum intensity, and afterwards from the atmosphere.

Without entering into any discussion respecting the merits of each of these theories, which would be inconsistent with the length of this article; the most rational opinion appears to be that of Sir J. Leslie, namely, that it is derived entirely from the surrounding medium; for were it derived from the water, and no compensation made, there could be no limits to refrigeration so long as moisture was supplied to the bulb, and evaporation continued.

The method by which the moistened bulb of a thermometer becomes reduced in temperature, appears to me to be the following.

Both bulbs while dry continue to receive equal increments of heat from the surrounding medium. We will suppose them both at  $70^{\circ}$  Fahrenheit, when water at the same temperature is applied to one of the bulbs, and the atmosphere not saturated with humidity: the existing vapour having a *final* tension of 0.657, corresponding to a temperature of  $65^{\circ}$ , at which full saturation would take place.

Evaporation will immediately commence: a portion of the water will enter into the state of vapour having a tension of 0.770 corresponding to a temperature of  $70^{\circ}$ , hence the bulb will be reduced in temperature; that portion of atmospheric air which surrounds it will become cooled in like proportion, its heat being expended in converting another portion of water into vapour, and thus its becoming latent will not affect the temperature of the thermometric fluid.

Evaporation continuing, the next portion of fluid converted into vapour will have a less tension corresponding to the reduction of temperature; the air in immediate contact with the bulb will be reduced in temperature in the same proportion, converting another portion of water into vapour.

These two processes will go on, the one gradually de-

creasing until the moistened bulb has become reduced in temperature corresponding to a tension of 0.670, equal to a temperature of 67.5, the mean between 70° and 65°, when the further reduction of temperature will be arrested; the temperature remaining stationary so long as the tension of the pre-existing vapour remains the same. The quantity evaporated will be greater at the commencement of this process; caloric being derived from two sources, namely, from the bulb itself, but chiefly from the surrounding medium; so that the final quantity of water given off will be regulated according to the temperature of the air; and will be constant and uniform. As soon as the maximum degree of cold has been once attained, the film of water may be conceived to act as a perfect non-conductor between the air and the bulb of the thermometer, for instead of transmitting the heat which it receives from the air to the thermometric fluid, it is expended in converting part of that film into vapour, thus, by becoming latent, it does not affect the thermometric fluid, because it is necessarily dissipated along with the vapour into the surrounding atmosphere.

3. *When the instrument is in the shade, the temperature of the moistened bulb, when reduced to its utmost limits of refrigeration by means of a strong current of air, indicates the exact mean between the temperature of the air in the shade and the Dew-point\*.*

The principle which regulates this indication is the following: the urged current of air coming in contact with the moistened bulb, becomes gradually reduced in temperature; its heat becoming latent in the process of evapora-

\* The *Dew-point Hygrometer*, which I have been in the habit of using, is described in Jameson's Journal, July, 1835, by A. Connel, Esq. modified by myself, and made by Mr. William McDowall, Infirmary Street, Edinburgh. It approaches most to Le Roy's method of taking the Dew-point. The instrument is delicate, and when aided by a high magnifying power, the slightest deposition of dew may be observed; but it is even more difficult to use, and requires more time than the beautiful instrument of Professor Daniell, the cold being produced by a mixture of dried muriate of ammonia and nitrate of potash, and the proper addition of water. The chief advantage of the instrument is, that the whole bulb of the thermometer is exposed to the refrigerating medium; the temperature can also be kept stationary for a great length of time; and the deposit of dew is more easily observed on a polished metallic surface than on glass.

J. A. M.

tion, until it has attained its utmost limits, the tension of the vapour given off depending upon the temperature of the evaporating surface, until the further refrigeration is finally checked by meeting with vapour of a similar elasticity existing in the atmosphere.

I think this opinion more consistent, and will better accord with experimental results than the law advanced by Professor Daniell, namely, "The *tension* of vapour given off in the process of evaporation is determined, *not by the temperature of the evaporating surface*, but by the elasticity of the aqueous atmosphere already existing." Professor Daniell afterwards observes that this takes place only within a limited circle, "the term limited circle" implies an exception to a general law, and is therefore objectionable. Now, if the tension of the vapour given off in the process of evaporation depended upon the elasticity of that already existing in the atmosphere, supposing vapour of only one tension to exist, it is by no means clear how the *Dew-point* could ever vary, or why the temperature of the moistened bulb of a thermometer does not correspond with the temperature of the Dew-point in the process of evaporation; when the tension of the vapour at the Dew-point sets limits to the process of evaporation at that temperature; because it is quite clear, that the tension of vapour given off at any higher temperature would overcome that of any other inferior to it.

From my experiments on the subject, I am led to the belief that the tension of the vapour given off depends upon temperature, and that the process of evaporation would be arrested at each temperature, by meeting with a similar force of vapour already existing in the atmosphere, provided the atmosphere was fully saturated at that temperature. Hence, it follows that the moistened bulb of a thermometer cannot be reduced to the temperature of the Dew-point, as it undoubtedly would have been, had vapour of only one tension existed; but that it will stand at a temperature corresponding to the *mean tension* of every force of vapour existing from that of *the air* to the *Dew-point*.

As this is an important point, I made repeated experiments for the purpose of verifying its accuracy, three of which I may particularize. The density of the atmosphere

was very different in each experiment, which will also prove that no barometrical corrections will be required in ascertaining the Dew-point.

Experiment 1st.

Barometer 29.5 inches.

Temperature of the shade 57 correction  
 Temperature of wet bulb 54 difference  $3 + .5 \times 2 = 7$  absolute dryness on the thermometric scale.  
 Temperature of the shade  $57 - 7 = 50^\circ$  Dew-point.  
 Temperature of the shade  $57 - 3.5 = 53.5$  lowest limits of refrigeration corresponding to a tension of .42067.  
 Ascertained Dew-point  $50^\circ$ .

Temp. of shade. Force of vapour.

50 . . . .	.37345
51 . . . .	.38640
52 . . . .	.39977
53 . . . .	.41356
54 . . . .	.42779
55 . . . .	.44249
56 . . . .	.45764
57 . . . .	.47328

3.37438

———— = .42179 mean tension.

8 .42067 } tension corresponding  
 to  $53^\circ.5$ .

Difference . . . .00112

Experiment 2nd.

Barometer 28.4 inches.

Temperature of the shade 51.0 correction  
 Temperature of wet bulb 47.5 diff.  $3.5 + .583 \times 2 = 8.166$  absolute dryness on the thermometric scale.  
 Temperature of the shade  $51 - 8.166 = 42.834$  Dew-point.  
 Temperature of the shade  $51 - 4.083 = 46.917$  lowest limits of refrigeration corresponding to a tension of .33630.  
 Ascertained Dew-point  $42^\circ$  Fahrenheit.



invariably corresponded\* to the temperature of the moistened bulb when reduced to its lowest limits by means of a strong current of air; the local air immediately in contact with the wet bulb becoming saturated before the utmost limits of cold has been attained, and thus, preventing mechanically the contact of the surrounding atmosphere with the bulb; by which means the utmost limits of refrigeration are impeded, which air at a certain dryness would ensure, did not this mechanical impediment prevent it.

The experiments which I gave in my last paper prove, that for each degree of humidity, the temperature of the moistened bulb can only be lowered a certain limit, and the constancy of this process, at every degree of humidity will be seen in the following table:

Table of corrections, to be made use of in ascertaining the dew-point by means of the wet bulb hygrometer.

When the instrument is free from a strong current of air, the number opposite to the degree of dryness is to be added to that degree, which will then show the utmost limits of refrigeration under those circumstances.

This table shows the number of degrees the moistened bulb of a Thermometer becomes reduced in temperature by a strong current of air, at each degree of dryness indicated by the Hygrometer, when free from air in motion, in order to reduce it to its lowest limits of refrigeration.

Deg. of dryness observed.	Excess of refrigeration to be added.	Deg. of dryness observed.	Excess of refrigeration to be added.	Deg. of dryness observed.	Excess of refrigeration to be added.	Deg. of dryness observed.	Excess of refrigeration to be added.
0	0.0	6.0	1.000	12.0	2.000	18.0	3.000
0.5	0.083	6.5	1.083	12.5	2.083	18.5	3.083
1.0	0.166	7.0	1.166	13.0	2.166	19.0	3.166
1.5	0.2495	7.5	1.2495	13.5	2.2495	19.5	3.2495
2.0	0.333	8.0	1.333	14.0	2.333	20.0	3.333
2.5	0.4165	8.5	1.4165	14.5	2.4165	20.5	3.4165
3.0	0.500	9.0	1.500	15.0	2.500	21.0	3.500
3.5	0.583	9.5	1.583	15.5	2.583	21.5	3.583
4.0	0.666	10.0	1.666	16.0	2.666	22.0	3.666
4.5	0.7495	10.5	1.7495	16.5	2.7495	22.5	3.7495
5.0	0.833	11.0	1.833	17.0	2.833		
5.5	0.9165	11.5	1.9165	17.5	2.9165		

\* I think it right to state that in the preceding experiments the variations for the force of vapour amount only to  $-0.0112$  in the 1st. Experiment, to  $+0.0358$  in the second, and to  $+0.0442$  in the third; which are too trifling to affect the comparison, and probably are to be attributed to errors in making the observations.

4. Having detailed the mode by which the temperature of the moistened bulb is reduced, I will proceed to state the method by which the *Dew-point* may be obtained by my Hygrometer.

Rule. —To the difference of temperature indicated between the dry and wet bulbs; add the correction necessary to produce the lowest limits of refrigeration at that degree of dryness in the preceding table; multiply the product by two, and subtract that result from the temperature of the shade, the remainder will be the *Dew-point*.

Example { 1, Dr. Mason's Hygrometer,  
 Temperature in the shade 57.  
 Temperature of moistened bulb 54 dif. 3 + <sup>correction.</sup> .5 = 3.5  
 $\times 2 = 7$  degrees of absolute dryness.  
 Temperature of shade 57 — 7 = 50 Dew-point.  
 2, Dew-point Hygrometer.

Temperature of the Dew-point ascertained by the *Dew-point Hygrometer* above referred to 50° Fah.

Temperature of shade 57.

Dew-point . . . 50 dif. 7 degrees of absolute dryness on the thermometric scale.

I will now give a comparison between the action of my Hygrometer and those of Professor Daniell and Sir John Leslie.

a. With respect to Professor Daniell's instrument, I may observe, that the observation must be made in the shade, or the results will be inaccurate, and a fallacious dryness be indicated; for although the *Dew-point* under all circumstances will be the same, the thermometer in the stem of the instrument will be proportionally higher or lower, according to the care taken to protect it from the different sources of radiation and reflection.

This circumstance is particularly applicable in Madeira and in tropical climates, where there is an almost impossibility of taking an observation in the shade, or free from the influence of radiant caloric given out from the various buildings.

In Madeira I have almost invariably found, that between

a thermometer placed in the shade, and another suspended in an ordinary room, free from the sun's direct rays, there are from four to five degrees difference.

When Professor Daniell's Hygrometer is employed in Madeira, the temperature of the air ought to be taken, not from the thermometer attached to the instrument, but from another thermometer, protected as much as possible from every source of radiant or reflected light and heat.

I think it right to state these facts, in order to inform the inexperienced operator, that even the best instruments will give erroneous data, provided every circumstance be not considered when making observations with the same instrument in different climates, for the only sure method by which one climate can be compared with another, is to make the experiments under precisely similar circumstances.

In my Hygrometer, as in all Dew-point Hygrometers, it is absolutely essential to observe great accuracy in taking the temperature of the *shade*; because an incorrect indication of the shade will necessarily lead to erroneous results in obtaining the humidity by both instruments.

It is, however, to be remarked that when both descriptions of Hygrometers are employed, the one will correct the other; the Dew-point Hygrometer giving greater dryness, and mine giving greater humidity than really exists in the atmosphere, each in equal ratio: so that the mean between the results of both instruments will indicate the actual condition of the atmosphere.

It is important to know this fact when exact results are required, and there is difficulty in obtaining with precision the true temperature of the shade, as often happens in southern latitudes.

If the degree of dryness on the thermometric scale be alone required no inaccuracy can result in my Hygrometer, even when the temperature of the shade is incorrectly taken, because the temperature does not affect its indication in this respect; whereas the Dew-point Hygrometer cannot possibly possess the same advantages, because if an error is committed in ascertaining the temperature of the shade, it will give a dryness exceeding the real condition of the air by

exactly as many degrees as the obtained temperature exceeds the absolute temperature of the shade.

The degree of dryness on the thermometric scale will be also equally increased, and thus indicate a fallacious dryness.

b. The comparative merits of the Hygrometer by evaporation now proposed and Sir John Leslie's may be seen by the following experiments:—

When both instruments were in perfect shade, and free from currents of air, my Hygrometer registered one-sixth of Sir John Leslie's; but it is only when Leslie's Hygrometer is in *perfect shade and free from currents of air* that this relation exists between the instruments; indeed the greatest nicety is required to make correct observations with Sir John Leslie's Hygrometer as these experiments will prove.

Experiment 1.—6 A. M. Both instruments in the shade and free from currents of air.

Temperature of shade  $66^{\circ}$ .

Moistened bulb . .  $62\frac{1}{2}^{\circ}$ --difference  $4\frac{1}{2}^{\circ} \times 6 = 27^{\circ}$ .

Leslie's Hygrometer  $27^{\circ}$ .

Mason's Ditto . .  $27^{\circ}$ . no difference indicated.

Experiment 2.—8 A. M. Windows and doors closed; instruments in the shade: the sun's rays fall upon one of the white shutters, and are reflected upon the instruments at three feet distance.

Temperature of air  $68^{\circ}$ .

Moistened bulb .  $62^{\circ}$ . difference  $6^{\circ} \times 6 = 36^{\circ}$ .

Leslie's Hygrometer  $45^{\circ}$ .

Mason's Ditto . .  $36^{\circ} - = 9^{\circ} +$  Leslie's

Giving a false indication of nine degrees from the reflection of the sun's rays from the white window shutter upon the instrument.

Experiment 3. Half-past 8 A. M. More of the shutter exposed to the sun's rays.

Temperature of the air .  $68^{\circ}$ .

Ditto of the moistened bulb  $62\frac{1}{2}^{\circ}$ , difference  $5\frac{1}{2}^{\circ} \times 6 = 33^{\circ}$ .

Leslie's Hygrometer  $46\frac{1}{2}^{\circ}$ .

Mason's Ditto . .  $33^{\circ} - = 13\frac{1}{2}^{\circ} +$  Leslie's

Showing a variation of  $13\frac{1}{2}$  degrees from reflection.

Experiment 4.—9 A. M. Instruments as in the first experiment.

Temperature of the air  $68^{\circ}$ .

Ditto of moistened bulb  $61^{\circ}$ . difference  $7^{\circ} \times 6 = 42^{\circ}$ .

Leslie's Hygrometer  $42^{\circ}$ .

Mason's Ditto .  $42^{\circ}$ , no difference indicated.

Experiment 5.—Half-past 9 A. M. Window shutters open. Sun's direct rays not upon the instruments; but much reflected light from different parts of the room.

Temperature of air .  $72^{\circ}$ .

Ditto of moistened bulb  $65^{\circ}$ , difference  $7^{\circ} \times 6 = 42^{\circ}$ .

Leslie's Hygrometer  $50^{\circ}$ .

Mason's Hygrometer  $42^{\circ} - = 8^{\circ} +$  Leslie's.

A variation of 8 degrees for reflected light.

Experiment 6.—10 A. M., Instruments in the shade; doors and windows open; strong current of air upon the Hygrometers.

Temperature of air . . . . .  $71^{\circ}$ .

Temperature of moistened bulb  $63^{\circ}$  difference  $8^{\circ} - 0.666$ . Correction for current of air  $= 7^{\circ} \cdot 334 \times 6 = 44^{\circ} \cdot 094$ .

Leslie's Hygrometer  $58^{\circ} \cdot 000$ .

Mason's Hygrometer  $44^{\circ} \cdot 004 - = 13^{\circ} \cdot 996 +$  Leslie's.

Giving a false indication of  $13^{\circ} \cdot 996$  degrees for current of air.

Experiment 7.—Noon: instruments as in the first experiment.

Temperature of air . . . . .  $70^{\circ}$ .

Temperature of moistened bulb  $62^{\circ}$  difference  $8^{\circ} \times 6 = 48^{\circ}$ .

Leslie's Hygrometer  $48^{\circ}$ .

Mason's Hygrometer  $48^{\circ}$ , no difference indicated.

Experiment 8.—1 P. M., Window shutters open; the direct rays of the sun fall upon both instruments.

Temperature of air . . . . .  $77^{\circ}$ .

Temperature of moistened bulb  $69^{\circ}$  difference  $8^{\circ} \times 6 = 48^{\circ}$ .

Leslie's Hygrometer  $103^{\circ}$ .

Mason's Hygrometer  $48^{\circ} - = 55^{\circ} +$  Leslie's.

Being a variation of 55 degrees, produced by the direct rays of the sun falling upon the instrument.

Experiment 9.—3 P. M., Instruments as in experiment 1st.

Temperature of air . . . . .  $68^{\circ}$ .

Temperature of moistened bulb  $61^{\circ}$  difference  $7^{\circ} \times 6 = 42^{\circ}$ .

Leslie's Hygrometer 42°.

Mason's Hygrometer 42°, no difference indicated.

Experiment 10.—4 P.M., Leslie's Hygrometer in the shade 40°; window shutters open so that the direct rays of the sun fell upon the instrument. It descended to 106 degrees in ten minutes, being a variation of 66 degrees under the same state of dryness.

Those experiments were conducted with much care in a large room having two windows on the same side, one of which remained open during the whole of the experiments.

From these experiments it will be perceived that Sir John Leslie's Hygrometer varied from 8 to 55 degrees, whereas the Hygrometer by evaporation now proposed, under the same circumstances, varied only 0.666 of a degree.

The above results were obtained in Madeira; a second Hygrometer by evaporation was kept in the shade during the time to compare with the other.

I regret I was not able to procure in the island two of Leslie's Hygrometers, as the comparison may be thought not so accurate; but in all those variations, when the causes of disturbance were removed, the Hygrometer always indicated the same relative difference with my own, as the observations at 6 A.M., 9 A.M., noon, and 3 P.M., sufficiently prove.

The following Table will enable the observer, at one glance, to ascertain the equivalent indications afforded by the three kinds of Hygrometers I have mentioned in my paper, namely, the Dew-point Hygrometer, Sir John Leslie's, and my own, which I have calculated for the purpose.

I have given the several results obtained by my own experiments as far as I have carried them, and they may be easily extended; I may also add that the whole experiments have been made from 40° to 90° of Fahrenheit; my experiments at lower temperatures are not yet sufficiently extensive, and may form the subject of a future communication.

To find by this Table the *Dew-point*, corresponding to the degree of dryness indicated by my Hygrometer, it is

only necessary to subtract the absolute dryness shown in the second column of the Table (headed, *Dew-point Hygrometer*) from the temperature of the shade. ^

Table, shewing the equivalent indications of Sir John Leslie's and the Dew-point Hygrometers, corresponding to every degree of Mason's Hygrometer.

Mason's Hygrometer.	Dew-point Hygrometer.	Leslie's Hygrometer.	Mason's Hygrometer.	Dew-point Hygrometer.	Leslie's Hygrometer.	Mason's Hygrometer.	Dew-point Hygrometer.	Leslie's Hygrometer.
0.0	0.0	0	8.0	18.666	48	16.0	37.332	96
0.5	1.166	3	8.5	19.833	51	16.5	38.499	99
1.0	2.332	6	9.0	21.000	54	17.0	39.666	102
1.5	3.499	9	9.5	22.166	57	17.5	40.833	105
2.0	4.666	12	10.0	23.332	60	18.0	42.000	108
2.5	5.833	15	10.5	24.499	63	18.5	43.166	111
3.0	7.000	18	11.0	25.666	66	19.0	44.332	114
3.5	8.166	21	11.5	26.833	69	19.5	45.499	117
4.0	9.332	24	12.0	28.000	72	20.0	46.666	120
4.5	10.499	27	12.5	29.166	75	20.5	47.833	123
5.0	11.666	30	13.0	30.332	78	21.0	49.000	126
5.5	12.833	33	13.5	31.499	81	21.5	50.166	129
6.0	14.000	36	14.0	32.664	84	22.0	51.322	132
6.5	15.166	39	14.5	33.833	87	22.5	52.499	135
7.0	16.332	42	15.0	35.000	90			
7.5	17.499	45	15.5	36.166	93			

The length of this article prevents me from entering into a detailed account of the extensive use which may be made of the instrument I have submitted to the public; since by knowing the temperature of the shade, both the relative and absolute humidity of the air can be easily ascertained by very simple calculation. Sir John Leslie has well observed, that, "The Hygrometer is an instrument of the greatest utility, not only in meteorological observations, but in aiding domestic economy, in regulating many processes of art, and in directing the purchase and selection of various articles of produce. It will detect, for instance, the dampness of an apartment, and discover the condition of a magazine, of an hospital, or of a sick-ward. Most warehouses require to be kept at a certain point of dryness, which is higher or lower according to the purposes for which they are designated. The printing of linen and cotton is carried on in very dry rooms; but the operations of

spinning and weaving succeed best in air which rather inclines to dampness. The manufacturer is at present entirely guided by observing the effects produced by stoves, and hence the goods are often shrivelled, or otherwise injured before he can discern any alteration in the state of the medium. Wool and corn have their weight augmented, sometimes as much as 10 or even 15 per cent. by the presence of moisture, &c."

I will conclude by briefly enumerating some of the more prominent purposes to which this Hygrometer may be applied.

1st. For meteorological purposes generally.

2nd. For medical uses, to regulate the dryness of rooms, hospitals, &c.

3rd. For regulating the dryness or humidity of certain manufactories; when a constant and particular state of dryness or humidity is required in the preparation of many articles of commerce; and especially in numerous chymical and pharmaceutical operations.

4th. In ascertaining the quantity of moisture absorbed by grain, wool, &c., where the weight of those articles is increased by absorbing moisture; by keeping them in damp situations; for fraudulent purposes, or otherwise.

5th. In regulating the dryness of hot-houses, green-houses, &c.

6th. In the navy and merchant service, when used in conjunction with the Barometer, or Sempysometer, to ascertain with more certainty the probability of severe gales, storms, &c., an advantage hitherto, unhappily, beyond the reach of seamen, since both Mr. Daniell's and Sir J. Leslie's Hygrometers have been comparatively useless on board ship.

The combined use of a simple and convenient Hygrometer, such as I have proposed, and acting with a fluid always at hand, will be of great utility in removing the objections raised by naval men to the Barometer, as it will enable them readily to determine, whether a fall in the mercurial column in the barometer indicates the approach of rain or wind, which the Barometer alone will not do. Thus if a fall of the Barometer is accompanied by the indication of a relative degree of dryness on my Hygrometer,

wind alone may be looked for; whereas, if the Hygrometer at the same time approaches to zero, or the point of saturation, rain, or rain with wind, may be expected to follow.

Its adaptation as a weather-glass, hence its great utility to the agriculturist and invalid, will form the subject of a future communication.\*

Sir, I am yours with sincere esteem,

JOHN ABRAHAM MASON.

18, Claremont Place, Pentonville,  
December 9th, 1835.

To Dr. R. D. Thomson.

ARTICLE IV.

*Catalogue of Plants collected at Bombay.*

BY JOHN GRAHAM, ESQ.

(Continued from page 40.)

- |                               |                          |
|-------------------------------|--------------------------|
| 59. <i>Barleria pronilis.</i> | } Common on waste lands. |
| 60. ,, <i>longifolia.</i>     |                          |
| 61. ,, <i>cristata</i> †.     |                          |
| 62. <i>Bombax pentandrum.</i> |                          |

63. ,, *heptaphyllum.* The first I have only seen in gardens; the latter is a very common tree. Both are deciduous, and the numerous large glowing red flowers of the latter make a very showy appearance when the tree is totally destitute of leaves. February and March are its flowering months. The cotton, I believe, which it produces is of no value.

64. *Butea frondosa.* The immense clusters of red coloured pea flowers which this tree produces, have also a very showy appearance—they come before the leaves,—independent of the flowers, the tree has nothing to recom-

\* Makers: Mr. Squire, 277, Oxford Street; Mr. Cary, 181, Strand, London; and Mr. William McDowall, 18, Infirmary Street, Edinburgh.

† I have picked specimens of this plant in Dane's Island, Whampoa, China. A very good figure of it is given in Osbeck's voyage to China—a work which those who write on the botany of that country should not fail to consult.—Edit.

mend it in the way of beauty. It is not very common; several grow in Elephanta.

65. *Butea superba*. A very strong climber, with far more splendid flowers. It grows on Salsette—rare.

66. *Bryonia grandis*.

67. „ *scabra*.

68. *Borassus flabelliformis*. Tall Palmyra tree; common.

69. *Borago Indica*. A very common annual springing up in the rains.

70. *Bignonia Rheedii*. I have only seen one tree. The flowers grow on a scape five or six feet long, and give the tree a curious appearance at a distance.

71. *Canna Indica*.

72. *Costus speciosus*. Found it on a hill near Wuzaum Poona road.

73. *Curcuma montana*. Very common on the top of the Ghauts. A species of arrow-root is made from it.

74. *Cissus vitiginea*.

75. „ *carnosa*.

76. „ *4-angularis*.

77. „ *ripanda*.

78. „ *crenata*.

79. *Convolvulus speciosus*. Elephant creeper.

80. „ *batatas*. Extensively cultivated.

81. „ *turpithum*.

82. „ *grandiflorus*.

83. „ *paniculatus*.

84. „ *pes-caprae*\*.

85. „ *tigridis*.

86. „ *muricatus*. There are several other species of *Convolvulus* common, but I have not been able to identify them.

87. *Coffea Arabica*. In gardens only.

88. *Capsicum annum*. Commonly cultivated in gardens.

89. „ *frutescens*. Ditto.

90. *Cocculus cordifolius*.

91. *Cicer arietinum*. Extensively cultivated in the

\* This fine creeper occurs abundantly on the shore by the race course of Macao in China, occupying the place of the *C. Soldanella* of the Scottish coast.  
—Edit.

Deccan and Guzurat. The grain plant. Horses are fed with it instead of corn.

92. *Celosia margaritacea*. An annual, springing up every where in the rains.

92. *Carissa Carandas*. Curwund of the Natives; a very common shrub strongly armed, and producing black berries about the size of a sloe, which are eaten raw, or made into jellies, &c.

93. *C. spinarum*. Berries red. This species I have only seen in gardens.

94. *Cerbera Thevetia*. Only in gardens.

95. *Ceropegia tuberosa*. Very rare, I have only once seen it on Malabar Hill.

96. *Crinum asiaticum*.

97. *Cardiospermum Halicacabum*.

98. *Cassytha filiformis*. Common in jungles.

99. *Cassia Fistula*. Elephanta and Salsette.

100. ,, *Sumatrana*. In gardens only.

101. ,, *auriculata*. Very common in the sterile parts of Deccan.

102. *Cochlospermum Gossypium*. } in gardens.

103. ,, *serratifolium*. }

104. *Coreopsis tinctoria*. Grown in pots, &c., as an ornamental plant.

105. *Crataeva religiosa*. Commonly to be found in the neighbourhood of temples.

106. *Cactus Ficus indica*.

107. *Calyptanthus caryophyllata*. Native name Jamb; the fruit is eaten.

108. *Capparis Zeylonica*.

109. ,, *trifoliad*, or *Crataeva religiosa*.

110. ,, *sepiaria*.

111. ,, *acuminata*.

112. *Calophyllum Inophyllum*. A very pretty tree, common in the Concan and Malabar. Oil is expressed from the seeds and used for lamps by the poorer classes of natives.

113. *Corchorus acutangulus*. Annual; common in the rains.

114. *Clerodendrum Siphonanthus*. In gardens only.

115. ,, *infortunatum*.\*

\* This plant occurs in Danes' I. China.—E. DIT.

116. *Clerodendrum fragrans* in gardens.  
 117. *Cleome 5-phylla*.  
 118. „ *viscosa*.  
 119. *Crotalaria verrucosa*,  
 120. *Clitorea Ternalea*.  
 121. *Citrus Decumana*. Pummalo or shaddock, commonly cultivated.  
 122. *Citrus Aurantium*.  
 123. „ *Limetta*.  
 124. *Cacalia sonchifolia*.\*  
 125. *Chrysanthemum Indicum*.  
 126. *Cadsuarina muricata*. Common in Bombay, where it is planted for ornament. It shoots up very rapidly.  
 127. *Coix Lachryma*.  
 128. *Cicca disticha*. Fruit sometimes used for tarts.  
 129. *Cocos nucifera*.  
 130. *Caryota urens*. This beautiful palm grows plentifully on the Ghauts.  
 131. *Croton variegatum*. This has obtained the name of laurel, and is very commonly grown in pots. The temporary bungalows on the Esplanade are surrounded with it to keep out the glare of the sun. The *C. Tiglium* grows in Guzurat. I have never seen it.  
 132. *Cynanchum extensum*. A common twining plant.  
 133. *Cucurbita Citrullus*.  
 134. „ *hispida*.  
 135. „ *lagenaria*. The melon and cucumber family are very generally cultivated, and form a common article of food with the natives.  
 136. *Cucumis sativus*.  
 137. „ *Colocynthis*. In the Deccan.  
 138. „ *Melo*.  
 139. „ *acutangulus*.  
 140. „ *Citrullus*.  
 141. „ *Maderaspatanus*.

\* This plant is also a native of China. I have found it abundantly on a rocky point W. of Danes Island village, Whampoa, and also on the opposite side of the river Tigris. The correspondence of the Flora of Malabar and China is very striking, but the present catalogue shews that the same observation does not apply to the Concan coast.—EDIT.

142. *Cylista scariosa*. Scarce.
143. *Cannabis sativa*. An intoxicating liquor called Bhang is prepared from it.
144. *Cycas circinalis*.
145. *Carica Papaya*. Generally cultivated.
146. *Cassandra undulæfolia*.
147. *Carthamus tenebrans*.
148. *Caesulia axillaris*.
149. *Combretum decandrum*.
150. *Conyza cinerea*.
151. *Cordia Myxa*. A tree much resembling the alder. Fruit sometimes pickled.
152. *Cordia angustifolia*.
153. *Coronilla grandiflora*. Natives commonly plant this tree about their houses. It has large showy flowers and is of very quick growth.
154. *Ceanothus Zeylonica*. Elephantia.
155. *Celtis orientalis*.
156. *Caesalpinia pulcherrima*.
157. *Capparis aphylla*. Common in the barren lands of Deccan.
158. *Careya arborea*. I have seen only one tree on Malabar hill.
159. *Casearia elliptica*.
160. *Chloris barbata*.
161. *Cyperus rotundus*.
162. *Cynosurus indicus*.
163. *Callicarpa lanata*.
164. *Celastrus montana*.
165. *Cynometra cauliflora*. In gardens scarce.
166. *Cookia punctata*.
167. *Cyperus dubius*.
168. „ *compressus*.
169. *Commelina communis*.
170. *Cleome icosandra*.
171. *Cissampelos convolvulacea*.

(To be continued.)

## ARTICLE VI.

*Of the Chemical Composition of Human Blood.* By Mr. THOMAS RICHARDSON.

THE apparent solidity of the whole of the blood after coagulation is only a deception, for as the truly solid portion, which is called the *clot*, contracts, a liquid, termed *serum*, is forced out upon the surface, and if it be allowed to stand for a few hours, this greenish yellow fluid may be drawn off.

The proportion between the clot and serum varies very much in different animals, and even in the same animal in different circumstances. Dr. Thomson states, that the following may be taken as the mean in healthy human blood.

Serum . . . . .	55
Clot . . . . .	45
	—
	100*

Dumas and Prevost, after numerous experiments upon this subject, find in the following instances

	Man.	Horse.	Tortoise.	Chicken.	Trout.
Water . . . . .	7839	8183	7688	7970	8637
Particles . . . . .	1242	920	1506	1466	638
Serum . . . . .	869	897	806	564	725

These gentlemen have also found more particles in the arterial than in the venous blood. In a comparative analysis of the arterial and venous blood of a sheep they obtained as follows :

	Arterial.	Venous.
Water . . . . .	8293	8364
Particles . . . . .	935	861
Albumen and salts	772	775†

Muller has confirmed these results; since it appears from his experiments, that the arterial blood of the goat contained 0·483 and venous blood ·395 per cent. of dissolved fibrin. ‡

Lucanu, from several experiments upon the blood of individuals of different sexes, age, and temperament, draws

\* Phil. Mag. xi., p. 349. † Ann. de Ch. et Ph. xiii., p. 64.

‡ Pogg. Annalen, xxx., 313., and Records ii. 16.

the following conclusions respecting the proportion of the constituents of the blood :

The *water* varies ; it is less in man than in woman. The quantity is not proportional to the age, beyond the limits of twenty to sixty years. In individuals of the same sex, it is less in those of a sanguine temperament, than in those of a lymphatic.

The *albumen* is sensibly the same in both sexes, and is proportional to the age, within the limits of twenty to sixty years. The temperament does not appear to affect the quantity.

The proportion of *globules* is greater in man than in woman ; it is greatest also in those of the sanguine temperament.

The *serum* is greater in proportion in woman than in man, and also in the lymphatic than in the sanguine temperament.\*

From the experiments of Mr. Andrews, the quantity of globules and albumen seems also to decrease from repeated bleedings.†

#### I. THE CLOT.

The clot is of a red colour and soft, and possesses a specific gravity of 1.126, as determined by Haller. To separate it from the serum, I followed the plan proposed by Dumas and Prevost.

The weight of the whole blood is taken, and the serum separated as well as possible by a sucker. The serum is then weighed, which, subtracted from the weight of the blood, gives the weight of the clot. Weighed portions of the serum and clot are then carefully evaporated to dryness, and what remains is weighed. The water in the clot, being in the state of serum, its quantity is easily determined from the above data.

After these preliminary experiments, the remaining clot is cut into small pieces and heated with water, till all saline matter is washed away.

This pure clot is mingled with water, and the whole often agitated. The water dissolves the hematosine, or colouring principle, and leaves the fibrin.

\* Ann. de Ch. et Ph. xlviij., 321.

† Records, i., p. 31.

1. *Fibrin.*

Fibrin, procured as above, has the appearance of soft masses formed of filaments interlaced with each other, which has been conceived by some to be similar to muscular fibre: Its bulk compared with the clot is much smaller, and in this state is heavier than water. To render the fibrin completely pure, according to Berzelius, it ought to be digested in alcohol or ether, to separate a quantity of fatty matters with which it is always united.

By desiccation, fibrin loses about  $\frac{3}{4}$  of its weight, becoming a little yellowish, hard, and brittle, on drying. When the fatty matter has been separated, it does not acquire any transparency, as was noticed by Berzelius. It has neither taste nor smell. By gently heating fibrin in water it regains its original appearance. By heat it is decomposed, fusing and burning with a brilliant flame, and leaving a shining charcoal behind. When exposed to destructive distillation it yields water, and the usual products of animal substances.

The charcoal is consumed with difficulty, caused by the phosphate of lime coating the surface with a glassy matter. The cinder left after burning is of a whitish grey colour and a half liquid consistency; about  $\frac{2}{3}$  per cent. of the original weight of the dry fibrin according to Berzelius. This chemist found that it leaves traces of silica when dissolved in muriatic acid. Its chief constituent is phosphate of lime.

The fibrin from the blood of oxen is much more difficult to burn than that from man. This arises probably from the former containing more phosphate of lime than the latter. Before the incineration of the fibrin these substances cannot be obtained by the action of acids\*.

After coagulation the fibrin is insoluble in cold or hot water. By long continued boiling, however, it changes its appearance and becomes soft. During the boiling it evolves no gas, but the solution becomes milky. Evaporating the solution to dryness, there remains a solid mass behind, brittle, of a slight yellow colour, and possessed of an agreeable flavour of boiled meat. It is soluble in water, but the solution is not similar to that of gelatine; it

\* Berzelius' *Chimie*, vii., 35.

does not gelatinize after concentration and is precipitated by nitrate of mercury and acetate and nitrate of lead\*.

This matter is supposed to give the specific flavour to the flesh of different animals, and especially to the part which forms the brown crust on roast meat. Thenard has given it the name of *osmazome*.

What remains from the fibrin loses all the characters of this substance; it no longer forms a jelly with acids or alkalies and is insoluble in acetic acid.

Dr. Rainy also found that when fibrin is dissolved in a solution of common salt, the greater part coagulates by heat, but there remains a minute portion of animal matter in solution very similar to that which has just been noticed.†

The manner in which acids and alkalies act upon fibrin, would lead to the opinion that it performs the part of an acid as well as a base.

1. Strong sulphuric acid acts upon pure and dry fibrin, which swells up into a yellow mass, absorbing the acid, but not dissolving. During the action considerable heat is evolved, which, when very great, according to Berzelius assists in the mutual decomposition of the two bodies. When this yellow substance is washed it becomes less bulky, and is a neutral compound of fibrin and sulphuric acid, as was noticed by Berzelius.

2. Nitric acid changes the colour of the fibrin to yellow. If the acid is strong the fibrin becomes of a lemon colour, which does not dissolve in water but changes to an orange. When allowed to remain in the water for some time, with a little heat, an oily looking substance appears on the surface, which agrees with Dr. Thomson's observation that fibrin is sometimes converted into fat by the action of nitric acid.

The yellow coloured mass Berzelius regards as a compound of fibrin slightly altered and malic and nitric acids.

3. Muriatic acid dissolves the fibrin and forms a fine deep blue coloured solution, in which water produces a white precipitate. This matter is a neutral compound of muriatic acid and fibrin.

4. The action of caustic soda upon the fibrin was perfectly similar to that of potash as observed by Berzelius.

5. Besides the salts enumerated by Dr. Rainy, I found

\* Berzelius.

† Records, vol. ii. p. 365.

that carbonate of ammonia has the same effect, as those found by that gentleman.\*

## 2. *Colouring matter or Hematosine.*

Various methods have been proposed for obtaining this substance in a state of purity, but all of them are liable to some objections. That which I followed, suggested by Dr. Thomson, affords the hematosine as pure as, perhaps, it can be obtained by any of the methods known.

The clot cut into very small pieces is agitated with six or seven different portions of water, till as much of the saline matter is washed away as possible. The remaining clot is then digested in the cold, in distilled water, for forty-eight hours, when a strong solution of the hematosine is obtained.

The solution has the disagreeable smell and taste of blood. It is quite dark coloured, but on adding water its red colour may be seen.

The solution may be evaporated to dryness without coagulating. When quite dry it is quite black, but in thin layers is deep red; its lustre is like jet, and the specific gravity in this state 1.2506.

Berzelius states the coagulating point of the hematosine to be  $158^{\circ}$ , and Lecanu found the solution to become muddy at  $149^{\circ}$ , and completely coagulated at  $158^{\circ}$ . The following experiments were made to determine this point. A wide mouthed phial, filled with mercury, was heated on the sand-bath to  $130^{\circ}$ , and a glass tube containing the hematosine plunged into the midst of the mercury; through a cork, which filled the open end of the tube, a thermometer was fixed, and the following observations made:—

At $140^{\circ}$	. . .	Solution quite clear.
150°	. . .	Slightly opaque.
155°	. . .	More opaque.
158°	. . .	Flocks apparent.
159°	. . .	Same as last.
160°	. . .	Quite coagulated.

This experiment was repeated three times, with the mercury heated at the commencement to  $170^{\circ}$ , but the same results were obtained.

With re-agents a moderately strong solution of the hematosine presents the following characters:—

\* Records, ii. 368.

Alcohol, red flocky precipitate.

Infusion of nut-galls, reddish brown precipitate.

Lime water, ditto.

Barytes water, ditto.

Acetic acid, colour becomes brown.

Sulphuric acid, brown precipitate soluble in excess of acid.

Muriatic acid, ditto.

Prussiate of Potash, ditto.

Dichloride of mercury, flocky precipitate.

Acetate of lead, ditto.

When a current of chlorine gas is passed through a strong solution of the hematosine, no effect is produced for some time, but as the bubbles of gas appear on the surface of the solution, they are covered with a white fatty looking matter. Continuing the process, the colour at last completely disappears, and a very dense white precipitate is formed.

It may be separated by a filter and washed.

During the drying it emits a very strong smell of hydrocyanic acid.

It has a yellowish colour.

Is insoluble in water or ether.

When dry it is only partially soluble in alcohol, from which it is precipitated by warm water. While moist and with a faint smell of chlorine, it dissolves readily in boiling alcohol. On evaporating the solution to dryness nearly the whole volatilizes.

Caustic soda changes the colour to a deep yellow, and then dissolves the whole. The colour of the solution is brown, and the substance appears to be precipitated unaltered on the addition of an acid.

Muriatic acid has no action upon it, while it dissolves in sulphuric with great facility.

Dissolves in common nitre with effervescence.

The solution on evaporation to dryness leaves a viscid yellow mass, in appearance not unlike gelatine. It is partially soluble in water, in which solution extract of nut-galls causes a precipitate. The portion insoluble in water while warm is very elastic, similar to caoutchouc, but on cooling becomes quite hard.

Acetic acid by long continued digestion appears to dis-

solve the most of this substance. When the hematosine is coagulated it becomes a clotty red mass, which, according as it is heated, exhales a very peculiar odour.

1. Sulphuric acid dissolves the coagulated hematosine with the application of heat. When evaporated to dryness a neutral compound of sulphuric acid and hematosine is obtained.

2. The hematosine dissolves in nitric acid with a copious evolution of gaseous fumes, forming a transparent yellow coloured solution. When evaporated to dryness a yellow mass remains very similar to gum, but more viscid than a strong solution of that substance. It has an exceedingly bitter taste. Alcohol partially dissolves it, and leaves a white powder behind. When digested in water all dissolves except a few drops of an oily matter which floats on the surface.

Time did not permit me to examine the action of nitric acid upon the hematosine more minutely, but it would appear to be different from its action upon fibrin.

3. Muriatic acid dissolves the coagulated hematosine very easily, and the solution has a deep-red colour. During the evaporation of this solution to dryness, I observed that a white greasy looking substance was formed, with the peculiar odour of burnt cheese. When quite dry this substance had disappeared, but the smell continued.

4. Caustic soda dissolves the colouring matter, and when the solution is evaporated to dryness what remains has a dark brown or black colour. It is partially soluble in boiling alcohol, and completely in water. The aqueous solution has a yellowish-brown colour.

5. As Berzelius, and Tiedemann, and Gmelin have observed, I found that alcohol dissolves the hematosine in a slight degree. I repeated Berzelius' experiments on this solution and obtained precisely the same results.

## II. THE SERUM.

The liquid which oozes out from the solid portion after the blood has coagulated, and to which the name of serum has been given, is chiefly composed of albumen.

It is a transparent greenish-yellow coloured liquid, of a saline flavour and somewhat unctuous to the touch. Its

specific gravity I found to be 1.0262. It coagulates at 159° according to Dr. Thomson, and after being cut into small pieces, if it is allowed to stand, a liquid gradually appears which is termed the *serosity*. By digesting the coagulated serum in alcohol and water the albumen is obtained nearly pure.

The sulphuric, muriatic and nitric acids coagulate the serum as well as heat. It is precipitated also by metaphosphoric acid, but not at all by phosphoric or pyrophosphoric acids. Acetic acid occasions no precipitate whatever.

The alkalies seem to occasion no change when poured into the serum.

None of the earths form insoluble compounds with the albumen of the serum.

Tannin occasions a very copious yellow precipitate, which, when dry, is very brittle.

The metallic salts are remarkable for precipitating albumen. The most delicate of all is the chloride of mercury, which, according to Bostock, occasions a milky mass, even when the albumen is diluted with 2000 parts of water.\*

When albumen is coagulated, either by alcohol, ether, heat, or acids, it becomes an opaque substance of a pearl white colour and possesses rather a sweetish taste. In this state it may be said to be totally insoluble in water, since Chevreul found that water did not dissolve  $\frac{7}{1000}$  lb. of its weight. When perfectly dry it possesses an amber colour, is very hard and brittle and semi-transparent like horn.

1. I found that it dissolves very easily in common nitric acid, the solution possessing a lemon colour. Evaporated to dryness the mass has the appearance of gelatine, and is soluble in water. Tincture of nut-galls occasions no precipitate but merely a slight opalescence in the solution. Acetate of lead throws down a dense flocky orange coloured precipitate. Prussiate of potash and chloride of calcium produce no change. It would, therefore, appear that this substance is quite different from that which Mr. Hatchett obtained, which was so similar to gelatine in its characters.

2. Sulphuric acid, appears at first to have no action upon the albumen, but on the application of heat a solution is effected with considerable effervescence. The solution at

\* See F. Rose's experiments on this subject, Records, i. p. 213.

first is red, but gradually acquires so deep a colour as to appear black. Dr. Hope first noticed this peculiar red colour of the solution. The substance obtained by evaporating the solution to dryness, is very similar to that obtained from fibrin in the same way.

3. Muriatic acid acts with energy upon coagulated albumen on the application of heat, and the solution becomes red. The colour deepens very much, and by evaporating the solution to dryness, the substance remaining differs very little from the muriate of fibrin.

4. When the albumen is boiled in caustic soda, a change in the nature of the albumen is effected and animal soap formed, which dissolves in water. Muriatic acid occasioned a precipitate in the solution.

### III. THE FATTY MATTERS OF THE BLOOD.

The plan I followed in order to obtain these fatty matters was very nearly that of Lecanu's; a quantity of boiling alcohol was poured into some blood which had been recently drawn from a man, and the precipitate which fell was separated by a filter and washed with hot alcohol.

The clear alcoholic solution as it cools deposits some fatty matter. When the whole is evaporated to dryness, what remains has a slight red colour owing to some of the colouring matter of the blood which it is impossible to separate. The flavour is agreeable, and the mass is deliquescent. When boiled with ether, part only dissolves.

1. The clear ethereal solution when evaporated to dryness leaves a substance of an acrid taste. A solid portion and an oily liquid constituted the residue. Lecanu calls the solid portion crystalline, but what I obtained scarcely possessed such an appearance. Boiling alcohol dissolves the solid portion and leaves the oily liquid.

(a). The alcohol on cooling deposits the whole of the solid portion, Lecanu says, under the form of crystalline plates.

It is white without taste or smell.

Presents a fatty appearance.

Cold alcohol does not dissolve it; boiling alcohol easily dissolves it, but deposits the whole on cooling.

Soluble in ether.

Not acted upon by caustic potash.

(b). The oily liquid had an acrid taste and the consistence of oil of turpentine.

Insoluble in hot or cold water.

Alcohol and ether dissolve it immediately when heated.

Nitric and muriatic acids have no action upon it, but sulphuric acid destroys it.

Potash or soda dissolves it by aid of a slight heat, and an acid precipitates white flocks from the solution. These flocks when separated possess acid properties. Boudet regards them as a mixture of oleic and margaric acids.

2. The solid portion insoluble in ether, dissolves in alcohol partially, when the whole is heated to about 100° Fah.

The alcoholic solution evaporated to dryness, left an orange yellow matter which was very deliquescent and possessed an agreeable flavour.

It had a decided taste of beef tea.

The colour was orange yellow.

Soluble in water and alcohol.

Insoluble in ether.

Causes a precipitate in acetate of zinc, with effervescence and strong smell of vinegar.

After standing for some time, causes a precipitate in acetate of magnesia with effervescence, and a slight smell of vinegar.

It was partly volatilized by heat.

It would, therefore, appear to be a mixture of lactic acid and extract of meat or osmazome, which agrees with the researches of Berzelius.

The portion insoluble in alcohol, dissolves in water and acetic acid causes a slight precipitate, but I could not examine it further from the small quantity in my possession. From Lecanu's experiments, with which those I made agree as far as they go, it would appear to be a compound of albumen and soda.

#### IV. THE SALTS OF THE BLOOD.

To ascertain the proportions of the alkaline salts, the serum was treated first with very strong alcohol and then with water. The alcoholic solution was found in the usual way, to contain the chlorides of sodium and potassium.

The aqueous solution contained no potash, but consisted merely of carbonate, phosphate, and sulphate of soda.

The salts insoluble in water and alcohol were determined by burning nearly 7000 grains of blood and digesting the residue in dilute muriatic acid. To this acid solution caustic ammonia was added and the precipitate separated by a filter. The solution which passed through was found to contain lime and magnesia, which were estimated in the usual way. The precipitate by caustic ammonia was dried and fused with carbonate of soda. The fused mass was digested for some time with pure water and the insoluble portion separated. To the solution, slight excess of muriatic acid was added, and then chloride of calcium and caustic ammonia. The precipitate which fell was composed of lime and phosphoric acid. The insoluble portion was by methods sufficiently well known to chemists, found to consist of peroxide of iron, lime, and magnesia, which must have existed in the state of phosphates.

By calculation it was found that the acids and bases might be combined, as seen in the following analysis of the human blood.

Water	- - - - -	785·890
Fibrin	- - - - -	2·120
Albumen	- - - - -	63·008
Hematosine	- - - - -	134·780
Crystalline fatty matter	- - -	1·357
Oily fatty matter	- - - - -	0·808
Extract of meat and Lactic acid		1·831
Albumen and soda	- - - - -	0·956
Chloride of sodium	- - } - -	5·341
Chloride of potassium	- } - -	
Carbonate, sulphate, and phosphate of soda	- - - - - } - -	2·110
Subsesquiphosphate of iron	- -	1·021
Subsesquiphosphate of lime	- -	0·056
Phosphate of magnesia	- - -	0·193
Peroxide of iron	- - - - -	0·203
Carbonate of lime	- - - } - -	0·326
Carbonate of magnesia	- } - -	
		1000·000

ARTICLE VII.

*On some Astronomical Methods of Observation.* BY WILLIAM GALBRAITH, A.M., Teacher of Mathematics, Edinburgh.

(Continued from page 45.)

II.—REMARKS ON THE METHODS GENERALLY EMPLOYED IN MAKING CIRCUMMERIDIAN OBSERVATIONS.

When the smaller instruments of astronomy are employed by the method of repetition, it is of importance to observers to be aware of the limits within which their observations ought to be restrained, so as to insure the requisite accuracy. This is the more to be insisted upon, as some authors seem unconscious of the limits to which observations, under given circumstances, ought to be restricted, and unacquainted with the degree of accuracy resulting from the use of different tables in the hands of the public.

The usual tables of reduction are generally formed by throwing the expression derived from the principles of spherical trigonometry into a series of two or three terms. In general, however, when it becomes necessary to embrace more than one, or at most two terms, besides the probability of introducing other errors, the application of a series is more troublesome than the direct computation by spherical trigonometry, and to avoid these, it becomes necessary to select objects which, by their situation with respect to the observer, are convenient and proper for such a mode of observation.

In general, it may be remarked, that objects near the zenith, though the most eligible for zenith sectors, or mural circles, are disadvantageous for smaller instruments, such as Borda's repeating circle, or other portable altitude and azimuth circles, when the observations are repeated a considerable number of times near the meridian. For the use of the latter class of instruments, a considerable zenith distance is necessary to obtain the requisite accuracy, for it will be found, by direct calculation, that when the latitude is  $30^{\circ}$ , the declination  $20^{\circ}$ , of the same name with the latitude, and consequently the meridian zenith distance  $10^{\circ}$ , that even Delambre's formula embracing these terms gives results erroneous to the amount of  $47''$  in excess, if the horary distance from the meridian, when the observation

is made, extend to 30 minutes of time; though, no doubt, this error is diminished when combined with observations made near the meridian. Again, when the latitude is  $40^\circ$ , the declination  $20^\circ$ , and the zenith distance also  $20^\circ$ , the same formula to three turns gives results incorrect to about half a second in excess, while the first two turns, or those commonly used, give an error of about 4" in defect. Lastly, when the latitude is so high as fifty degrees, the declination still  $20^\circ$ , and the zenith distance  $30^\circ$ , Delambre's formula to these turns gives, at 30 minutes distance from the meridian, correct results; while two turns give a small error of about half a second in defect. Assuming different numbers somewhat analogous but with similar relations, the same conclusion would follow. It may, therefore, be concluded that when the zenith distance in mean latitudes amounts to about  $30^\circ$ , two terms of Delambre's formula, or their results in tables, are sufficiently correct for practical purposes at a horary distance from the meridian of about 30 minutes, and then the calculation for the mean of a considerable number of repetitions is comparatively simple.

Instead of Delambre's formula, or tables derived from it, some practical astronomers recommend a table given by the late Dr. Thomas Young, consisting of natural versed sines, which are nothing more than the first part of Delambre's table in a less convenient form, and requiring the additional trouble of employing a constant log within to convert them into Delambre's numbers in every operation, without any equivalent advantage in any respect over the other method;\* in the words of Dr. Pearson, "Dr. Young having simplified (complicated he should have said) the preceding formula by omitting the second term," &c. Now it has already been shown that the second term cannot be admitted unless the zenith distance be considerable, not less than  $20^\circ$  or  $30^\circ$ , at 30 minutes from the meridian, or the object to be observed be a circumpolar star not very distant from the pole, in mean latitudes, and of this any observer may easily satisfy himself.

If, for example, at London circummeridian observations be made extending to 24 minutes from the meridian, (the extent to which Dr. Young's table has been carried, in a

\* The author of these remarks has endeavoured elsewhere to remedy this.

tract published by Messrs. Troughton and Simms,) to determine the obliquity of the ecliptic at the summer solstice, the first two terms of Delambre's formula would be sufficient, though Dr. Young's table, recommended by Dr. Pearson, and more lately approved by Mr. Simms, would, at 24 minutes from the meridian, give results erroneous to about 7", a quantity quite inadmissible, though this problem is just such a one as is, under the given circumstances, suited to the smaller class of altitude and azimuth circles, generally in the hands of astronomical students, and repeating circles previously alluded to.

If, however, the horary distance from the meridian be, under such circumstances, restricted to 12 minutes of time, which will admit of a sufficiently extensive number of repetitions useful to exterminate casual errors of observation, reading and dividing; two terms of Delambre's formula will be fully adequate for the purpose, while the error arising from the use of Dr. Young's table will not exceed half a second.

With regard to the most eligible size of an instrument, it is difficult to come at an accurate conclusion. That must, in a great degree, be regulated by the purposes for which it is intended. I am strongly inclined to think, however, that circles of moderate size, and of the most simple yet substantial construction are the most likely to give satisfaction. Very large mural circles that do not revolve in azimuth, especially when employed to make observations on the sun, are liable to suffer unequal expansions from heat on that side next the sun, being acted on powerfully if not shaded, which it is difficult to do completely, while the opposite side is slightly affected by its position in the shade of the other, and it is doubtful, in my opinion, whether a considerable number of microscopes except under peculiar circumstances will correct the errors arising from this cause. On the other hand, a much smaller instrument revolving in azimuth, and by that means having its different sides, though as much shaded as possible, exposed partially in succession to the sun will expand much more equally, and when the mean of three or four verniers or microscopes read at each observation, which may be repeated two or three times in pairs of double observations, within the

proper time near the meridian; on the principles of the theory of probabilities, the errors arising from all the different causes affecting the accuracy of the results must, in a great degree, destroy each other.

Though this conclusion is the most probable in reference to a steady well constructed instrument, yet it must be received under certain qualifications, since too much praise has doubtless been lavished on the omnipotence of Borda's repeating circle, especially by foreigners. M. Biot, after explaining the principles of the repeating circle, says, "Let us examine, now, in what respect the repeated multiplication of the angle proves advantageous. It would have none, if the divisions cut upon the circle were mathematically exact, and if the observer could direct the intersections of the cross wires in his telescope perfectly correct, for, in that case, one observation would give the zenith distance exact. But as these conditions cannot be accomplished in practice, the repetition of the angles supplies the defect by compensations. With regard to the error of the divisions, it is clear, that the arcs measured, follow without interruption upon the limb, in such a manner that the print of the limb, which is the termination of the previous observation, becomes the origin of the succeeding. From this it follows," says M. Biot, "that the sum of the observations, or the whole arc passed over by the verniers, *comprehends no intermediate error*, but the errors of the two extreme readings at the commencement and termination of the observations." That this conclusion of M. Biot may be true, it is necessary that there be no, or at least an insensible, resistance in the centre work to the action of the tangent screws, and that there is no imperfection in the tangent screws in producing motion, nor in the clamping screws in securing permanent positions. Now, it is clear that if there is the least defect in all or any of these, M. Biot's conclusion will be erroneous, and such must of necessity be the case to a certain degree, since it depends upon the materials of which the instrument is constructed, and cannot be removed by the abilities of the artist, or the perfection of the workmanship, however excellent it may be. *Hence, it necessarily follows that a slight relative motion must take place between the verniers and the circle for each repetition,*

*causing by that means a small error, which will be continually repeated, and which, therefore, the principle of repetition cannot cure.* It is, I believe, owing to this cause that a constant error of about 5", according to Baron Zach, may remain in some instruments in a series of many hundred observations made with the repeating circle when the clamping irons are imperfect. M. Biot goes on to say, that the errors of the extreme readings at the commencement and termination are much diminished, because the circle has generally four verniers that are read separately, and of which, the mean marks the commencement and termination of the total with a great probability of accuracy. Finally, the small error which still remains, notwithstanding these precautions in the extreme readings, is distributed over the entire arc measured on the limb, and therefore has an insensible influence on the simple value of one observation, when these observations are sufficiently multiplied. The errors of the division, then, in the repeating circle itself are also thus diminished by repetition, and the compensation of errors is not the effect of probability, but of certainty.

“To estimate the extent of this compensation, it may be remarked, that our (French) repeating circles are generally about 15 inches in diameter, and the error of division cannot exceed 15". If the error would be reduced to half a second after thirty observations, what would it become after eighty or one hundred? What does it become after, as has often been done, the series of different days are made to succeed one another, without interruption, upon the limb, so that the two errors of the extreme readings are extended upon a total arc, which contains the simple arc many thousand times? The errors of division, then, in this instrument become evanescent, and it is impossible they can be entirely destroyed in the largest instruments, if they are not repeaters. Never can the address of an artist equal a mathematical proceeding.”

But there are other errors which are destroyed by the principle of probabilities in the use of the repeating circle that still remain in other instruments. Such are, the errors of the level, which were small in the original repeating circles, and in those later constructed still less, in which the divisions of the level give immediately fractions of a

second. Such is also the case with the errors of *pointing*, or those arising from directing the intersection of the cross wires of the telescopes to the objects observed, which, though small of themselves, are destroyed like those of the level by their fortuitous compensation in many thousands of observations. These errors exist also (though I may add in a less degree) in the observations made with large instruments, as the mural circles. For the error of pointing is still found, though diminished by the greater power of the telescope, and that of the level is represented by the error of the plumb-line. But in this case the small number of observations does not admit of a compensation as exact as in the repeating circle. If we suppose that the accuracy of mean results is in the ratio compounded of the number of observations, and of the length of the radius of the instrument, *one hundred observations* made with a repeating circle of two decimetres, or about eight English inches radius, would be equivalent to *one* observation made with a mural circle of twenty metres radius, or about sixty-six English feet. "Could we obtain such instruments," says M. Biot, "and, above all, could we employ them in observations which require us to transport them from place to place?" Now, though the repeating circle is in the hands of an able observer an instrument capable of great precision, yet we cannot assent to the extravagant eulogium thus betowed upon it by M. Biot in his *Astronomie Physique*, because it rests on assumptions too gratuitous to be granted without qualification; and, as we have already remarked, *he has not alluded at all to the errors inseparable from its construction, and the method of using it when executed by the best artists.*

However perfect the damping screws may be, yet still, by repeating the observations, repeated small relative motions by pressure must take place between the verniers and limbs, which remain as a constant error that no continuation of the process of repetition can remove, because it arises from that very principle. If, however, an equal number of observations at nearly equal zenith distances on opposite sides of the zenith be taken, then on the principles of probabilities, it may be expected that the errors from this cause will likewise have a tendency to destroy each

other. Thus, by a judicious use of the repeating circle it may be employed to great advantage in all observations which require a moderately sized instrument capable of easy transportation. Still, however, the complex nature of its construction and the involved methods of observation are inherent disadvantages, which render a commodious instrument of similar dimensions but more simple in principle a desideratum to a numerous class of practical astronomers.

The only other instruments whose prices are moderate and dimensions convenient are Captain Kater's circle somewhat enlarged, and Mr. Troughton's portable altitude and azimuth circle. In these the repeating principle so much recommended in Borda's, is dispensed with for the purpose of securing stability and permanency of adjustment, which are the main desiderata in the other.

Though the same principle of repetition cannot be practised by these instruments as in that of Borda, yet the observations may be repeated near the meridian with success, in which the constant error arising from the imperfection of the repeated damping on Borda's plan, is thereby avoided, while by means of three verniers carefully made, combined with the motion of the celestial body in zenith distance during the time of repetition, the errors of division and pointing will, if not entirely destroyed, be greatly diminished—a proposition supported by uniform experience.

In this country the use of the great mural circle permanently fixed in the meridian is generally adhered to, and by means of its size, the power of its telescope, and the number of its reading microscopes, its errors are supposed to be almost entirely destroyed, though the principle of repetition be abandoned. Thus by the introduction of one advantage, another is lost instead of attempting a union of both. The smaller circles possessing the property of repeating the observations, may, therefore, approach very nearly the precision of the larger, as has been proved in the measurement of the French arc of the meridian compared with the British trigonometrical survey. It is indeed difficult to say, whether the final results of the one or the other possess the superiority, though the former was executed chiefly with Borda's repeating circle of about 16

inches diameter, or 8 inches radius both with regard to astronomical and geodetical observations, while the latter had the benefit of a zenith sector of 8 feet radius, and a theodolite of 3 feet in diameter, both without the principle of repetition adopted by Borda. Hence, it may in general be concluded, that the principle of repetition employed in one class of instruments was nearly equivalent in securing accuracy of results to the advantage of large size, and the superior power of the telescopes in the other. Hence, we may also infer, that one of Mr. Troughton's objections to the repeating circle, namely, that when the instrument has a telescope of small power the observations are charged with errors of vision which the repeating principle will *not cure*, is not borne out by experience. Indeed we cannot comprehend the notion why the errors of vision as well as of division according to the usual doctrine of probabilities are, if not destroyed, at least greatly diminished, by the principle of repetition. MM. Lenoir and Fortin have lately improved the movements of the repeating circle in some respects according to Puissant, and Mr. Troughton has given some of its parts a better position for diminishing friction and insuring accuracy of motion, though on the whole it is still complex in its construction, and, so long as its peculiar repeating principle is retained, it cannot be much simplified. The late M. Reichenbach, of Munich, constructed repeating circles which for some time have enjoyed great reputation, chiefly on account of the goodness of the tangent and damping screws, and the accuracy of the division. The superior telescope is also attached to a circle turning with ease and precision within the graduated circle and having their faces both in the same plane.

On the recommendation of Laplace this artist constructed a large repeating circle of this description for the Royal Observatory at Paris. Whatever properties it may possess yet it has been thought advisable to have also a large mural circle on the principles of Mr. Troughton constructed by Fortin, which the French express a hope will contribute greatly to the advancement of astronomy. From these circumstances it seems to be admitted that the powers of the repeating circle have been overrated, and that the advantages derived from the repeating principle are in a great

degree counterbalanced by the defects of its construction. May we, therefore, infer that the smaller class of portable circles of the constructions of Troughton and Kater which admit of repeating the observations near the meridian a sufficient number of times to insure accuracy, are, from the compactness of their structure, their stability and accuracy of motion, superior to the repeating circle. Of all these circles Kater's is the cheapest and susceptible of great accuracy when not too small. The size I would venture to recommend would be about 6 or 8 inches in diameter with telescopes magnifying 20 or 30 times and the three verniers each reading 10". To those who are willing and able to afford the expense, one of Troughton's altitude and azimuth circles of 10 or 12 inches in diameter would prove an excellent instrument, though for travellers it would be rather too heavy. In that case Kater's would be a good substitute and its efficiency will be shown in the following observations.

(*To be continued.*)

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#### ARTICLE VIII.

*On the Curved Figures produced by rapidly rotating Disks.*

By CHARLES TOMLINSON, ESQ.

(*Continued from Vol. iii., page 44.*)

SINCE the publication of the last paper, I have taken advantage of a visit from my friend Mr. Dodd, further to investigate several additional facts on this subject.

A part of the experiments before detailed, as well as one by Professor Wheatstone, was performed simply by viewing the rotating disk for one instant of time during various sudden flashes of light, the result being the resolution of the various figures on the disk (whatever they may be) into precisely their original stationary arrangement; but the experiments with a slitted disk are of a more extended nature, a new element entering into the resulting phenomena, namely, the generation of curved lines where none previously existed, and the consequent production of new figures of considerable beauty.

By referring to vol. iii. page 42, the reader will find that

the radii of a star on the smaller disk when viewed through a slit in the larger disk, and both revolving, were curved. The nature and symmetry of these curves are dependent on several conditions, which may be thus stated :

1. The relative sizes of the disks.
2. The relative velocities of the disks.
3. The respective rotations being in the same, or in opposite directions.
4. The situation of the slit.
5. The part of the slit to which the eye is applied.

The great variations produced by changes in these conditions render it impossible to convey a correct idea of the curves, unless we adhere to one ratio of proportion in each of the above particulars, and to effect this in the completest manner, the following mode of observation was adopted :

Instead of employing two rotating disks, the slit is made in the disk, which is the object of experiment, and the eye, placed behind the disk, views through the slit the image of the rotating figure reflected in a stationary plane mirror, placed in front of the disk, within about two feet.

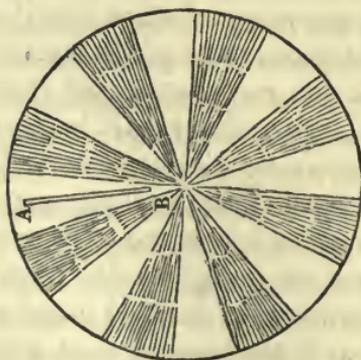
Thus, by considering the revolving image as a second revolving disk, the following conditions are fulfilled :

1. The two disks are of equal size.
2. They rotate with equal velocities.
3. They rotate in the same direction.

These three data being thus constant, the effect of variation in the fourth and fifth will now be described.

The disk,

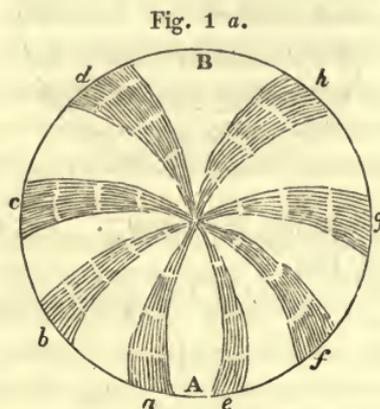
Fig. 1.



is covered with a star, consisting of eight black and eight red radial bands. The slit A. B. occupies nearly a

semi-diameter in the centre of one of the bands, and it may be stated, once for all, that the eye is placed behind the disk, so that the front of the disk may be seen reflected in the mirror, which occurs once during one revolution of the disk, and if the disk perform more than six revolutions per second, an uninterrupted view of the figure can, of course, be obtained. This arrangement may be termed the mirror apparatus.

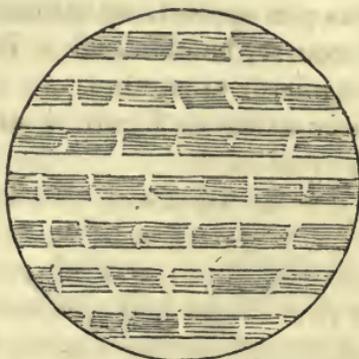
Under these circumstances figure 1 assumes the form Fig. 1, *a*.



Which offers a general view of the figure, for the curvature of each radius varies with every position of the eye, considered with reference to the inner or the outer end of the slit. When the eye is near the inner end of the slit the radii are congregated much nearer the point A., (figure 1 *a*) while at and about B the interval between the two upper radii is greatly increased; but if the eye be held near the outer end of the slit the radii are less curved, and their distribution more equable; but at whatever part of the slit the eye be applied this rule is constant—that the point A, towards which the curves tend, is seen at the axial end of the slit, in consequence of the slit being only at one side of the axis, and the radial band occupying that position is of the same colour as that which contains the slit. If, however, the slit, instead of occupying the centre of one radial band, be on a line of division between two bands, a line of division will occupy the central position, which in the former case was occupied by a band, and, in the latter case, the bands *a*, *b*, *c*, *d*, will be opposite in colour to *e*, *f*, *g*, *h*.

A beautiful, but very simple, change in the apparent figure of a striated disk is made by merely causing it to revolve, and viewing it in the usual way, without the mirror apparatus.

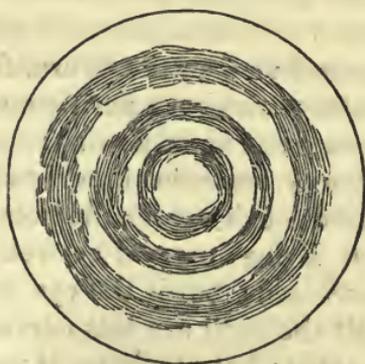
Fig. 2.



represents the disk while stationary. Figure 2 *a* the disk when simply revolving.

The law which regulates the production of the rings, figure 2 *a*,

Fig. 2 *a*.



appears to be this: whatever tint predominates at any point of the disk, the rapidity of revolution causes the same tint to appear at every other point equidistant from the centre; and, to ascertain what that tint would be, describe a concentric circle through that point, and by adding the dark portions through which it would pass into one series, and the light portions into another series, by combining the two aggregates we may ascertain beforehand what number of concentric circles will result: the outlines are, of course, not defined, but melt into each other.

In this case the dark stripes form an odd number, and the proportion of white is greater than the black; hence the rings; but suppose the proportions of the two colours to be strictly equal; that the disk is poised exactly on its centre, and very slightly rotated without the slightest eccentricity of motion, then the stripes, radii, chequers, or whatever figures occupy the surface of the disk (except concentric circles) blend into one uniform tint midway between the two colours of the disk, entirely free from rings.

From this we may deduce :

1. That, when a disk is so divided that with any radius a circle would pass through equal portions of the two colours, an universal blending of colour will result.

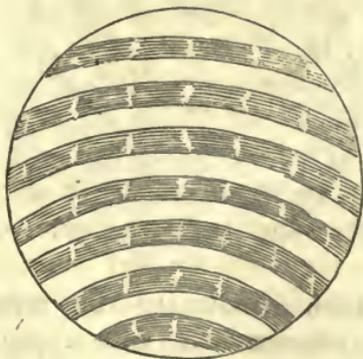
2. That if at any part of the disk a balance of colour be not observed at opposite sides of the centre, concentric rings will result.

3. That, as the *non*-existence of that balance depends upon a definite principle of construction, the number and breadth of the rings can be computed; but,

4. If the equipoise be disturbed by extraneous causes, such as imperfect division, or eccentricity of adjustment, the rings are uncertain and incommensurable.

But the disk, fig. 2, when employed with the mirror apparatus, assumes altogether a different arrangement, of which

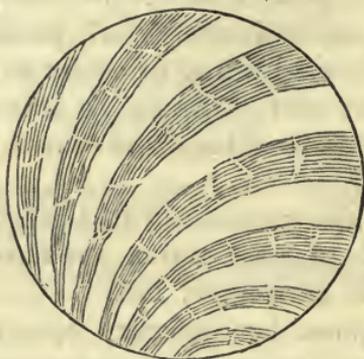
Fig. 2 b.



will convey an idea. The surface is laid out in curved concentric bands of the utmost symmetry, the number and breadth of which are the same as in the rectilinear striæ. The centre of these concentric segments, which is at the

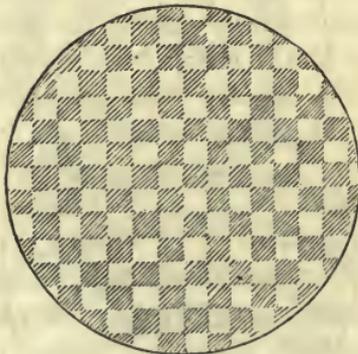
axial end of the slit, is either at or exterior to the edge of the disk, according to the position of the eye.

This figure (2 *b*) is produced when the slit cuts the striæ at right angles, but when it is parallel to them the figure is totally changed: the bands are all rectilinear, but their parallism is destroyed, as they all seem to diverge from a point situate at the axial end of the slit. Again, when the slit is inclined  $45^\circ$  to the striæ, the image presents the shell-like form represented in

Fig. 2 *c*.

The same principle, but beautifully modified in its application, is perceptible on employing a chequered disk,

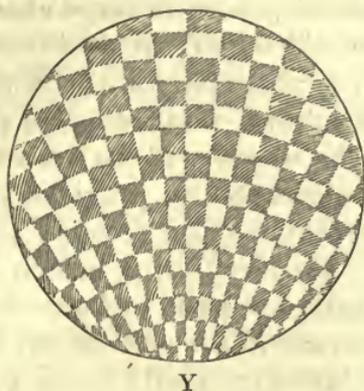
Fig. 3.



The figure which illustrates the striated disk, when simply revolving, figure 2 *a*, will apply to the present, the only difference being a more gentle blending of the annular tints into each other; but when viewed through the slit, the reflected image presents the appearance of a tessellated globe, and the illusion is so perfect that it requires an effort to preserve the idea of a flat surface. Figure 3 *a*.

These chequers, when viewed through a slit, parallel to the rectilinear division, present

Fig. 3 a.



evidently partaking in the general character of fig. 2 b, the difference being due to the division of the striæ into squares. If the slit be inclined  $45^\circ$  to the divisions, all the diverging lines proceeding from Y, figure 3 a, cut the diagonals of the chequers instead of marking their boundaries.

The same principles obtain through an endless variety of figures: thus, if the disk be diametrically divided into two, and the slit be on the line of division, the figure is altogether unchanged; but if it be at right angles to that line, the half which contains the slit encroaches upon the other half by the curvature of the line of division, thus presenting the gibbous form of a three quarter moon.

Nearly the whole of the experiments now detailed were performed with the mirror-apparatus, for the purpose of getting uniformity in the revolutions, otherwise no definite figures could have been given; but the figures are the same in principle as those produced by two revolving disks, the latter arrangement presenting figures modified in every imaginable way by one or more of the five conditions before stated.

When two disks, for instance, are moving with unequal velocities, the nucleus towards which the curves tend, or from which they seem to spring, oscillates to and fro, and the curves themselves vary in their number and respective distances from each other.

The slitted disk is a modification of M. Plateau's Fantascopé, the latter containing many short slits, and the

former only one, which consists of a radius nearly of the disk. It is capable of many amusing modifications, with one of which I now conclude this subject. Below, and at right angles to the slit, paste in large characters any word or words, such as AT REST, in an inverted position, so that when seen in a plane mirror the reflected image may be erect and in its natural position. On placing the head behind the disk the words will be seen through the slit in the mirror uninterruptedly, provided the disk perform more than six revolutions per second. In this case the words are seen in a curved form, which is evidently due to the principle before stated, and the letters are all of the same size, whereas if the slit be parallel to the order of the letters, the latter gradually increase in size from the axis to the outer end of the slit.

All the disks should be well illuminated by a direct light falling upon them; and their backs, where the eye is placed, should be blackened all over, and the multiplying arrangement to which the disks are successively attached should allow of comparatively slow and very rapid rotation.

Salisbury, 8th January, 1836.

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#### ARTICLE IX.

##### ANALYSES OF BOOKS.

(Continued from page 68.)

I.—*The Transactions of the Linnean Society of London.* Vol. XVII, part 3rd, 1835.

*Description of five new species of the Genus Pinus, discovered by Dr. Coulter in California.* By Mr. DAVID DON, Lib. L. S.

NOTWITHSTANDING the addition of seven new species to this genus, by Mr. Douglas, within the space of a very few years, we have in this paper a detail of the character of five additional species discovered by Dr. Coulter in California; especially on the western flanks of the northern Andes, and the extensive parallel ranges of mountains which extend from south to north through that country.

1. The *P. Coulteri* rises to the height of 80 or 100 feet at an elevation of from 3000 to 4000 feet above the level of the sea; growing intermingled with the *P. Lambertiana* on the mountains of St. Lucia, near the Mission of San Antonio, in latitude 36°. 2. *P. muricata*, attains a height of 40 feet. It was found at San Luis

Obispo in latitude 35°, at an elevation of 3000 feet. 3. *P. radiata* found about Monterey in latitude 36°, near the level of the sea, and growing almost close to the beach. It affords excellent timber, which is very tough and admirably adapted for building boats. 4. *P. tuberculata*, resembling in position and appearance the preceding. 5. *P. bracteata* was found growing on the sea side of the mountain range of St. Lucia, about 1000 feet lower than *P. Coulteri*. The trunk rises to the height of 120 feet, not exceeding 2 feet in circumference and as straight as an arrow.

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*Some account of the Galls found on a species of Oak, from the shores of the Dead Sea.* By Aylmer Bourke Lambert, Esq., F.R.S., V.P.L.S.

This paper contains a description with figures of some galls brought from the Holy Land by the Hon. R. Curzen, and which the author considers to be the "mala insana," or apples of Sodom of history. They grow on the *Q. vercus infectoria*, a tree which grows abundantly in Syria. The insect which forms them has been named by Olivier *Diplolepis*. When on the tree the galls are of a rich purple and are varnished over with a light substance of the consistence of honey, shining with a most brilliant lustre in the sun, which makes them look like a most delicious and tempting fruit.

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*Note on the Mustard Plant of the Scripture.* By Mr. Lambert.

The author considers this plant to be the same as that daily used among us. He conceives that the expression "less than all the seeds that be in the earth," used in Scripture was used comparatively and meant nothing more than a small seed. Captains Irby and Mangles have informed the author that our mustard plant, the *sinapis nigra*, grows in the Holy Land as high as their horses heads, and other travellers have seen it growing to the height of 10 feet.

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*On several new or imperfectly understood British and European Plants.* By C. C. Babington, F.L.S., &c.

1. *Herniaria hirsuta*, has been found only at Colney Hatch Barnet, by Hudson, and Milne, and Gordon, but not since 1793.

2. *H. glabra*. Near Newmarket, Rev. Mr. Hemsted. The description under this title in Hooker's Flora applies to *H. ciliata*.

3. *H. ciliata*. Liyard point, Ray and Borrer.

4. *Crepis Virens*. Common on walls, &c. This plant has usually been confounded with *C. tectorum*, which does not appear to be a native of this country. It is distinguished from *virens* by its "very long fruit, equalling the pappus: attenuated above, its ribs rough; the margin also of the upper leaves is revolute that not being the case in *C. virens*."

5. *C. biennis*. Involucrum, ovate, oblong, both when in flower and seed, not becoming ventricose as in *C. virens*.

6. *Erica Tetralix*. Stems branched only towards the base. Leaves and sepals linear, lanceolate, downy, their margins secured so as almost to meet behind.

7. *E. Machaiana*, N. S.—Fol. quatern. ovat. ciliat. supra glabris, floribus capitat. pedicellatis, sepalis ovat. ciliat. glabris, pedicellis pilos. et comentosis, corolla oblong. ovatis, antheris anstat. inclusis, stylo exserto.—Distinguished from *E. Tetralix* by the form and structure of its leaves and sepals, the glabrous upper surface of the former, and its total difference in habit. It agrees with *E. ciliaris* in the character of its foliage, but differs from that plant by having anthers awned. Gathered by the author on Craigha Moira, Connamara, Ireland, in August, 1835. Mr. McCalla, of Roundstone, directed his attention to it, as being, perhaps, a new British heath. It is named after Mr. Mackay, of Dublin. Some botanists consider it as a variety of *E. Tetralix*.

8. *Polygonum Maritimum*.—Christchurchhead, towards Mudiford Borrer; Herne Bay, Jersey. Mr. W. C. Trevelyan.

9. *P. Raii*.—Intermediate between *P. Maritimum* and *aviculare*. The *P. aviculare*  $\beta$  of Hook. Brit. Flora.

10. *P. dumetorum*—Wood at Wimbledon: Mr. J. A. Hankey.

11. *P. Convolvulus*—Improved description by the author.

12. *Euphorbia pilosa*—*E. pilosa*  $\beta$  of Hooker.

13. *Euphorbia coralloides*—*E pilosa*  $\alpha$  of Hooker: naturalized at Henfold, Sussex.

14. *Habenaria chlorantha*—*Orchis bifolia*  $\alpha$  of Smith.

15. *H. bifolia*—*O. bifolia*  $\beta$  of Smith.

16. *H. fornicata*—A distinct species, having its anther rounded at the tip and hooded, and the cells parallel; plant smaller than *H. bifolia*.

*Observations on the Species of Fedia.* By Joseph Woods, Esq., F. L. S.

This genus was originally made from the varieties of the Linnean species, *Valeriana locusta*, being separated from *Valeriana* by habit as well as by the want of a feathery crown to the seed. The name comes from *Hædus*, or *Fædus*, a kid, and was introduced by Adanson, although not applied by him to this genus. Decandolle divides it into four divisions. 1. *Locusta*: with one or two empty cells and a gibbous corky or spongy mass at the back of the fertile one. 2. *Psilocoelae*: the two empty cells, each reduced to a hollow nerve. 3. *Platycoelae*: two empty cells, nearly as large as the fertile ones. 4. *Selenocoelae*: section of the fruit, crescent shaped, with two empty cells.

Mr. Woods suggests that the European species may be divided as follows: A. Flowers ringent. 1. *F. Cornucopiæ*: B. flowers nearly regular: A. fruit with a corky mass at the back of the seed. 2. *F. olitoria*. 3. *F. gibbosa* b. section of the fruit crescent shaped, two barren cells. a. *F. turgida*. 5. *F. carinata*. 6. *F. platyloba*: C. barren cells two, hardly touching in the middle; divisions of the calyx hooked; flowers in globular heads; upper leaves

generally pinnatifid at the base. 7. *F. Hamata*. 8. *F. Coronata*. 9. *F. Ciliata*: *d* barren cells two, hardly touching in the middle; prolonged into teeth or horns, but not forming a membranous calyx. 10. *F. echinata*. 11. *F. trigonocarpa*. 12. *F. Sphaerocarpa*. 13. *F. pumila*: *e* barren cells two contiguous; crown erect. 14. *F. auricula*: *f* barren cells four. 15. *F. vesicaria*: *g* barren cells wanting, or reduced to a mere nerve; panicle nearly fastigiate; the lower flowers solitary. 16. *F. lasiocephala*. 17. *F. eriocarpa*. 18. *F. dentata*. 19. *F. puberula*. 20. *F. microcarpa*. 21. *F. truncata*. The paper is illustrated by drawings.

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*De Merchantieis* Auctore Thoma, &c. Taylor, M. D., F. L. S.

The species of this order of plants, although limited in number, are widely spread over the world, as we find from the Baltic sea to the Mediterranean in Europe, over all America and even the mountains of Nepaul. The author treats of such in this paper as have come under his notice, under the genera *Marchantia*, *Fegatella*, *Fimbraria*, *Lunularia*, *Hygropyla*. Those who are fond of the study of this beautiful order of plants, we cannot direct to a more distinct source for the solution of any difficulties which they may happen to meet with, although it would have more congenial to the acquirements of most botanists if the concluding remarks on each species had been couched in English instead of Latin. We approve of the use of the latter language for stating the specific characters, but to carry the use of a dead language any futher is an abuse.

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*On the Eriogoneae, a tribe of the order Polygonaceae.* By G. Bentham, Esq., F. L. S.

The genus *Eriogonum* was first established by Michaux in his *Flora Boreali-Americana*. The number of plants now known which approach nearly to this genus amount to 40 species. In this paper Mr. Bentham proposes to divide these into three genera. All the species are equally distinguished by their involucrate inflorescence and absence of stipulae, at least to the lower or cauline leaves. But a considerable difference of habit has induced him, at the suggestion of Mr. Brown, not only to separate generically 5 species with uniflorous involucre; but among these to isolate one (*Mucronea*) which has a compressed and bidentate involucre formed of two leaves instead of a triangular sexdentate one formed of six leaves as in the four species (*Chorizanthe*). The latter genus is further confirmed and augmented by seven species collected in Chili by Macrae, Cuming, Bridges, &c.

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*Observations on the Genus Hosackia and the American Loti.* By George Bentham, Esq., F. L. S.

The author modifying his views expressed in the Botanical Register (vol. xv. tab. 1257) in reference to these two genera, is now induced to confine the circumscription of *Hosackia* to the umbellate species,

and proposes to consider the uniflorous ones as belonging to *Lotus* of which they would form a separate section, which, with reference to the size of the flowers, might be called *Microlotus*. The two genera would then be characterized by the form of the flower; and the peculiarities observable in the organs of vegetation would again be reduced to their proper level, that of subsidiary, not essential characters. In the true *Hosackiae* the claw of the vexillum is always at some distance from those of the other petals; the alae adhere by their margins to the carina, and usually (if not always) spread at right angles from it; the carina is usually less rostrate than in *Lotus* and the stigma more distinctly capitate. In *Microlotus* the flower does not present any essential differences from that of our European *Loti*. The author describes 11 species of *Hosackia*, and 5 species of *Microlotus*.

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ENTOMOLOGY.

*Descriptions, &c., of the Insects collected by Captain P. P. King, R. N., F. R. S., in the Survey of the Straits of Magellan.* By John Curtis, Esq., F. L. S. A. H. Haliday, Esq., M. A., and Francis Walker, Esq., F. L. S.

The collection was formed along the coast from St. Paul's in Brazil to Valparaiso. It is interesting to trace the similarity which exists between the corresponding parallels of the southern and northern hemispheres such as is afforded by the present collection. Thus the genus *Carabus* appears unknown in S. America, excepting about lat. 50° where a species of that group with a narrow thorax has been found; the genus *culex* also occurs. The insects of S. America bear little resemblance to those of S. Africa. Descriptions are given of species belonging to 55 genera of *Hymenoptera*, and of 78 genera of *Diptera*.

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*Characters of Embia, a genus of insects allied to the White Ants, (Termites) with descriptions of the species of which it is composed* By J. O. Westwood, Esq., F. L. S.

This genus is remarkable at present not only because it consists of species nearly allied to the white ants, but because it is composed of 3 exotic species, each from a different quarter of the globe, while a single specimen only of each has hitherto come under the observation of entomologists; each possesses also characters of a higher rank than mere specific distinction, whence he has been under the necessity of considering each as a distinct subgenus; these are *Embia Savignii*, *Oligotoma Saundersii*, and *Olynthia Braziliensis*. Mr. Westwood has also observed two species imbedded in Gum Copal or Anime, which he has not been able sufficiently to identify.

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*On a new Arachnide uniting the genera Gonyleptes and Phalangium.* By the Rev. F. W. Hope, M.A., F.R.S., F.L.S.

This remarkable insect with disproportionally long hinder legs, so long that it is difficult to conceive of what utility they can be, was collected in Brazil by the late Mr. Haworth, a zealous promoter of

entomology in all its branches. Mr. Hope terms it *Dolichoscelis Haworthii*.

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ZOOLOGY.

I.—*Description of a New Species of the genus Chameleon.* By Mr. Samuel Stutchburg, A. L. S., &c.

*Chameleon cristatus.* C. Superciliari occipitalique carina elevatâ et crenulatâ, caudæ anteriori parte dorsique apophysibus elongatis cristam dorsalem constituentibus : squamis fere rotundis subæqualibus. The striking peculiarity of this animal consists in its having a dorsal crest, supported by the spinous processes of the Vertebrae, by which character it approaches the Basilisks. It was brought from the banks of the river Gaboon in Western Equinoctial Africa, and was presented to the Museum of the Bristol Institution, by Messrs. King and Sons of that city.

II.—*The Practical Mechanic's Pocket Guide, &c.* By Robert Wallace, A. M., Glasgow, 1836. p. 120.

This is a very neat and useful little compendium of the most important rules for the practical mechanic, arranged under the heads of I. Prime movers of machinery : 1st. Animal Power.—2d. Wind power.—3rd. Water power.—4th. Steam power. II. Weight, strength, and strain of materials. III. Practical tables : 1st. Weight of metals.—2nd. Specific gravity and weight of materials.—3rd. Steam and steam engines.—4th. Specific cohesion and strength of materials.—5th. Mechanical powers. The section upon steam is illustrated by a good plate of the steam engine, and a plan is appended to the work of the land which has been drained behind the town of Greenock, and of the great reservoir which is supplied by these numerous drains. We have no doubt that Mr. Wallace's book will be duly appreciated by those for whom it is intended, and we recommend it to the attention not of mechanics alone, but of all who are interested in this important branch of philosophy.

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ARTICLE X.

SCIENTIFIC INTELLIGENCE.

I.—*Pharmacy, &c.*

1. *Quinin and Cinchonin.*—Geiger has described a simple method of separating these principles from the alkaline mass which does not crystallize with acids, and which Serturmer termed *Quinoidin*. The compound dissolved in water, saturated with an acid, should be mixed with an excess of neutral acetate of lead, by which means the resinous matter in combination with the basis will be precipitated in union with the oxide of lead. The solution should then be filtered and digested with freshly heated animal charcoal, until a filtered specimen ceases to re-act on oxide of lead. The lead may then be precipitated by phosphate of lime, or sulphuretted hydrogen. Potash or soda will now precipitate the Quinin and Cinchonin, and the mother

liquor may be evaporated to separate the last portions.—(*Jahresbericht*, 1835.—252.)

2. *Delphinin*.—Couerbe has obtained this principle by the following process. The seeds of the *Delphinium staphysagria*, which are gray or brownish, but not black, the latter containing little active matter, were first subjected to the action of boiling alcohol. The alcoholic extract, after distilling over the alcohol, was treated with dilute sulphuric acid; the filtered acid solution was precipitated by alkali; the precipitate dissolved after drying, in boiling alcohol; the solution treated with incinerated blood, filtered, and evaporated, when the Delphinin remained of the same nature as that which occurs in commerce. One French pound of the seeds affords 55 to 60 grains of such Delphinin. It was dissolved in water acidulated with sulphuric acid, filtered and mixed up by drops with nitric acid, which precipitated a dark brown or reddish as well as a black substance, by which the colour of the solution was greatly altered. After twenty-four hours the precipitate had collected at the bottom of the vessel, when the supernatant liquor was decanted. It was precipitated with a very dilute solution of potash; the precipitate was well washed, dried, dissolved in absolute alcohol, filtered, and evaporated, when a resinous, yellowish, strongly alkaline mass remains, which may be moistened with a little water, lest some nitre should still remain attached to it. Delphinin thus obtained is purer than any hitherto procured, but is a mixture of two different bodies, which may be separated by ether. This dissolves the *Delphinin*, and leaves another substance, which Couerbe calls *Staphisain*. Delphinin is a yellowish resin, but its powder is almost white. It has a burning taste, and leaves the impression long behind it. It cannot be crystallized, and melts at 248°. At a higher temperature it decomposes. It is not affected by chlorine at common temperatures, but at 302° it is decomposed, while it first becomes green, then dark brown, and disengages muriatic acid: 150 parts of Delphinin absorbed 20 parts of muriatic acid gas. From which, and two other trials, its atomic weight appears to be 26·5. It consists of carbon 76·69, Azote 5·93, Hydrogen 8·89, Oxygen 7·49. *Staphisain* is an uncrystallized yellow substance. It melts at 312°. It is almost insoluble in water, which, however, takes up some thousandth parts, and imparts to it an acrid taste. Whether it has an alkaline reaction has not been ascertained. It is soluble in acids, but is not neutralized by them. Hot nitric acid converts it into a bitter resinous substance. It consists of carbon 73·566, Azote 5·779, Hydrogen 8·709, Oxygen 11·946. (*Ib.* 255.)

3. *Brazilin*.—Chevreul has obtained the colouring matter of Brazil wood, in the form of small yellowish red needles.—(*Ib.* 317.)

4. *Cactin*.—Voget has procured 30 per cent. of a carmine red colouring matter from the flowers of the *Cactus speciosus*. It is taken up with alcohol of 60 to 70 per cent. It is not dissolved by ether and absolute alcohol. After the leaves have been treated with alcohol, a mixture of alcohol and ether takes up 5 to 10 per cent. of a scarlet red colouring matter. Both are soluble in water.—(*Ib.* 318.)

5. *Urticin*.—According to Knezaureck, the tops of the *Urtica*

*dioica*, or common nettle, afford, in autumn when the leaves are falling off, to water a red colouring matter, which answers well as a dye. With chloride of tin it colours the solution bright red, and forms a red precipitate.—(*Ib.* 318.)

6. *Elaterine*.—Clamor Marquart gives the following process for separating this alkaloid from the *momordica elaterium*. The nearly ripe fruit was collected in July, pressed, and the juice evaporated to the consistence of extract. This was then digested with alcohol of 90 per cent., the latter distilled off, and the residue agitated in boiling water, in which, after cooling, the crystals of *elaterin* are observed covered with chlorophylle. The solid matter is separated from the fluid first, collected upon a filter, and separated from chlorophylle by dropping ether upon it. A colourless crystalline, almost tasteless powder remains, which by distillation leaves a product, containing ammonia. It is insoluble in water, easily soluble in alcohol. It is quite neutral, scarcely soluble in ether, little soluble in cold, very soluble in hot oil of turpentine, from which it does not separate on cooling.

7. *Atropin*. Brandes and Geiger and Hesse procured this alkaloid by means of ether. According to Geiger and Hesse  $62\frac{1}{2}$  grains of atropin may be procured from a pound of the extract. The impure atropin should be dissolved in water containing  $\frac{1}{10}$  of sulphuric acid. More should be taken than is necessary for dissolving the atropin, and the solution should be digested for several hours with blood charcoal (*blutlaugenkohle*). The filtered yellow solution should then be precipitated with dilute caustic soda. The flocky precipitate is then to be separated from the liquid and washed with cold water, by which means it becomes pulverulent. Some more precipitate will fall down from the mother liquor and will be increased by the addition of common salt. Atropin may be obtained in a crystalline state by dissolving it in the smallest possible quantity of boiling water; on cooling it crystallizes, or when dissolved in alcohol and allowed to evaporate spontaneously it crystallizes. Atropin possesses the following properties:—When precipitated by alkali it is a pure white powder containing crystalline particles. It crystallizes from its solution in needle shaped crystals. It possesses no smell but a highly bitter and acrid taste. It is not acted on by light. It has an alkaline reaction. It melts at  $122^{\circ}$ . At  $212^{\circ}$  it becomes brown. At  $338^{\circ}$  it is very brown, and a portion of it sublimes unchanged; then follows a combustible oil and ammoniacal vapour, and the mass is decomposed leaving much carbon behind. It requires 500 parts of cold water to dissolve it, but when dissolved in 58 parts of hot water it does not separate on cooling. 30 parts of boiling water are saturated by 1 part of atropin, on cooling the greater proportion crystallizes. On boiling the solution a small portion of the atropin appears to be volatilized with the vapour of the water; 8 parts of cold absolute alcohol dissolve it. It is soluble in 63 times its weight of cold ether. Dilute acids are completely neutralized by atropin; concentrated acids decompose it. It gives a yellowish precipitate with chloride of gold and chloride of platinum. It is precipitated white by infusion of galls. According to Liebig 59 parts of anhy-

drous muriatic acid, saturate 312 parts of atropin which gives the atomic weight 240.6. It consists of carbon 70.986, azote 7.519, hydrogen 3.144, oxygen 13.351; from which the atom comes out 23.45. According to Geiger and Hesse the salts of atropin have a bitter taste, are readily soluble in water and alcohol, less so in ether.

*Muriate of Atropin* crystallizes in star like needles. When prepared by saturating dry atropin with muriatic acid gas, the salt has an acid re-action, but when crystallized an alkaline re-action.

*Sulphate of Atropin* crystallizes readily.

*Nitrate of Atropin* dries into a clear colourless mass:  $\frac{3}{4}$  grain of this salt dissolves in a drachm of water.

*Acetate of Atropin* crystallizes in stelliform fine needles which lose their acetic acid when completely dried, and are no longer completely soluble in water.

*Tartrate of Atropin* forms a transparent colourless mass.

The discovery of atropin is a very important one, because it is to this substance that the extract of *Belladonna* owes its peculiar properties in a medical point of view; 1 part of a salt of atropin is equivalent to 200 parts of extract or 600 of the dried plant.

This alkaloid appears to have been first discovered by Mein in 1831. He prepared it by digesting the bruised root of *Belladonna* in alcohol in the proportion of 24 parts root to 60 alcohol, of 90 per cent. The clear solution was digested with slaked lime and filtered, sulphuric acid was then added which precipitated gypsum. The acid solution was distilled to more than one half, mixed with 6 or 8 parts of water and the alcohol evaporated. The remaining solution was mixed with a little carbonate of potash, which at first precipitated a resinous matter forming a gelatinous mass, when more atropin was precipitated by the addition of carbonate of potash. In from 12 to 24 hours it exhibits a disposition to crystallize, and can then be separated from the mother liquor and pressed; it is then soluble in alcohol and crystallizes by spontaneous evaporation. By a new analysis made by Liebig, the formula for atropin is  $C^{34} H^{23} N^2 O^3$  and its atomic weight 289.0. (*Jahresbericht*, 1834, 262.)

## II.—*Phenomena of Crystallization.*

WHEN the formation of crystals are observed under the microscope according to Ehrenberg, the first thing which attracts attention is a rapid action going on about the crystal; suddenly a solid point forms in the transparent liquid, appreciable by its opalescence, and increases with astonishing rapidity, shewing that this point concentrates and condenses the saline particles previously dispersed and suspended in the water. This concentration supposes a motion towards the centre, and one is apt to think that the aggregation of the atoms is of such a nature that the density will increase towards the edge. In this view it is rather surprising that there should be no motion nor agitation in the neighbourhood of the crystal. In order to investigate the subject more accurately, Ehrenberg examined strongly coloured crystals. He dissolved bichromate of potash and sulphate of copper in water: he could not discover in either case any visible current re-

sulting from the concentration of the coloured particles, nor an aggregation around the crystal, while it increased with great rapidity; yet even by sprinkling a fine powder over the liquid which crystallized, no currents could be detected. Hence, crystallization is analogous to the phenomena which it is generally supposed take place when masses aggregate in space. A nebulous appearance first occurs, the matter of which gradually condenses in the centre, then a kernel is formed with an areola, and lastly a properly formed world is completed.

Ehrenberg has carefully studied some drops of a solution of common salt, and has observed that hexagonal tables are formed at the limit of evaporation often very regular but frequently deposited one upon the other. In the middle of these very delicate hexagonal tables a point was suddenly formed which attracted to it the mass of tables. Immediately the observer noticed there a small tube increasing with immense rapidity and enlarging as the tables diminished. The water of the Baltic and N. Sea are particularly fitted for these observations. Conceiving that the phenomena might be owing to the presence of two different salts, he made an experiment upon common salt, chemically pure and dissolved in distilled water. In this case he observed the same, only not so frequently; the cubes being generally formed immediately. Mitscherlich has shown that common salt forms hexagonal plates at very low temperatures. But in the present case the temperature was that of the atmosphere. Did the cold produced by the evaporation influence it?—*Poggendorff's Ann.* No. x., 1835.

### III.—*Uric Acid Calculi in the Biliary Canals.*

DR. AUBE in dissecting the *Lucanus Capreolus* found two small gray rough calculi with a crystalline appearance, which were found by Audouin to consist of uric acid. These vessels, to which the name of biliary has been given by modern anatomists, are small insulated canals, commonly in the Caecum, and open behind the stomach either by a single mouth or by two. Sometimes one of these extremities opens near the anus. Meckel considered that they secreted a liquid similar to the urine, while Gaede conceived that they were absorbents. The circumstance of uric acid calculi being found in them, however, supports the opinion of Meckel.—*Bibliot. Univers.*, April, 1836.

### IV.—*Effect of the price of Corn upon the Population.*

M. DUPIN has examined this question, by taking the average from the year 1817 to 1832. The mean annual price of corn during that period has varied from 36 francs 16 cents to 154 fr. 49 c. per hectrolitre. Taking the average of deaths for the 6 years when corn was dear and 6 when it was cheap, we have

Mean Price.	Annual Deaths.
25 fr. .06 c. $\frac{1}{2}$	25023
16 .44	24950

Thus there is only a difference of 73 deaths for 1 million of inhabitants. The following shews the influence of the same cause upon the births:—

Mean Price.	Annual Births.
24 fr. 68 c.	30647
16 44	31047

In 1817, the number of marriages diminished 918 per million.—*L'Institut.*, 164.

#### V.—*Impurity of Sulphuric Acid.*

It has been already stated, that English sulphuric acid contains arsenic (*Records*, ii. 73.) I have found it easy to prove that it contains muriatic acid, by placing a small portion of it in a tube with a vegetable substance, such as an oil, and applying heat; speedily a very strong odour of chlorine is perceptible. The methods by which these two acids may be removed are sufficiently obvious. But, besides these, it is well known to contain nitric acid, which is more difficult of separation. Barruel (*Jour. de Chim. Medic.*, ii. 184.) proved the presence of this acid, by its power of dissolving platinum. This effect, I have no doubt, was the consequence of the action of muriatic and nitric acids, 1000 grs. of sulphuric acid dissolving 0.16 of platinum, and was not owing to the sulphuric acid assuming the function of muriatic acid as Barruel seems to think. He found also, that if 2 ounces of sulphuric acid were heated for 2½ hours with 3 grammes of sulphur at a temperature of about 300°, no nitric acid could be detected on cooling the acid, shewing that it had been all decomposed. Hence, an obvious method of removing nitric acid, which is absolutely necessary when working with indigo, especially in manufactures. Barruel conceives that the acid may be hyponitrous and not nitric acid.—EDRR.

#### VI.—*Test for Strychnin.*

ARTUS recommends sulphocyanodide of potassium as a test for strychnin. The resulting salt consists of fine crystals. Unfortunately, however, quinin affords a similar product; Winckler recommends, therefore, corrosive sublimate as a more distinctive test. The mercury can readily be separated from the precipitate by a current of sulphuretted hydrogen, the muriate of strychnin remaining in solution.—*Buchner's Repertorium* iii., 397.

#### VII.—*Fossil Flowers.*

PROFESSOR GOPPERT, of Breslaw and Hrn-Keferstein, have obtained flowers from the brown coal of Wetterau in which the anthers and pollen are distinct. These will be described in the next number of the transactions of the Leopold Academy.—*Poggendorff's Ann.* xxxvi., 655.

VIII.—*Composition of Silk.*

MULDER of Rotterdam has lately made an analysis of silk. Some yellow Neapolitan silk was boiled with distilled water until infusion of galls no longer produced any precipitate; the solution was then evaporated,—a thick gray substance remained. When treated with water a portion of this matter dissolved; it was *gelatin*: but the greater part remained insoluble; it was *albumen*. The boiled silk being treated with absolute alcohol afforded some yellow flocks, *cerin*. The silk was then treated with hot ether. On evaporating the latter a colourless residue remained. Digestion in weak caustic potash took up some fatty matter from this residue, and when boiled with it some resin was separated; a red colouring matter was left undissolved. The silk was then boiled with concentrated acetic acid. Some albumen was taken up; the remainder was *fibrin of silk*. When silk is distilled with dilute sulphuric acid a peculiar acid comes over in minute quantities, which Mulder terms *bombic acid*. The constituents of yellow silk are—fibrin of silk 53·37, gelatin 20·66, albumen 24·43, cerin 1·39, colouring matter 0·05, fat and resin 0·10. —*Poggendorff's Annalen*, xxxvii. 594.

IX.—*Analysis of Phosphate of Lead.* By Dr. R. D. THOMSON.

This specimen was from Lead Hills. Sp. Gr. 6·631.

Its constituents are—

Chlorine . . . . .	2·656
Lead . . . . .	7·668
Protoxide of Lead . . . . .	69·636
Phosphoric acid . . . . .	17·626
Protoxide of iron . . . . .	2·008
Water . . . . .	0·800
Silica . . . . .	0·400

100·794

X.—*Effect of Alkalies &c. on Vegetables.*

The following has been forwarded by a correspondent:—

“ DURING last summer I performed a series of experiments with a view to ascertain what effect various substances had in supporting vegetable growth; for this purpose I procured several plants and placed them in vessels of water, adding to each various proportions of the alkalis, salts and other matters. Now, I found that more than one thousandth part of potassa, soda or their salts, would prove injurious, rendering the plant sickly or destroying it according to the strength of the solution. By adding a thousandth part I found the plant invigorated and decidedly more healthy than those parts which were in water alone; some of these plants existed in these vehicles for six or eight weeks, and, to all appearance, would longer had I prolonged the experiment; but they required renewing with the stimulents after the space of 12, 24, or 36 hours, according to the

size of the plant ; for water rendered alkaline by carbonate of soda, for instance, to the strength above mentioned, although it would at the time turn reddened litmus paper blue and display its presence on tumeric paper, yet, after a lapse of some hours, the water ceased to possess these properties, and even the plants if cut transversely near their lower part, although the alkali had been absorbed, would denote the presence of a free acid. Beneficial as were the alkalis, still more visible development ensued from those vessels wherein I had placed the oxide of iron and of zinc ; these would bear a larger quantity than the former, as they are less soluble in water and not so abundantly absorbed—still, after some time standing it seemed their solubility was increased, probably from the secretion of some acid from the plant converting the oxide into salt soluble in water, consequently, more readily absorbed into the plant. That plants are capable of secreting an acid, or that water is capable of abstracting one, cannot be doubted, as the alkaline waters, after some time standing, would produce an acid effect on the tests.”

#### XI.—*French School of Pharmacy.*

The object of this school is to teach all the sciences connected with Pharmacy, and to receive such apothecaries as in 4 trials prove that they possess the requisite knowledge for exercising this profession. Every candidate must produce certificates of his having studied for 8 years—of his having attained his 25th year, and must place in the hands of the treasurer the sum of 1300 francs (£54). Since 1830, a practical school has been formed, where the students admitted by competition are exercised in chemical and pharmaceutical manipulations. The school consists of a Director, a Joint-Director, 10 Professors, of which 4 are Joint-Professors, and a Treasurer. The chairs are *General Chemistry*, Bussy ; *Organic Chemistry*, Gaultier de Claubry ; *Pharmacy*, Lecanu, Chevalier ; *Mineralogy*, Pelletier ; *Natural History* (Zoology), Guilbert ; *Toxicology*, Caventou ; *Physics*, Soubeiran ; *Natural History* (Vegetable), Guibourt ; *Botany* (Organism and Physiology), Guiant ; *Botany* (Descriptive Botany), Clarion.

The necessary examinations are two upon theory, one of which is upon the principles of the art, the other upon the botany and natural history of simple drugs ; the third and fourth on the practice of the art last for four days, and consist of at least 10 chemical or pharmaceutical operations, which the candidate must perform himself, describing the process, materials, and results. If at one of the examinations he is not found competent he is remanded for three months. At the third trial the adjournment is for a year. The interval between each examination is a month. Unless two thirds of the votes are in his favour he is rejected. *The examinations are public.* In 1835 ninety apothecaries were received.

The existence of this establishment then enables every apothecary in France to be a chemist ; while in England who ever heard of an apothecary being a chemist, or of doing any thing for the improvement of pharmacy ? This anomaly does not arise from the want of

talent in our apothecaries, but from the miserable drudgery to which they are subjected (five year's apprenticeships), at the very time of life when they should be busily employed in the chemical and pharmaceutical laboratory under the eye of the scientific chemist.

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XII.—*Death of Professor Geiger.*

THIS distinguished pharmaceutical chemist and professor of pharmacy at Heidelberg, died on the 19th of January last, in the 46th year of his age. He was the discoverer of concein and other alkaloids. By his death pharmacy has experienced a heavy loss.

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XIII.—*Alpine Plants of Cote D'Or.*

THE greatest elevation in this department is 617 metres (2024½ feet) above the level of the sea. The soil is calcareous and rocky. In the vallies the following plants are met with:—*Swertia perennis*; *Cineraria sibirica*; *Polystichum thelypteris*; *Schoenus nigricans*; *Selinum carvifolia*; *Gentiana germanica*; *G. ciliata*; *G. pneumonanthe*; *Linaria alpina*. On the sides and tops of the hills the following occur:—*Daphne cneorum*; *Cypripedium calceolus*; *Aster amellus*; *Buphtalmum salicifolium*; *Iberis durandii*; *Arbutus uva-ursi*; *Dianthus superbus*; *Hieracium praemorsum*; *Carlina chamaeleon*; *Rubus saxatilis*; *R. tomentosus*.—*Bib. Univer., May, 1836.*

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XIV.—*Ozokerite, a mineral substance.*

THIS substance was first noticed in 1833 by Dr. Meyer. Its name was applied by Professor Glocker. It is found in Moldavia near Slanik, below sandstone, near coal and rock salt. It is also found near Gresten in Austria. At Slanik it occurs in irregular pieces; texture conchoidal; fracture slightly pearly; in thick slices the colour is brown-red, and in thin slices it is brown or yellowish brown. The smell is agreeable, similar to that of petroleum. It is soft and similar to wax. Its density is .953 at 59°. It is soluble in sulphuret of carbon, ether, naphtha, oil of turpentine, but it is very little soluble in alcohol, or even in boiling alcohol. It appears, therefore, to be a variety of black bitumen. It boils at 410°. It consists of hydrogen 13.787, carbon 86.204, according to Professor Schrötter.—*Bib. Univer., May, 1836.*

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XV.—*Additional Notice of an Oxide similar to Donium.* By  
H. S. BOASE, M. D.

*Penzance, 18th June, 1836.*

SIR,—I send you herewith, as requested, some of my oxide of *Treenium*, and think that it will be found to correspond with Mr. Richardson's, oxide of *Donium*.\* I regret exceedingly having been

\* See some remarks on this substance, from which it would appear, that it contains matter distinct from alumina although in very minute quantity, p. 28.—EDIT.

thus prematurely called upon to announce this substance, before it had been subjected to a more detailed and rigid examination. Will you have the goodness to publish the following remarks, as an appendix to my former hasty communication?

Solutions of the neutral salts of Treenium do not give precipitates with hydrosulphuret of ammonia, if the latter be as free as possible from an excess of alkali. It appears to be requisite that the oxide should be previously separated by an alkali, and then the hydrosulphuret produces an abundant dark green precipitate.

The solutions, concentrated by heat, do not afford crystals; and, if the evaporation be carried beyond a certain point, they gelatinize, and in this state cannot be re-dissolved by adding more water, nor indeed perfectly even by an excess of acid. An increase of heat drives off the acid, appearing to effect the perfect decomposition of these salts.

The oxide obtained from these salts by alkalis is easily dissolved by the stronger acids whilst in the state of hydrate: but when dried, even at  $212^{\circ}$ , its solubility is greatly impaired. The alkalis will dissolve a portion of the recently formed hydrate, but not sufficiently so to render it an useful property in analysis. The precipitation by alkalis cannot be completely performed, for, whatever care be taken to attain this object, the remaining liquid will be found to contain no inconsiderable quantity of the oxide. This circumstance, combined with the impossibility of obtaining an unaltered solid salt (at least by the ordinary means, perhaps it may be procured under the receiver of an air pump), renders the examination of this substance difficult. And the difficulty is farther increased by its saline solutions not affording insoluble salts with the ordinary tests used for that purpose. Triphosphate of soda only causes a slight cloudiness, not much exceeding an opalescence of the liquid: which fact, however, as the solution of the salt was neutral, serves to distinguish this substance from alumina, with which it is associated and so nearly allied.

Oxalate of ammonia gives no precipitate in a dilute solution; in a concentrated one flocculi may be separated, but are re-dissolved when washed on the filter. The carbonates afford the most satisfactory appearance, and therefore the following experiment was made:

Five grains bicarbonate of potassa (previously dried between bibulous paper) were dissolved in water, and neutral nitrate of treenium, added drop and drop till no farther change. A brisk effervescence followed each addition of the nitrate, and an abundant yellow flocculent precipitate was collected on a filter, and well washed. Dried at  $212^{\circ}$  it weighed 0.85 grs., and its colour nearly approached that of orange yellow. It effervesced in acids, and even a minute portion gave a copious gelatinous salt, which, decomposed by excess of ammonia, afforded a dark green precipitate, with hydrosulphuret of ammonia. These properties show that the carbonate contains the oxide of Treenium; but its weight curiously coincided with the quantity of alumina equivalent to 5 grs. of bi-carbonate of potassa.

Now, half of the carbonic acid of the precipitant is equal to 1.1 gr., therefore it is evident that this experiment is not a case of simple decomposition. The remaining liquid was therefore examined. I

omitted to observe that it had been boiled, to expel the carbonic acid, in solution, and a minute quantity of carbonate of Treenium thereby separated had been added to the rest on the filter. The solution evaporated to dryness, and, gently heated, evolved copious acid fumes, and fused = 4.6 grs. Water readily dissolved the residue, giving a dark brown powder = 0.25 grs. The solution was strongly alkaline; and the powder, dissolved in nitric acid, gave a copious white precipitate, with ferrocyanate of potassa; like the peroxide, mentioned in my last, obtained by fusing the yellowish oxide with chlorate of potassa.

In this case, then, as in the separation of the oxide from its salts by pure alkalis, the resulting saline substance holds some of the oxide in solution; showing a great tendency in Treenium to form double salts.

A more extensive knowledge of the properties of Treenium (should it prove a new substance) may furnish satisfactory data on which to calculate its *combining quantity*. Its present impracticable characters place it by the side of Tungsten and Titanium.

I remain, Sir, your obedient servant,  
HENRY S. BOASE.

To Dr. R. D. Thomson.

#### XVI.—*Showers of Frogs.*

SEVERAL notices have lately been brought before the French Academy, of showers of frogs having fallen at different times in different parts of France. Professor Pontus, of Cahors, states, that in the month of August, 1804; while distant three leagues from Toulouse, the sky being clear, suddenly a very thick cloud covered the horizon, and thunder and lightning came on. The cloud burst over the road about 60 toises (383.7 feet) from the place where M. Pontus was. Two gentlemen returning from Toulouse were surprised by being exposed not only to a storm but to a shower of frogs. Pontus states that he saw the young frogs on their cloaks. When the diligence in which he was travelling arrived at the place where the storm burst, the road and the fields alongside of it were observed full of frogs, which equalled in bulk from one to two cubic inches, and consisted of three or four layers placed one above the other. The feet of the horses and the wheels of the carriage killed several thousands. The diligence travelled for a quarter of an hour at least along this living road, the horses being at a trot.—(*L'Institut*, 166.)

#### XVII.—*Luminous Vibrations in Diaphanous Media.*

M. Lamé has endeavoured to deduce the laws of the equilibrium of the ethereal fluid in the interior of diaphanous bodies, from data which he considers have been ascertained in regard to the theory of light, and has inferred that the elasticity of the ether varies in proportion to its density, and that ponderable particles act upon the portion of ether situated at those places where the luminous vibrations

may be propagated by a repulsive force, the intensity of which varies inversely as the square of the distance. These conclusions depend upon two facts. 1. That diaphanous bodies exist in nature. 2. That the particles of ether during luminous vibrations oscillate on the surface of the undulations.

He has further deduced, from experiments and calculations, that under the influence of a single species of light, the ether which surrounds ponderable particles may be divided into an infinite number of different systems whose motions are isochronal.

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#### XVIII.—*Globules of the Zannichellia palustris.*

M. Pouchet has observed in the juice of the common horned pond weed, a plant very frequent in Britain, two kinds of moveable vesicles, the one opaque and bristly, the other transparent and smooth. They possess a similar structure with the vesicles of the pollen. In the interior of the smooth vesicles, he observed smaller secondary vesicles and very fine corpuscles. These corpuscles analogous to those which exist in the grains of the pollen are most frequently moveable; they are separated from each other, but sometimes united in one irregular globular mass they move altogether.

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#### XIX.—*White Crystalline Grains in the Intestines.*

THE body of a woman, who was supposed to have been poisoned, was exhumated. The excrementitious matter was well washed with repeated additions of water. M. Braconnot observed in the residue a white sandy substance, which disengaged a fetid odour when thrown on charcoal. It consisted of grains, each of which was a tetrahedral prism, terminated by a pyramid with two, three, or four faces. The colourless crystals when exposed to the air lost their transparency. They were not sensibly soluble in water. Warm dilute nitric acid dissolved them readily, with a slight effervescence. Ammonia poured into the solution formed a white precipitate, which disappeared on the addition of dilute sulphuric acid. When exposed to heat on platinum foil they lost their transparency, and fused into a grayish residue. Before the blowpipe they fused into a white enamel. With potash they give out an ammoniacal smell, and leave magnesia; heated in a glass tube with a little muriatic acid, they afford a sublimate of sal ammoniac. Before the blowpipe with nitrate of cobalt a reddish glass is produced, and with boracic acid and iron phosphuret of iron is formed. Hence they appear to be the ammoniaco magnesian phosphate.

Denis, Henri, and Guibourt found some crystals in the intestines likewise, in a case which they at first mistook for arsenic, but they did not determine their composition.

Braconnot once observed similar crystals in a case of poisoning by arsenic. Warm dilute nitric acid dissolved them, but the abundant flocky precipitate formed with solution by ammonia was insoluble in sulphuric acid, which converted it into sulphate of lime. They

fused easily before the blowpipe into a white opaque globule. Hence he concluded that they consisted of neutral phosphate of lime.—*Jour. de Chim. Med.* i., 179.

XX.—*Two Concretions found in the knee of an Old Man.*

ANTHRITIC concretions were found by Dr. Wollaston to consist of urate of soda, while some, according to Guyton and Fourcroy, were composed of phosphate of lime. M. Lassaigne had two sent him which were taken from the knee of a man aged 70. One of them weighed  $2\frac{1}{3}$  grains. It was insoluble in cold or hot water and in alcohol. Caustic potash had no action upon it. When heated with nitric acid no colour was produced indicating the absence of uric acid, which was also proved by the negative action of potash. Muriatic acid dissolved it partly with slight effervescence, leaving a white opalescent animal matter, which, when submitted to the action of boiling water, was transformed into gelatine.

Ammonia precipitated from the muriatic acid solution, gelatinous flocks of phosphate of lime. The remaining liquid afforded a precipitate with oxalate of ammonia indicating the presence of lime, which had existed in the concretion in the state of carbonate. The concretion was analyzed quantitatively by calcining a portion, dissolving the residue in muriatic acid, and precipitating the phosphate of lime by ammonia. The constituents are,

Animal matter soluble in boiling water	37·2
Phosphate of lime . . . . .	49·9
Carbonate of lime . . . . .	12·9
	100·0

XXI.—*Analysis of Copper Pyrites.* By Mr. THOMAS RICHARDSON.

Specific Gravity . . . . .	4·166
Sulphur . . . . .	34·70=2·12 atoms.
Copper . . . . .	32·75=1·00
Iron . . . . .	32·80=1·14
	100·25

XXII.—*Notices of New Books.*

Mr. John Weale, Architectural Library, will shortly publish a Supplementary Part to the original Edition of STUART'S ATHENS, containing the very curious plate wanting in the second volume of all the copies extant, together with several other plates from drawings by Sir J. L. Chantrey, &c.

SCIENTIFIC MEMOIRS; selected and translated from Foreign Journals, and from the Transactions of Foreign Academies and Learned Societies: Edited by RICHARD TAYLOR, F.L.S., G.S., and Astr. S.

# Meteorological Journal, — For June.

Kept at the Manse of the Parish of Abbey St. Bathans, Berwickshire. Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea.  
By the Rev. JOHN WALLACE.

DATE.	THERMOMETER.			HYGROMETER (Leslie's).			BAROMETER. At 32° of Fahrenheit.			Rain in Inches Weekly.	Direction of Wind at X. A. M.	REMARKS.
	IX. A. M.	III. P. M.	X. P. M.	IX. A. M.	III. P. M.	X. P. M.	IX. A. M.	X. A. M.	III. P. M.			
1855.												
We.	49½	50½	46½	18	18	7	29.597	29.563	29.555	29.468	N. E.	Calm, cloudy, in the evening the wind rising in sudden gusts.
Th.	54½	50½	47	20	19	0	29.319	29.310	29.234	29.156	E. by S.	Brisk wind, overcast, rain in the evening.
Fr.	50	55	51½	0	0	3	28.966	28.959	28.892	28.845	E.	Calm, frequent and heavy showers,
Sa.	57½	50	58	13	21	6	28.805	28.803	28.798	28.839	E. by S.	Brisk wind, showery, wind veering southward, evening calm and cloudy.
Su.	57½	58½	59	19	19	25	28.946	28.961	29.067	29.092	W. S. W.	Brisk wind, sky overspread with cirrostratus, evening calm, deposition.
Mo.	53	56	56½	23	28	20	29.253	29.244	29.141	29.078	W.	Brisk wind, A.M. overcast, P.M. rain, evening calm and showery.
Tu.	53	53½	57½	31	32	39	29.128	29.153	29.141	29.078	SW by W.	Brisk wind, A.M. cumuli prevalent, P.M. cirros, passing into cirroc. evg. calm [and nearly cloudless.
We.	56½	58½	64	22	22	33	28.975	28.969	28.922	28.939	S. E.	Gentle wind, cumuli prevalent, evening clear.
Th.	55	55½	57½	17	20	0	28.931	28.935	28.941	28.916	S. W.	Brisk wind, overcast, P.M. rain.
Fr.	54	58	61	5	21	4	28.970	28.984	29.070	29.136	S. W.	Strong wind, A.M. showery, P.M. sky veiled with cirros, soft clouds below.
Sa.	55½	57½	56½	2	6	2	29.080	29.072	29.029	29.082	S. E.	Calm, A.M. frequent showers, P.M. a sharp thunderstorm, evg. gradually clear.
Su.	54½	54½	57½	12	30	20	29.251	29.223	29.374	29.480	S. E.	Calm, frequent and heavy showers, in the evening the sky streaked with cirros.
Mo.	61	61	64½	16	17	27	29.629	29.632	29.568	29.513	W.	Calm, cloudy, and occasionally overcast.
New M Tu.	14	59	66	15	15	24	29.550	29.574	29.625	29.637	S. W. by S.	Gentle breeze, sky covered with cirri, cirros, and cirroc, evening calm.
We.	65	65	68	23	26	38	29.436	29.426	29.369	29.289	S. E.	Very calm, A.M. sky of a thunderous aspect, P.M. cirri prevalent, evg. cloudless
Th.	64½	65	68½	23	26	38	29.275	29.276	29.296	29.293	S. W.	Very calm, A.M. sky of a thunderous aspect, P.M. cirri prevalent, evg. cloudless
Fr.	62½	65	64½	24	33	27	29.326	29.319	29.278	29.207	S. W.	Very calm, sky cloudy, overcast, of thunderous aspect P.M. distant thunder.
Sa.	51½	52½	55½	36	39	52	29.318	29.314	29.237	29.274	S. E.	Very calm, A.M. thick fog with slight drizzling rain, P.M. partially clear.
Su.	50	49	52½	30	33	26	29.160	29.156	29.064	29.120	E. N. E.	Very calm, A.M. cumuli prevalent, P.M. fair, evening cloudless.
Mo.	53	54	58½	18	21	0	29.173	29.199	29.266	29.293	NW by W	Calm, A.M. frequent and heavy showers, P.M. fair, evening calm.
Tu.	50	51½	58½	30	33	26	29.323	29.311	29.345	29.266	NW by W	Brisk wind, cumuli abundant, in the evening gentle breeze.
We.	56½	58½	54½	18	21	27	29.190	29.189	29.021	28.957	S.	Gentle breeze, overcast, frequent showers.
Th.	56½	57	60½	14	17	27	28.914	28.931	28.863	28.855	SW by W.	Strong wind, soft cumulous clouds abundant, with occasional showers.
Fr.	56	57	57½	15	19	25	28.968	28.968	28.964	29.039	SW by W.	Strong wind, cloudy, showery, P.M. gentle wind, sky clear, evg. calm.
Sa.	54	58½	46	33	34	44	29.138	29.153	29.252	29.382	W.	Boisterous wind, cirrostratus prevalent, wind gradually hushed.
Su.	53	54	58½	33	30	47	29.594	29.614	29.674	29.721	W. N. W.	Gentle wind, A.M. cumuli floating on a blue sky, P.M. cirrostratus prevalent.
Sa.	56	53	63	21	21	25	29.659	29.648	29.609	29.547	S. W.	Brisk wind, A.M. cumuli abundant, P.M. gradually overcast, in evening rain.
Mo.	62	63	65	36	40	45	29.447	29.462	29.492	29.590	W. by N.	Gentle wind, A.M. showery, P.M. gradually clearing, evening calm and clear.
Tu.	58	57	61	36	40	45	29.637	29.745	29.654	29.711	W. by N.	Gentle wind, cumuli floating on a blue sky, evening clear.
We.	56	60	47	32	35	15	29.798	29.656	29.607	29.607	SW by W.	Calm, A.M. overcast and lowering, in the evening rain.
Th.	60	62½	59½									
Mean.	56	57-1	59-2	18	21	25	29.258	29.218	29.245	29.254		

Means { Therm. 53°·5 } Mean temperature of spring water 46°½  
 for the Hygr. 13° } Mean point of deposition 48°·4  
 Month { Bar. 29·256 in. } Moisture in a cubic inch of air = 0·0228 grs.

# RECORDS

OF

## GENERAL SCIENCE.

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### ARTICLE I.

#### *Biographical Account of Sir Humphry Davy, Bart.*

(Concluded from page 93.)

THE week after the paper respecting the rotations of the electro magnetic wire was read, Davy communicated to the Royal Society a paper by Mr. Faraday, "on Fluid Chlorine." This was followed by a successful attempt at liquifying several gases. The idea appears to have been suggested by Davy; but to all who are acquainted with the ingenuity of Mr. Faraday, there can be little doubt that the suggestion to him was the utmost that was requisite. He succeeded in condensing sulphurous and sulphuretted hydrogen, carbonic acid, euchlorine, nitrous oxide, cyanogen and ammonia.

In July, 1823, Sir Humphry made an excursion into Ireland and Scotland, in company with Dr. Wollaston, who also had acquired a taste for fishing, though at a late period of his life.

From 1823 to 1826, he presented the following papers to the Royal Society:—"On the corrosion of copper sheeting by sea water, and on the methods of preventing this effect; and on their application to ships of war and other ships."

"Additional experiments and observations on the application of electrical combinations to the preservation of copper sheathing of ships and to other purposes."

"Further observations on the preservation of metals by electro chemical means."

The Bakerian lecture for 1826, "On the relation of electrical and chemical changes."

These were important papers, having for their object the application of zinc protectors to the copper sheathing of ships, so as to prevent the latter from being corroded by the action of sea water. As the plan proposed appeared fraught with so many important consequences to ship owners, it was immediately applied to practice. Several vessels were sent on long voyages under the protecting influence of minute portions of zinc, equal together to  $\frac{1}{104}$  or  $\frac{1}{70}$  of the surface of the copper. The copper was found to be completely protected—not a particle of it had corroded—but, in some instances, the accumulation of sea weeds and shell fish in consequence of the surface of the copper being covered with a coating of zinc (for such is the consequence of applying even a mere point of zinc to a sheet of copper in a saline solution) was so very great as actually to impede the sailing of the vessel. This appears to have been considered a fatal objection to the application of his important discovery—as it has never been acted on in more than two or three cases—although it is sufficiently obvious that methods could readily be contrived for removing accumulations of the nature in question from the bottom. If copper protectors were used, not only would a great saving accrue to the country but likewise to the interests of humanity.

In June, 1824, Sir Humphry Davy made an excursion to Norway, and through Sweden, Denmark, Holstein and Hanover. During this tour he met with several distinguished men of science, whom he particularly notices. At Bremen he "dined with Dr. Olbers, and saw with much pleasure the telescope with which he discovered his two new planets, and met Gauss. *Olbers* gave us an excellent dinner, and is a most amiable and enlightened philosopher; I spent a very pleasant day." "I am rejoiced that I made the excursion to Altona and Bremen: it has given me a better idea of human nature; for *Schumacher*, *Olbers* and *Gauss* appear to me no less amiable as men than distinguished as philosophers, and they have all the simplicity, goodness of heart and urbanity of manners, which ought to make us proud of their name and of the influence of intellect, and scientific pursuits upon the morals, the habits, and the

affections." Of *Oersted* he says, he "is chiefly distinguished by his discovery of electro magnetism. He was a man of simple manners, of no pretensions, and not of extensive resources; but ingenious and a little of a German metaphysician." "*Berzelius* was the worthy countryman of *Scheele*, and certainly one of the great ornaments of the age. Indefatigable in labour, accurate in manipulation, no one has worked with more profit. His manner was not distinguished, his appearance rather coarse, and his conversation was limited much to his own subjects."

In 1826, Davy began to be threatened with symptoms of paralysis. He complained frequently of uneasy feeling and slight numbness of the right hand, and sometimes pain of his forearm, shooting up to the chest with occasional inordinate action of the heart, and occasional pain and weakness of the right leg. While delivering his last discourse to the Royal Society, at the anniversary dinner on St. Andrew's day, it was done with such effort that drops of sweat flowed down his countenance, and those who were near him were apprehensive of his having an apoplectic attack—and he was so unwell afterwards that he was unable to attend the dinner of the society. In December following, he was affected with a paralytic seizure, from which, however, he recovered as his strength improved.

On the 22nd January, 1827, he had so far recovered as to be able to start for the continent, in company with his brother.

Notwithstanding the great severity of the winter he passed through France, and reached Ravenna in safety. In March, his brother left him for his official station at Corfu, considering that his health being so much restored he might be left with safety. There he amused himself with making observations on natural history—examining the torpedo—the double snipe—the generation of the eel. From Ravenna he retreated before the heats of summer to his former haunts among the eastern Alps; his sufferings being very great at this period as the notices in his journal testify. His situation, however, was melancholy; for he was in a foreign country, and had no person to exchange a word with; moreover, he was ill, and had no physician if not to cure his complaints, at least to palliate his sufferings,

and sympathize with him amid the agonies of his disease. Yet, destitute of these comforts—nay, necessities, as we might term them in his weakly state, his mind was supported by a constant attention to the scenery around, and the noble pursuits of science.

From Ravenna he proceeded to Laybach in Illyria, where he arrived on the 19th of April. During this journey, and his stay in this part of the country, he took much exercise in fishing and shooting. Amid these scenes he delighted to study the habits and nature of the various fishes which he killed. His journal, of which extracts are now published, is full of such references. But the fatigue which he underwent appears to have acted otherwise than as a restorative, and his frequently registered expressions of suffering exhibit too plainly what the philosopher was enduring. We find him incapable of enjoying the finest mountain scenery, such as he used formerly to delight in, and his expressions refer more frequently to the past than to the present. “Ah! could I recover,” he exclaims, “any thing like that freshness of mind which I possessed at twenty-five, and which, like the dawning morning covered all objects, and nourished all things that grew, and in which they were more beautiful than in mid sunshine, what could I not give? All that I have gained in an active and not unprofitable life. How well I remember that delightful season, when full of power, I sought for power in others; and power was sympathy, and sympathy was power; when the dead and the unknown, the great of other ages and of distant places were made, by the force of the imagination, my companions and friends; when every voice seemed one of praise and love; when every flower had the bloom and odour of the rose; and every spray or plant seemed either the poet’s laurel or the civic oak, which appeared to offer themselves as wreaths to adorn my throbbing brow. But, alas, this cannot be!” Such are the sentiments expressed by Physicus in his “Last days of a Philosopher,” but which are obviously the result of his own feelings.

Towards the end of September his symptoms were aggravated. At Baden, he says, “The scenery is very beautiful, and for a person well, or becoming convalescent, it

would be a beautiful place ; but I fear my light of life is burned out." From Salzburgh, he had previously written to Mr. Gilbert, resigning the chair of the Royal Society. "I have gained strength," he observes, "under the most favourable circumstances, very slowly, and though I have had no new attack, and have regained, to a certain extent, the use of my limbs, yet the tendency of the system to accumulate blood in the head still continues, and I am obliged to counteract it by a most rigid vegetable diet, and by frequent bleedings, with leeches and blisterings, which, of course, keep me very low. From my youth up, to last year, I had suffered more or less from a slight hemorrhoidal affection ; and the fulness of the vessels, there only a slight inconvenience, becomes a serious and dangerous evil in the head to which it seems to have been transferred. I am far from despairing of an ultimate recovery, but it must be a work of time ; and the vessels which have been over distended only very slowly regain their former dimensions and tone ; and for my recovery, not only diet, and regimen, and physical discipline, but a freedom from anxiety, and from all business, and all intellectual exertion, is absolutely required." He then concludes, by begging Mr. Gilbert to communicate his resignation to the Society. On the 6th of October, he returned to England in a very infirm state of health. He remained in this country till the last week of the following March. During a portion of this period, he paid two visits, one to a nobleman in Sussex, for whom he had a great regard ; the other to his friend Mr. Poole, in Somersetshire. But he did not enter into London society, to which he felt his strength inadequate.

By the advice of his friends, he decided on again visiting the continent, and, accordingly, on the 29th of March, 1828, he left London, accompanied by Mr. Tobin (now Dr. Tobin), the eldest son of his early friend, Mr. James Tobin. Passing through Austrian Flanders, they crossed from the Rhine to the Danube ; and from thence to Donanworth, proceeding southward ; the season not being sufficiently advanced to enjoy the Alpine country, they travelled rapidly to Laybach, where they arrived on the 4th of May. Here, he amused himself in fishing, and pursuing his journey leisurely, he followed up the same amusement. From

Aussee, in Styria, in June, he wrote his brother, "Notwithstanding the long, severe, and distressing malady under which I still labour, I am not entirely without the hope of ultimate recovery, and the few pleasures that I retain in this very state of earthly purgatory have principally reference to the enjoyments and prospects of my friends, and I indulge in the idea that you are well and happy, and *enjoying* a life which I can say I only *support*, supposing that it pleases Omniscience to preserve me for some ends which I cannot understand, but which I trust belong to the great plan of goodness and mercy belonging to his divine mind." It was thus that he reconciled himself to his sufferings. He next proceeded to Ischl, where he planned his "Consolations in Travel," a work which one cannot fail to admire, whether it be regarded as a piece of beautiful writing, or as the last efforts of a great and good man. At Wurzen he amused himself with writing a literary curiosity, viz., a romance, to which he affixed the title of "The Last of the O'Donoghues, an Irish Story." It is a kind of historical piece, the scene being Ross Castle on the Lakes of Killarney.

On the 30th of August, he arrived at Laybach. From thence, on the 6th of October, he went to Trieste expressly for the purpose of trying some experiments which he meditated on the torpedo. Here, through the attention of the English consul he was supplied with two recently caught torpedos. The result of this investigation constituted his last communication to the Royal Society. He sums up his views in it with regard to the different kinds of electricity. "*Common electricity* is excited upon non-conductors, and is readily carried off by conductors and imperfect conductors. *Voltaic electricity* is excited upon combinations of perfect and imperfect conductors. *Animal electricity* resides only in imperfect conductors forming the organs of living animals, and its objects in the economy of nature is to act on living animals. *Magnetism*, if it be a form of electricity, belongs only to perfect conductors, and in its modifications to a particular class of them." His brother following up the investigation concluded that the electricity of the torpedo is not of a peculiar kind; but the mode of its production he could not detect. He found that

the sensation imparted by this animal was similar to common electricity—that iodine was precipitated by its agency—and that its effect on the galvanometer and on steel in the spiral was similar to that of common electricity.\*

On the 31st October, he proceeded to Rome, where he arrived on the 18th November. Here he remained for several months in much the same state, till on the 20th February, he was seized with the severe attack which ultimately proved fatal. “That morning he felt better than usual, his pulse about 68, the tongue clean; the ordinary functions of the body well performed. After breakfast he had sat some time dictating an addition to the sixth dialogue. When he had finished it he attempted to rise to go into his bed-room, which was adjoining, but found that he could not stand, and that he had lost all power over his limbs, without pain of head or vertigo or loss of power of intellect, accompanied merely by a feeling of sickness of stomach. Medical aid was immediately had; leeches were applied to the head as if the brain had been affected; and a lowering (or as it is called antiphlogistic) plan of treatment was pursued, but with no good effect. He spent the night very restlessly, and the following morning the right side was quite powerless and the stomach much deranged.” He now gave up all hopes of recovery, and wrote to his brother at Malta to come and see him before his death. Dr. Davy arrived at Rome on the 16th March. The account of his feelings, on this occasion, are highly affecting. “I shall not attempt to describe my feelings on receiving his last letter, making known to me the illness of a brother who had acted the part of a father to me; whom I regarded as a brother, a teacher, and most kind friend; and to whom I necessarily owe very much of what I most valued in life. My anxiety naturally increased the nearer I came to Rome. In vain I sought for fresh letters and additional information at Naples. When I entered Rome I knew not where to find him; for his address in that city was not sent. I in vain went from one hotel to another, making inquiries, without being able to hear any thing of him. I fortunately recollected that his friend Morichini was a physician, and a resident in Rome. He was easily found, and presently I

\* Records, vol. i. 306.

had a comfortable message from him, that my brother that morning was rather better, accompanied with a direction to his lodging; and in a few minutes I was by his bed side. Never shall I forget the manner in which he received me; the joy which lighted up his pale and emaciated countenance; his cheerful looks and extreme kindness; and his endeavours to sooth a grief which I had not the power of controlling on finding him so ill, or rather at hearing him speak as if a dying man who had only a few hours to live, and who wished to use every moment of such precious time. With a most cheerful voice, a smile on his countenance, and most warm pressure of the hand, he bade me not be grieved, but consider the event as a philosopher." During their stay at Rome, Lady Davy arrived from England, bringing with her a copy of the second edition of "Salmonia," which gave him very great pleasure. His strength gradually improved under the lovely Italian climate, and by the 30th of April he was able to quit Rome for Geneva, for the purpose of removing from the increasing heat to a cooler climate. The party advanced by Florence to Genoa, where they arrived on the 12th of May. On the 28th they arrived at Geneva. Here he appeared to have improved, for on his arrival at the inn of La Couronne he merely reclined on a sofa, and occasionally walked to the window and looked out upon the lake, and "expressed a longing wish to throw a fly, as he had been before in the habit of doing, on his favourite Rhine. Here he learned the death of his old friend Dr. Thomas Young." Dr. Davy says, "I was not present when Lady Davy made the communication to him, but when I returned I saw him affected, and he told me how deeply he had been affected by it even to profusion of tears, and in a manner that was almost unaccountable. In a short time he recovered his composure, and conversed on indifferent matters. At five o'clock he dined at table and made a tolerable dinner. After dinner he was read to according to his custom. At nine o'clock he prepared to go to bed. In undressing he struck his elbow against the projecting arm of the sofa on which he sat. The effect was very extraordinary—he was suddenly seized with a universal tremor—he experienced an intense pain in the part struck—and a sensation he said as if he

were dying. He was got into bed as soon as possible; the painful sensations quickly subsided, and in a few minutes were entirely gone." No appearance of injury could be detected on the arm. He took an anodyne draught, and about half-past nine expressed a wish to be left alone. At half-past two he was insensible, and at 3, A. M., on the 29th May, 1829, he died. His funeral took place on the 1st June, and was attended by the literary and scientific men of Geneva. His remains were deposited in the burying ground of the city, without the walls, and close to the grave of Professor Pictet.

The precise cause of the complaint of which he died was never ascertained, as he made his brother promise that he should not open his body; being of opinion, that it was possible for sensation to remain in the animal fibre after the loss of irritability. He had also a great horror of being buried alive, and desired that his interment should not take place till after ten days; but it was found impossible to comply with his request, as it was contrary to the law of Geneva, and as signs of decomposition speedily shewed themselves.

It is remarkable, that the same year deprived England of three of her most distinguished philosophers, Young, Wollaston and Davy. The first was Foreign Secretary and the latter Secretaries of the Society—and all three foreign associates of the first class of the Institute of France.

Such is a sketch of the life of one of England's greatest ornaments. He was of middle stature, about five feet seven inches high, but appeared shorter perhaps from his compact and stout make; his countenance was very expressive—his voice was full toned and melodious—which is rather remarkable as he was destitute of a musical ear. He possessed very strong feelings; of a kind disposition with those whom he formed friendships, but latterly of an extremely irritable temper, and disagreeable bearing. He was always of a jealous disposition, which appears to have been encouraged by the fear that he should be anticipated in any of his discoveries; and, unfortunately, he too often displayed by his actions what were the feelings of his mind.

To point out the weaknesses in the character ought to be particularly attended to, by the biographer, for, unless this

is done, the grand end of biography is marred. But, while the imperfections of the subject of our memoir have been exhibited, we trust that the great benefits he conferred upon science have been detailed in such characters as may render them worthy of imitation; and thus, though he no longer resides among us, that rivals may be incited to start from the point where his labours ceased, and to act in subservience to the growing wants of society.

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## ARTICLE II.

*Experiments on the Absorption of Air by Water.* By THOMAS THOMSON, M.D., F.R.S.L. & E., &c., Regius Professor of Chemistry in the University of Glasgow.

Not being aware of any direct experiments upon the subjects mentioned in the title of this paper, I amused myself, during the early part of the present summer, in making a few trials to satisfy myself whether the opinions at present entertained on these subjects were entitled to confidence. I shall state my experiments on each of the different subjects in order.

### I.—*Of the quantity of Air contained in Clyde water.*

The city of Glasgow is supplied with water pumped out of the river Clyde, and conveyed to reservoirs in the higher parts of the town, from which it is conveyed in pipes to every house. As one of these pipes supplies my laboratory, I have only to turn a stock cock to obtain as much river water as I have occasion for.

1. I filled a retort, the belly of which held 168 cubic inches, and its throat and beak 75 cubic inches more, with river water, plunged the beak into a water trough, and placed a small inverted jar full of water over the extremity of the beak. I then boiled the water till it ceased to give out any air. I collected 5.25 cubic inches of air. Barometer at 29.5 inches. Thermometer 53°.

In this experiment 168 cubic inches of water gave out 5.23 cubic inches (making the requisite corrections) of air, supposing the barometer at 30 inches and the thermometer at 60°. The 75 cubic inches which filled the throat and beak

of the retort became hot, and no doubt gave out a little air ; but not much ; because as soon as the water in the retort began to boil briskly, the water in the throat and beak was driven out by the steam, and never boiled at all. Thus, it appears that 100 cubic inches of Clyde water contain 3·113 cubic inches of air.

The experiment being repeated in precisely the same way, the product of air was so nearly the same that it seems unnecessary to state the particulars minutely.

II.—*Composition of the Air thus extracted.*

I let up 100 volumes of this air into a small jar, filled with water and standing over the water-trough, and put into it a stick of phosphorus of such a length that it reached from the bottom to the top of the jar, and traversed all the air. In 24 hours the bulk of this air was reduced to 71·48 volumes of azotic gas. Hence, the air extricated from the water was composed of

71·48 volumes of azotic, and  
28·52 volumes of oxygen gas.

The air extricated during the second experiment, analyzed in the same way, was composed of

70·32 volumes of azotic, and  
29·68 volumes of oxygen gas.

If we take the mean of these two analyses, we get the constituents of the air extracted from Clyde water by boiling as follows :

Volumes of azotic gas . . .	70·9
Volumes of oxygen gas . . .	29·1
	100·0

III.—*Alteration produced on this Air when left standing on the water-trough.*

1. The 5·25 cubic inches of air extracted by boiling water from the Clyde, were put into a small cylindrical glass capable of holding 11 cubic inches, and left inverted over the water trough. Every 24 hours 1 cubic inch (or 100 volumes) of this air was taken out, and left till next day with a stick of phosphorus passing through it. The

following table shows the composition of the air after standing over the water.

	Azotic.	Oxygen.
1. Fresh extracted of . . . . .	71·48	+ 28·52
2. After one day . . . . .	74·43	+ 25·57
3. After two days . . . . .	75·38	+ 24·62
4. After three days . . . . .	77·51	+ 22·49
5. After four days . . . . .	80·97	+ 19·03

2. The 5·25 cubic inches extracted from the second quantity of water by boiling was treated in the same way: excepting that the 11 cubic inch jar containing the air, instead of standing open on the water-trough, was corked tight. The result was as follows:

	Azotic.	Oxygen.
1. Air newly extricated composed of . . . . .	70·32	+ 29·69
2. After one day . . . . .	72·5	+ 27·5
3. After two days . . . . .	73·44	+ 26·54
4. After three days . . . . .	73·35	+ 27·65
5. After four days . . . . .	77·43	+ 22·57

Here, as in the first case, the oxygen was absorbed more rapidly than the azotic gas; but the rapidity of this absorption was somewhat diminished by corking the glass in which the air was kept.

#### IV.—*Alteration produced on Common Air by leaving it standing in a glass vessel inverted on the water-trough.*

Curious to know whether a similar diminution in the quantity of oxygen in common air would take place when left standing over the water trough, as had taken place in the preceding experiments with air extricated from water by boiling, I put ten cubic inches of common air, collected at the window of my laboratory on a windy day, into a cylindrical glass jar and left it standing inverted over the water-trough, analyzing every day one cubic inch by means of phosphorus, till the whole was exhausted. The following table shows the result of these analyses.

	Azotic	Oxygen.
1. After standing 24 hours . . . . .	79·47	+ 20·53
2. After two days . . . . .	79·27	+ 20·73
3. After three days . . . . .	lost	
4. After four days . . . . .	79·65	+ 20·35

	Azotic.	Oxygen.
5. After five days . . . .	79·65	+ 20·35
6. After six days . . . .	82·99	+ 17·01
7. After seven days . . . .	80·71	+ 19·29
8. After eight days . . . .	80	+ 20
9. After nine days . . . .	80·84	+ 19·16
10. After ten days . . . .	82·26	+ 17·74

On the ninth day, after analyzing the gas, I dissolved some sulphate of iron in the water-trough. This is the reason of the greater proportion of azotic gas found in the last cubic inch of the air, which was analyzed on the tenth day.

If we compare these experiments with the former ones, we must be struck with the great difference between them. The air extracted from water by boiling is much richer in oxygen than common air, containing rather more than 29 per cent., while common air contains only 20 per cent. by volume. But this excess of oxygen diminishes rapidly; so that after four days it does not contain more than common air does.

Common air, on the contrary, may be left upon the water-trough for ten days without undergoing any sensible alteration in its composition. Indeed, I left nine cubic inches of air in a tube standing inverted over water, from the first of May to the 25th of that month, and found its constituents unaltered.

If we take the mean of the constituents of air from the preceding table, leaving out the last term, because the sulphate of iron had increased the quantity of oxygen absorbed, we obtain

Azotic gas . . . .	80·32	volumes.
Oxygen gas . . . .	19·68	,,
	100·00	

Now this differs very little from the composition of air. If we analyze air without removing previously the carbonic acid gas and the moisture which it contains, we always find the volume of its oxygen below 20 per cent.

#### V.—Absorption of Air by Water.

To determine the absorption of air by water I boiled a quantity of distilled water for a quarter of an hour, and

filled with it, while boiling hot, a small cylindrical glass jar: the capacity of the jar was about 11 cubic inches. I determined what bulk of the boiling water would when cold constitute 10 cubic inches. This quantity was put into the glass, and the rest of it being filled with mercury it was placed inverted on the shelf of a mercurial trough. As soon as the water was cold, a cubic inch of air was let up into the glass, and the whole was left one day, then two days, then three days, then four days, and at last eight days, in order to see how much of it would be absorbed by the water during that time. The following table exhibits the residue of the air, the original cubic inch being considered as divided into 10,000 parts :

	Residue.
1st trial . . . .	0·8709
2nd „ . . . .	0·9062
3rd „ . . . .	0·7207
4th „ . . . .	0·8659
5th „ . . . .	0·8673
6th „ . . . .	0·8566
7th „ . . . .	0·8069
8th „ . . . .	0·8178
9th „ . . . .	0·7990
10th „ . . . .	0·7672
	<hr/>
Mean . . . .	0·8278

I was very much surprised at the great discordance in these results, as every experiment was made exactly in the same way, and the air was always collected from the outside of the window of my laboratory at the commencement of each trial. The mean absorption in these trials by 100 cubic inches of newly boiled water is 1·722 cubic inches of air. The greatest absorption by 100 cubic inches of water was 2·793 cubic inches, and the least 0·938 cubic inches.

But it has been stated above that by boiling 100 cubic inches of the water of the Clyde, 3·113 cubic inches of air could be extricated. The mean absorption found amounts only to about the half of this quantity. The greatest absorption falls short of it by almost  $\frac{1}{10}$ th, while the smallest absorption amounts only to about a third of it. It is obvious from this that the absorption of air by water is a very slow process.

VI.—*Alteration produced in Air by standing over distilled and newly boiled Water.*

We have seen that when air is left for 25 days over common river water it undergoes no sensible alteration in its composition. But the case is very different when air is left in contact with newly boiled distilled water. I analyzed the residual air in the ten trials exhibited in the last section by leaving it in each case for 24 hours over the water-trough with a stick of phosphorus in contact with it. The diminution of volume was considered as the bulk of oxygen which it contained. The proper corrections being made for alterations in the barometer and thermometer which had taken place during the interval, the following table exhibits the volume of azotic and oxygen gases contained in these residues :

	Vol. of Oxygen.	Vol. of Azotic gas.
1st. . . . .	10·16	76·93
2nd. . . . .	16·72	73·90
3rd. . . . .	7·71	64·36
4th. . . . .	6·22	80·37
5th. . . . .	11·51	75·22
6th. . . . .	13·44	72·22
7th. . . . .	13·37	67·32
8th. . . . .	13·42	68·36
9th. . . . .	10·48	69·42
10th. . . . .	4·59	72·13

But, in order to be able to judge more accurately of the constitution of this residue, we shall suppose it to amount in each case to 100 volumes, and set down the number of volumes of oxygen and azotic gas of which it is composed :

	Vol. of Oxygen.	Vol. of Azotic gas.
1st. . . . .	11·67	88·33
2nd. . . . .	17·35	82·65
3rd. . . . .	10·70	89·30
4th. . . . .	7·18	92·82
5th. . . . .	13·27	86·73
6th. . . . .	15·70	84·30
7th. . . . .	16·57	83·43
8th. . . . .	16·41	83·59
9th. . . . .	13·12	86·88
10th. . . . .	5·99	94·01
Mean	<u>12·80</u>	<u>87·20</u>

The same diversity appears in the constitution of the residue as in the amount of the absorption. But the mean derived from the ten trials is, that the air after standing from 1 to 8 days on newly boiled water is composed of,

12·8 volumes oxygen gas,  
87·2 volumes azotic gas.

It is obvious from this, that a greater quantity of oxygen has been absorbed than of azotic gas, for the air before it was placed over the water (leaving out of view the carbonic acid and vapour which it contained) was composed of,

20 volumes oxygen,  
80 volumes azotic gas.

As the original volume of air was one cubic inch, which we suppose divided into 100 parts we will obtain the volume of oxygen and azotic gases absorbed in each trial by subtracting the volumes of oxygen in the table page 175 from 20, and the volumes of azotic gas from 80. The following table shows the result, and exhibits the volume of oxygen and azotic gas absorbed in each trial :

	Vols. of Oxygen absorbed.	Vols. of Azotic gas absorbed.
1st. . . . .	9·84	3·07
2nd. . . . .	3·28	6·10
3rd. . . . .	12·29	15·64
4th. . . . .	13·78	0
5th. . . . .	8·49	4·78
6th. . . . .	6·56	7·78
7th. . . . .	6·63	12·68
8th. . . . .	6·58	11·64
9th. . . . .	9·52	10·58
10th. . . . .	15·41	7·87

The want of coincidence in these trials is very surprising. But the mean of the whole is that ten cubic inches of water absorbed,

·0924 cubic inch of oxygen,  
·0801 cubic inch of azotic gas.

So that 100 cubic inches of water would absorb,

0·924 cubic inch of oxygen,  
0·801 cubic inch of azotic gas.

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1·725

100 volumes of the air thus absorbed would consist of,  
53·57 volumes oxygen,  
46·43 volumes azotic gas,

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100·00

But, it has been shewn in the first part of this paper, that the air extricated by boiling from the water of the Clyde was composed of,

29·1 volumes oxygen,  
70·9 volumes azotic gas.

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100·0

It follows from this, that the air extricated from water by boiling differs essentially in its composition from the air absorbed by water; the quantity of oxygen in it being very much diminished.

Let us suppose that water has absorbed a hundred volumes of air. They will consist, as appears from the preceding statement, of,

46·43 volumes of azotic gas,  
53·57 volumes of oxygen.

But, when these 100 volumes are extricated from water by boiling, they are reduced to 64·02 volumes, which consist of,

45·39 volumes of azotic gas,  
18·63 volumes of oxygen.

Two-thirds of the original oxygen gas absorbed by the water disappear, and only one-third remains. Doubtless this portion of oxygen which disappears is employed in the respiration of fishes, (if such an expression may be permitted,) and for various other important purposes of which we are at present ignorant.

If any reliance can be placed in these statements, it will follow that the difference between the absorbability of oxygen and azotic gas by water, is much greater than has hitherto been supposed. Oxygen must be about three times as absorbable by water as azotic gas.

If we compare the quantity and constitution of the air extricated from water by boiling, with the constitution of the air absorbed by newly boiled water, we shall find that 100 volumes of water absorb,

2·21 volumes of azotic gas,

2·65 volumes of oxygen.

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4·86

Now, let the absorbability of azotic gas by water =  $x$  and that of oxygen gas =  $y$ . We have from the constitution of the atmosphere,

$$\frac{4x}{5} = 2\cdot21. \quad \text{Hence } x = 2\cdot76.$$

$$\frac{y}{5} = 2\cdot65. \quad \text{Hence } y = 12\cdot25.$$

So that oxygen gas is about  $4\frac{1}{2}$  times as absorbable by water as azotic gas. But, as two-thirds of this oxygen gas absorbed by water disappear, the absorbability of oxygen gas appears only to be to that of azote as 4·5 to 2·76, or not so much as double.

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### ARTICLE III.

*On Manganic and Hypermanganic Acids, on Hyperchloric Acid, and the Salts of these Acids.* By E. MITSCHERLICH.\*

SCHEELE first observed a part of the phenomena, which, as I shall immediately show, are produced by two acids formed from manganese; and after him chemists of eminence have repeatedly turned their attention to the subject. Chevreul, Chevillot and Edwards, Forchhammer, Fromherz and Unverdorben, have added more or less interesting facts to those previously known, although they have by no means exhausted the subject. These compounds, however, would unquestionably have been long ago completely investigated, had not the great difficulty of obtaining the pure acid in sufficient quantity rendered their examination almost an impossibility. They are decomposed very easily by a great number of circumstances; their solutions cannot be filtered, nor their crystals laid upon paper, because they are instantaneously decomposed by organic substances.

Very distinct crystals, which I obtained of manganate of potash, enabled me to determine their shape, and as it was found to agree in every respect with that of the chromate, seleniate, and sulphate of potash, this circumstance, which

\* Poggendorff's Ann., xxv., 287. (The publication of this interesting paper is rendered necessary to illustrate that of Dr. Clark, printed in Records, vol. iii. 433, and vol. iv. 43.—EDIT.)

is particularly interesting in the elucidation of the connexion of the crystalline shape of bodies with their composition, induced me to investigate more closely these acids and their compounds.

I.—*On the action of Potash on the Binoxide of Manganese.*

When equal parts of potash and binoxide of manganese are ignited together, and the ignited mass treated with water, a green solution is obtained, which contains in solution, carbonate of potash, caustic potash, and a compound of potash with manganese in a higher degree of oxidation, while a brown powder remains undissolved. Oxygen is absorbed when the mass is ignited in contact with the air; as Edwards and Chevillot have already shewn. But the green compound may be obtained equally well, when the binoxide of manganese is ignited with potash in a retort shut up from the air. Thus 10 grammes of binoxide of manganese, heated with potash without the access of air, and treated with water, gave a solution, which, when the acid was decomposed, and the manganese precipitated and ignited, yielded 1 gramme of the red oxide (oxidum manganoso-manganicum.)

In this case, the higher degree of oxidation of the manganese is produced in the same manner in which the brown oxide of lead is formed from red lead, when the latter is treated with nitric acid, and the brown residue which is left, when the green compound is dissolved, consists of hydrated sesqui and binoxides of manganese, but whether mixed or chemically combined, I cannot venture to decide. The manganic acid is formed by a part of the binoxide giving up a portion of its oxygen to the remainder, by which it is changed to sesquioxide, but the quantity of manganic acid formed, shows that a portion of the binoxide remains undecomposed.

If we pour off the deep green solution, after allowing the brown insoluble portion to subside, and allow it to evaporate over sulphuric acid, under the exhausted receiver of an air pump, we obtain beautiful pure crystals of a green colour, mixed with abundance of crystals of hydrate and carbonate of potash. These crystals must be laid on porous tile or clay, which absorbs the moisture without producing decom-

position. If the solution be allowed to evaporate in contact with the atmosphere, red crystals, the composition of which, I shall attend to afterwards, may be formed by the action of the carbonic acid of the air.

If the green crystals be treated with water, a red solution is obtained, which, by evaporation, yields red crystals. The green crystals consist of manganate of potash, which is isomorphous with sulphate of potash, while the red have the same form as the perchlorate of potash. Accurate analyses have shown that both the perchloric acid and the highest degree of oxidation of manganese, contain 7 proportions of oxygen. It appears to me, therefore, to be convenient to denominate that degree of oxidation of manganese which corresponds with sulphuric, selenic, and chromic acids, *manganic acid*, while the highest degree of oxidation of manganese may be called *hypermanganic acid*, and that of chlorine *hyperchloric acid*, following the nomenclature of Gay Lussac with respect to *hypo-sulphuric acid*.\*

## II.—*Manganic Acid and its Salts.*

I had tried many ways of analyzing manganic and hypermanganic acids, when at last, I met with one as accurate as it was easy. It depends on the fact, that hypermanganic acid begins to decompose at the temperature of 86° F., and at 212° is completely changed into binoxide and oxygen gas, and as the manganate of potash when treated with water changes into hypermanganate of potash and binoxide of manganese, the compounds of manganic acid may be analyzed the same way. I add to the manganic or hypermanganic acid, sulphuric or nitric acid, collect the oxygen in a graduated tube over quick-silver, and reduce the gas obtained to what it would be at 32° F. and when the barometer stands at 30 inches.

0·705 gramme of manganate of potash, treated with dilute nitric acid, and boiled till the solution became colourless, gave 58·9 cubic centimetres dry oxygen gas, which is equivalent to 0·0844 gr. by weight. 100 parts of the salt had, therefore, given out 8·7 of oxygen when treated with nitric acid.

\* In this investigation I have been greatly aided in the preparation of the substances by my assistant, M. Wolf, a very skilful pharmacist.

1.204 gr. of manganate of potash, decomposed by muriatic acid and precipitated by carbonate of ammonia, yielded 0.459 gr. ignited red oxide of manganese; and the liquid evaporated to dryness and heated to redness, gave 0.882 chloride of potassium; or 100 grains would have given 46.34 potash, and 38.12 red oxide of manganese. These 38.12 parts of red oxide are equivalent to 44.30 of the binoxide, containing 15.95 of oxygen. Therefore, the oxygen extricated by the action of the nitric acid, is nearly half that contained in the residual manganese. Now, 46.34 potash contain 7.85 oxygen, that is half that of the binoxide of manganese, or  $\frac{1}{3}$  of that in the manganic acid. If we calculate from this the composition of the manganate of potash, we find it to be composed in 100 parts of 47.37 potash, 52.63 manganic acid.

When 52.63 parts of manganic acid are converted into binoxide of manganese, 8.03 parts of oxygen ought to be extricated; the increase of  $\frac{7}{10}$  in the result of the experiment is occasioned by the extreme facility with which the salt is decomposed, by which the potash is diminished in quantity.

The hydrated binoxide of manganese has already been discovered by Berthier, and prepared by various methods, but it was not known that it could be prepared by the decomposition of manganic and hypermanganic acids. When prepared from these acids by nitric acid, its colour is so deep brown as to appear almost black, but when prepared by sulphuric acid it is somewhat lighter. An unknown quantity of this oxide was ignited in a retort and the oxygen extricated was collected; this (the barometer standing at 30 inches and the thermometer at 32°) measured 46.2 C. C., or 0.06618 grammes. I could not succeed in converting it into red oxide in the retort, but when heated strongly in a platinum crucible, it gave out 0.049 gr. of oxygen, and became red. The red oxide obtained, weighed 0.954 gr. To be more certain of its amount, it was treated with sulphuric acid, and the solution evaporated to dryness and ignited. The sulphate of manganese weighed 1.8363 gr., corresponding to 0.9521 gr. red oxide. 0.954 gr. red oxide of manganese are obtained by igniting 1.083 gr. binoxide, as they contain 0.129 gr. oxygen. According to the experiment, the loss in the quantity employed was 0.115 gr.

This discrepancy, which amounts to somewhat more than one per cent., is caused by the difficulty of analyzing the binoxide; the experiment, however, clearly shows that the powder examined was binoxide of manganese.

In another experiment, 0.6525 gr. dried hydrous binoxide of manganese gave 0.4735 gr. red oxide, which is equivalent to 0.538 gr. binoxide. Consequently 0.1145 gr. water, which contain 0.1009 gr. oxygen, are combined with 0.538 gr. binoxide of manganese, containing 0.194 oxygen. The oxygen of the water, is therefore, to that of the binoxide of manganese as 1 to 2. The binoxide of manganese does not give off the last portion of its water till the oxygen begins to be extricated.

I have in vain tried to find a method of distinguishing the sesquioxide and the binoxide of manganese from one another, easier than by ignition. If the hydrated binoxide be treated with a solution of sulphurous acid in water, the greater part of it is converted into the sulphite of manganese, but a portion, sometimes greater and sometimes smaller, becomes sulphate of manganese. I have estimated the quantity of both, the one as sulphite the other as sulphate of barytes, and from the quantity of each have calculated the quantity of oxygen supplied to the sulphurous acid. By this method also, I have ascertained that the binoxide of manganese formed by the decomposition of the manganic and hypermanganic acids, is pure, containing no sesquioxide, as the latter would have oxidized only half as much sulphurous acid. This method of examining the binoxide is still more difficult than its estimation by ignition. It had already been observed by Heeren, that sulphuric acid is formed when the native binoxide is treated with sulphurous acid.

The crystals of the manganate of potash have the same secondary faces, and the same composition as the sulphate, seleniate, and chromate of potash, and show even to the most trifling minutiae the same modifications with respect to the size of the faces.

On account of the facility with which the manganate of potash is decomposed, no other salts of manganic acid can be made by means of it. Caustic soda, fused with binoxide of manganese, yields manganate of soda, which, however, is too soluble to be freed from the carbonated and caustic soda by crystallization. Nitrate of barytes fused with

binocide of manganese, yields manganate of barytes. If, to a solution of the hypermanganate of barytes a solution of barytes be added, and the liquid be allowed to stand a long time in a jar, which it about half fills, then green crystals separate themselves, which are manganate of barytes, and which, like the sulphate of barytes, are insoluble in water.

### III.—*Hypermanganic Acid and its Salts.*

If manganate of potash be treated with a solution of caustic potash, it dissolves without decomposition, and if the solution be evaporated in vacuo we obtain again crystals of manganate and hydrate of potash, which latter, under the air pump, may be obtained of great beauty. If, on the contrary, the manganate of potash be dissolved in water, it is decomposed; a brown crystalline precipitate falls down, which appears to be a combination of binocide of manganese with potash. This precipitate is decomposed when it is washed, the water dissolving the potash, and converting it into pure hydrated binocide. The solution has a deep red colour, and if it be evaporated till crystals appear on the surface, and the solution poured off, from any precipitate which may have appeared into a warmed dish, then beautiful deep red crystals are obtained when it cools. The same thing happens when a solution of manganate of potash is exposed to the air so that it can attract carbonic acid; as soon as the excess of alkali is saturated the solution becomes red, and at the same time a precipitate appears. Hence, we sometimes obtain a mixture of these red crystals during the preparation of manganate of potash, if the solution during evaporation be so situated that it is exposed to much carbonic acid.

(*To be continued.*)

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### ARTICLE IV.

*On some Methods of Astronomical Observation.* By WILLIAM GALBRAITH, A.M., Teacher of Mathematics, Edinburgh.

(*Continued from page 135.*)

III.—*On the method of finding the value of the divisions on the scales of levels applied to altitude and azimuth circles, registering observations, &c.*

ALL the more usual astronomical instruments have a level applied to them so as to insure the verticality of their axis, or to make the necessary allowance for their deviation from it. The scale of the level is so graduated as to show single seconds, or some multiple of the second, and reads most conveniently from a central zero. In those instruments that revolve in azimuth, which all the smaller, and more especially the portable circles do, (and even the large eight feet circle at Dublin, though provided with a plumb line rather inconveniently situated, and the most accurate, perhaps in principle, of any hitherto constructed,) the observations are repeated several times in pairs near the meridian, reading the divisions at both extremities of the air bubble on the scale of the level each time along with the verniers or microscopes. When there are three verniers and about six observations made, it is advantageous to have a simple and convenient method of registering the observations, taking the means, and allowing for the effects of the level.

The value of the divisions of the level is generally got from the maker, or it may be readily found by an instrument called the level trier, constructed expressly for this purpose.

If the observer has not had these communicated to him, or if he wishes to satisfy himself with regard to the accuracy of the values given to him along with the instrument, he may either ascertain these by the circle itself, when the verniers or reading microscopes are competent to the purpose, or he may have recourse to the following methods, which, in the course of my experience, I have found very convenient.

1. Put up the usual levelling rod of the best construction truly vertical, at such a distance from the circle as may be most convenient, though somewhat considerable.

2. Set the level exactly in the direction of two of the feet screws, or one perpendicular to the line joining the other two, when there are three; clamp the verniers, and direct the intersections of the cross wires of the telescope to the mark on the sliding vane, which must be moved up or down till an exact coincidence takes place.

3. By turning one of the feet screws cause the bubble to move through a given number of the divisions of the scale,

comprehending those usually employed in recording observations, while at the same time the sliding vane must be moved till its mark again coincides with the intersection of the cross wires in the telescope, still clamped to the circle, and the number of divisions on the rod which it has passed over to thousandths, or, at least hundredths of a foot, by this motion must then be recorded.

4. Measure the horizontal distance with great care between the centre of the circle and the levelling rod. These afford data for computing trigonometrically the value of the divisions of the scale of the level.

5. To investigate formulæ for this purpose, let  $R''$  be the length of an arc equal to the radius in seconds,  $D$  the horizontal distance,  $d$  the distance passed up or down by the vane,  $A''$  the arc in seconds subtended by  $d$ , at the distance  $D$  then by the principles of trigonometry,

$$A'' = \frac{R'' \times d}{D} \quad . . . . . (1)$$

If  $L$  be the length of a given number of seconds,  $a''$  on the scale of the level, and  $r$  the length of the whole run in the same measure as  $D$  and  $d$ ,

$$L'' = \frac{D \times a'' \times r}{R'' \times d} \quad . . . . . (2)$$

Indeed, if any four of the five quantities,  $D$ ,  $d$ ,  $r$ ,  $a''$ , and  $L$  be known, the value of the fifth may be found by transforming the preceding equation, thus :

$$a'' = \frac{R'' \times L \times d}{D \times r} \quad . . . . . (3)$$

If  $n$  be the number of divisions in the run of the level,

$$a'' = \frac{R'' \times d}{D \times n} \quad . . . . . (4)$$

If  $\rho$  be the radius of curvature of the level,

$$\rho = \frac{R'' \times L}{a''} = \frac{R'' \times r}{A''} \quad . . . . . (5)$$

Examples for the use of these formulæ.

1. The cross wires of the telescope of an astronomical instrument, at the distance of 250 feet from a levelling rod, moved over two inches in a run of the bubble through an inch and a half, by turning the feet screws in the direction

of the level and rod, what was the value of the whole arc  $A''$  passed over by the bubble, and the length  $L$ , of a division of  $a''$  (10") on the scale of the level?

$$\text{By formula (1) } A'' = \frac{R'' \times d}{D} = \frac{206264'' \cdot 8 \times 2}{3000} = 137'' \cdot 51$$

$$\text{By formula (2) } L = \frac{D \times a'' \times r}{R \times d} = \frac{3000 \times 10 \times 1 \cdot 5}{206264 \cdot 8 \times 2} = 0.109 \text{ in.}$$

From this last formula, a scale may be readily adapted to a level.

2. Let the length  $L$  of one of the divisions of the scale of a level be one-twentieth of an inch, the run of the bubble two inches, the distance  $d$  one inch and a fifth, and  $D$  five hundred feet, required  $a''$ , the value of one division of the scale in seconds?

$$\text{By formula (3) } a'' = \frac{R'' \times L \times d}{D \times r} = \frac{206264'' \cdot 8 \times 0 \cdot 05 \times 1 \cdot 2}{6000 \times 2} = 1'' \cdot 03$$

3. At the distance  $D = 90 \cdot 6$  feet, the vane of a levelling rod passed over  $0 \cdot 06$  of a foot in a run of 25 divisions of the level, what was the value  $a''$  of one division of the scale, and the radius of curvature of the level,  $L$  being one-tenth of an inch?

$$\text{By formula (4) } a'' = \frac{R'' \times d}{D \times n} = \frac{206264'' \cdot 8 \times 0 \cdot 06}{90 \cdot 6 \times 25} = 5'' \cdot 5$$

$$\text{By formula (5) } \rho = \frac{R'' \times L}{a''} = \frac{R'' \times \frac{1}{10} \times \frac{1}{12}}{5'' \cdot 5} = \frac{R''}{5 \cdot 5 \times 10 \times 12} = \frac{206264'' \cdot 8}{660} = 312 \cdot 5 \text{ feet.}$$

This result  $5'' \cdot 5$  is nearly the value of one division of a level attached to a six-inch travelling circle of Captain Kater's construction, made by Robinson. It is obvious, that the same method may be applied to determine the value of the divisions of a level belonging to larger instruments when required, and it is susceptible of very considerable accuracy when sufficient care is taken in performing the necessary operations.

II. After having determined the value of the divisions of the scale of a level, it is next proper to adopt a simple and ready method of applying its effects to observations.

Let  $e$  be the eye end of the telescope next the observer,  $o$  the object end,  $a''$  the value of one division of the level in

seconds,  $n$  the number of observations, and  $l$  their effect when applied to the zenith distance.

$$l = \frac{(e - o) a''}{2n} \dots \dots \dots (6)$$

The sign must be changed when applied to the altitude.

III. When three or more verniers are applied to a circle, and the observations are repeated and read each time, the mean result will be readily determined by the following formula in which  $\Sigma r$  is the sum of the readings of all the verniers or microscopes,  $n$  the number of observations,  $v$  the number of verniers, and  $m$  the mean value of the whole.

$$m = \frac{\Sigma r}{nv} \dots \dots \dots (7)$$

These formulæ will apply with ease and certainty to any case likely to occur in practice, and are more simple than any I have seen.

IV. The case to which they are now to be applied is one of a series of observations made by a small circle of Captain Kater's construction, to determine the obliquity of the ecliptic at the late summer solstice, at Edinburgh, in latitude  $55^{\circ} 57' 15'' \cdot 67$  N.

It may seem to be an attempt much beyond the powers of so small an instrument, one of six inches diameter, furnished with three verniers, each showing  $15''$  and a level, indicating by each division only to the accuracy of  $5'' \cdot 5$ . Yet, the correctness of the final result, which differs from Bessel's by about  $2'' \frac{1}{3}$ , and from mine, obtained by a comparison of the late observations made at Greenwich, with those of Bradley, reduced with the best tables by  $1'' \frac{1}{3}$ , shows how much may be accomplished with moderate means. With what pleasure would modern astronomers have contemplated the observations of Hipparchus and Ptolemy had they been made with such precision!

To determine the obliquity of the ecliptic in the most accurate manner, the sun's declination (daily if possible), near the solstices, must, it is well known, be observed carefully for some time, and the results, by means of appropriate formulæ or tables, are reduced correctly to the moment of the solstice computed from the best solar tables, or obtained from corresponding observations.

All the formulæ\* with which I am acquainted, and most of the tables are adapted to the sun's distance from the solstice reckoned on the ecliptic, or the difference between the sun's longitude, at the time of observation, and 90° or 270°. Now, by those possessing an ephemeris giving the sun's longitude at *apparent noon*, with differences to reduce to any given meridian, this is readily found. The sun's longitude, however, in the new Nautical Almanac for 1834, and succeeding years, is given to mean noon without differences or proportional parts, consequently, the distance of the sun, at apparent noon, from the solstice is not so easily obtained in terms of the longitude, as in those of the right ascension. Besides, in an observatory, the sidereal time is generally known by observation, and, therefore, on the whole, arguments depending on the right ascension are the more convenient for obtaining the reduction of the sun's observed declination to the solstice.

A very convenient formula for this purpose, may be obtained in terms of the right ascension as follows :

Let  $\kappa$  be the right ascension at the time of observation,  $\delta$  the declination,  $w$  the obliquity of the ecliptic, and  $x$  the connexion necessary to reduce the observed declination to the solstice.

By spherics,  $\sin. \kappa \tan. w = \tan. \delta = \tan. (w - x)$ .

But  $\tan. (w - x) = \frac{\tan. w - \tan. x}{1 + \tan. w \tan. x}$  therefore,

$$\sin. \kappa \tan. w = \frac{\tan. w - \tan. x}{1 + \tan. w \tan. x}$$

which by reduction becomes,

$$\tan. x = \frac{(1 - \sin. \kappa) \tan. w}{1 + \sin. \kappa \tan.^2 w} \dots \dots \dots (8)$$

This equation would give the reduction to the solstice, but it is not in a form to be readily applied. It admits of a transformation, however, from the following considerations, which renders it remarkably simple. Since  $\kappa$  does not in this case differ much from 6<sup>h</sup> or 18<sup>h</sup>, let  $\Delta = 6^h - \kappa$ ,  $\kappa - 6^h$ ,  $18^h - \kappa$ ,  $\kappa - 18^h$ , and  $\Delta$  being small  $\cos. \kappa = 1 - \frac{\Delta^2}{2} + \frac{\Delta^4}{24} - \frac{\Delta^6}{720} + \&c$ . If this value of  $\cos. \kappa$  be sub-

\* There are, I have since found, formulæ, though still requiring simplification, in some works on Astronomy for this purpose, and not free from obliquity.

stituted in formula (8) it becomes,

$$\tan. x = \frac{(1 - 1 + \frac{\Delta^2}{2} - \frac{\Delta^4}{24} + \frac{\Delta^6}{720} - \&c.) \tan. w}{1 (- + \frac{\Delta^2}{2} + \frac{\Delta^4}{24} - \frac{\Delta^6}{720} - \&c.) \tan.^2 w} \quad (9)$$

Now taking  $w = 23^\circ 27' 40''$ ,  $\tan. w = 0.4340056$ , and  $\tan.^2 w = 0.1883608$ . By introducing these values into equation (9) it becomes,

$$\tan. x = \frac{0.2170028 \Delta^2 - 0.0180836 \Delta^4 + 0.0006028 \Delta^6}{1.1883608 - 0.0941804 \Delta^2 + 0.0078483 \Delta^4 - 0.0002616 \Delta^6}$$

$$\tan. x = 0.18260684 \Delta^2 - 0.0007454 \Delta^4 - 0.00075777 \Delta^6 \&c. \quad (10)$$

in which  $\Delta$  is the length of the circular arc to radius unity.

It is now only necessary to adopt the co-efficients of formula (10) to degrees of arc or minutes of time, as these are the terms in which the right ascension of the sun is generally given, while  $\tan. x$  may in like manner be converted into seconds of arc. This is accomplished by applying the logarithms of  $R^\circ$ ,  $R''$ , &c. to the logarithms of the co-efficients of formula, (10) and they become those for  $\Delta$  expressed in degrees and decimals of a degree and  $x$  in seconds.

I.	II.	III.	
Const. logs.	1.0596970,	5.154114,	1.64523. . (A)

Similarly are obtained the logs. of the constants for minutes of time when the right ascension is given in time, and the distance from the solstice is known in minutes of time and decimals.

I.	II.	III.	
Const. logs.	9.8555770,	2.745874,	8.03287* . (B)

To render these co-efficients generally applicable, it is necessary to find the variation of  $x$  corresponding to a change of one second in  $w$ .

For this purpose from formula (9) we get

$$\tan. x = \frac{\frac{\Delta^2 \tan. w}{2}}{1 + \tan.^2 w} = \frac{5}{12} \Delta^2 \tan. w \text{ nearly, and thence,}$$

$$x = \frac{5}{12} \Delta^2 \sin. 1'' \tan. w \quad . \quad . \quad . \quad (11)$$

\*  $0''.7170955 \Delta^2 - 0''.000000557024 \Delta^4 - \&c.$

Differentiating equation (H) and

$$\delta x = \frac{5}{12} \Delta^2 \sin. 1'' \frac{\delta w}{\cos.^2 w} = \frac{5}{12} \Delta^2 \sin. 1'' \tan. \text{co.} \cos. w \frac{\delta w}{\cos.^2 w}$$

since  $\tan. + \cos. = R^2 = 1$ . But  $\frac{\cos.}{\sin.} = \cos.$  therefore

$$\delta x = \frac{5}{12} \Delta^2 \sin. 1'' \tan. w \times \frac{\cos. w}{\sin. w} \times \frac{w}{\cos.^2 w} = \frac{x \sin. 1'' \delta w}{\sin. w \cos. w}$$

and since the  $\sin. 2 w = 2 \sin. w \cos. w$ , we have

$$\delta x = \frac{2 \sin. 1'' x \delta w}{\sin. 2 w} = \frac{\sin. 2'' x \delta w}{\sin. 2 w} \dots (12)$$

Taking  $\delta w = 1''$ , substituting for  $\sin. 2 w$  its value when  $w = 23^\circ 27' 40''$ , formula (12) will become

$$\delta x = 0.0000132748x \dots (13)$$

Log. of 0.0000132748 is 5.1230279

By this means the correction for the variation of  $w$  from  $23^\circ 27' 40''$  may be readily obtained, by adding this constant logarithm and the log. of  $\delta w$  in the given case to the sum of the logs. under I, the sum will be the log. of the correction of  $x$ .

Example 1. Let  $w = 23^\circ 27' 43''.76$ ,  $\Delta = 60^m$   $\delta w = + 3''.76$ , required the reduction to the solstice.

	I.	II.	III.
Const. logs. . . .	9.8555770,	2.745874,	8.03287
$\Delta = 60^m$ log. $\Delta^2 =$	3.5563025,	$\Delta^4 = 7.112605,$	$\Delta^6 = 0.66891$
1 = + 43' 1''.54 log.	3.4118795	9.858479	8.70178
2 = — 0.72 C. L.	5.123	2d = — 0''.72	3d = — 0''.05
3 = — 0.05 log. $\delta w$	0.575		
4 = + 0.13	9.110		
$x = + 43$ 0.90	4th = + 0''.13		

$\Delta$	Cor. —
20	0.01
30	0.05
40	0.15
50	0.36
55	0.54
60	0.77
65	1.08
70	1.46
75	1.96
80	2.56

When  $\Delta$  does not exceed 30 or 40 minutes, which will in general be sufficiently distant from the solstice, the operation by the formula, even in natural numbers, becomes remarkably simple, because in that case, the second and third terms are insensible.

To render the first term applicable to every case, the sum of parts II and III may be taken from the small table in the margin, and is always to be subtracted.

Example 2. Let the sun's right ascension be  $7^h 16^m 36^s$ , the obliquity of the ecliptic  $23^\circ 27' 32'' \cdot 8$  and, consequently,  $\Delta = 1^h 16^m 36^s$ ,  $\delta w = 7'' \cdot 2$ , required the reduction to the solstice?

In this way, the computation assumes the following very simple form :

Const. logarithm . . . . .	9·855577
$\Delta = 1^h 16^m 36^s = 76^m \cdot 6$ , log. $\times 2$ . . . . .	3·768458
<hr style="width: 100%;"/>	
1st cor. = $+1^\circ 10' 7'' \cdot 6$ log. . . . .	3·624035
2d cor. = — 2·2 from this small table, $\delta w = -7'' \cdot 2$ log 0·857	
3d cor. = — 0·4 $\delta x$ from calculation. Const. log. . . . .	5·123
<hr style="width: 100%;"/>	
$x = + 1 10 5 \cdot 0 =$ red. to solstice. $\delta x$ log. . . . .	— 9·604

Hence, it appears that by this formula, the reduction to either solstice is a very easy operation. From these preliminary formulæ it is now proposed to show their general application to one day's observations, consisting of six sets or three pairs, made on the 5th of July last, at Edinburgh, in latitude  $55^\circ 57' 15'' \cdot 67$  N.

1834, July 5th. Chronometer fast for mean time . . . . .	2' 40"
Equation of time with a contrary sign . . . . .	4 16
<hr style="width: 100%;"/>	
Chronometer fast on apparent time . . . . .	6 56

Barometer,  $30^{\text{in}} \cdot 17$ , attached thermometer,  $70^\circ$  F., detached, or that in the open air,  $68^\circ$  F. Or, instead of making the  $6^m 56^s$  the error to each, it may be applied to  $12^h$  by subtraction, thus giving  $11^h 53^m 4^s$  for the time of apparent noon by chronometer, a method rather more convenient.

*Method of recording the Observations.*

Obs.	Time by Chronometer.			Level.	Limb.	Ver.	Observed Altitude.	Face of Circle.
	<i>h.</i>	<i>m.</i>	<i>s.</i>					
1	11	50	50	19	16	θ's <i>l. l.</i>	A 56° 32' 15"	E.
	Er.	6	56					
<hr/>								
	11	43	54				B 30 15	
							C 31 0	
2	11	53	14	13	21	θ's <i>u. l.</i>	A 57 1 30	W.
	E.	6	56					
<hr/>								
	11	46	18				B 3 45	
							C 0 45	
3	11	55	58	23	11	θ's <i>l. l.</i>	A 56 36 45	E.
	E.	6	56					
<hr/>								
	11	49	2				B 34 45	
							C 35 30	
4	11	58	41	18	16	θ's <i>u. l.</i>	A 57 5 15	W.
	E.	6	56					
<hr/>								
	11	51	45				B 3 0	
							C 4 45	
5	12	2	31	20	13	θ's <i>l. l.</i>	A 56 40 0	E.
	E.	6	56					
<hr/>								
	11	55	35				B 37 45	
							C 39 0	
6	12	5	39	10	23	θ's <i>u. l.</i>	A 57 6 45	W.
	E.	6	56					
<hr/>								
	11	58	43				B 8 45	
							C 6 0	

$$e = 103 \quad o = 100 \quad \text{Mean of Deg. } 56 \quad 30 \quad 0$$

$$o = \frac{100}{\quad} \quad \frac{\Sigma r}{n \bar{v}} = \frac{357' 45''}{6 \times 3} = + 19.52.5$$

$$e - o = 3 \& l = \frac{(e - o) a''}{2 n} = \frac{3 \times 5.5}{2 \times 6} = - 1.33$$

Correct mean of the whole, . . . . . 56 49 51.12

*Reduction to the Meridian.*

Times.	Dist. from Mer.			<i>m</i>	<i>n</i>
	<i>h.</i>	<i>m.</i>	<i>s.</i>		
1	11	43	54	508.77	0.62
2		46	18	368.46	0.30
3		49	2	236.10	0.14
4		51	45	133.63	0.05
5		53	35	38.30	0.01
6		58	43	3.23	0.00
				<hr/>	<hr/>
				1288.49	1.12
				<hr/>	<hr/>
Mean, . . . . .				214.49	0.1867

Reduction to the Meridian.

Refraction.

$\lambda = 55^\circ 57'$  cos. 9.748123

o. ' "

$\delta = 22^\circ 50'$  cos. 9.964560

$Z = 33 10$  log.  $\delta \theta$  1.5818

$A = 56^\circ 50'$  sec. 0.261952 tan. 0.1847  $B = 30.17$  log. 0.0025

$9.974635 \times 2 = 9.9493$   $\tau = 70^\circ$  log. 9.9991

$m = 214''.75$  log.  $2.331933n$  log.  $9.2711$   $t = 68^\circ$  log. 9.9840

$c = + 202''.57$  log.  $2.306568c'$  log.  $9.4051$   $r = 36''.93$  log. 1.5674

$c' = 0''.25$

$R.M = 202.32 = 3' 22''.32.$

Reduction to the Solstice.

$\Theta$ 's R. A. at app. noon =  $\overset{h.}{6} \overset{m.}{56} \overset{s.}{8.19}$  at Edinburgh,  $w = 23^\circ 27' 40''$   
Solstice, . . . . .  $\overset{h.}{6} \overset{m.}{.0000}$

$\Delta$ , or distance from solstice,  $0 56 8.19 = 56^m.1365.$

Now by formula (B) page 10.

Constant logarithm, . . . . . 9.855576

$\Delta = 56^m.1365$  log.  $\times 2$ , . . . . . 3.498490

$1 = 37' 39''.78$  log., . . . . . 3.354066

$2 = 0.61$  by small table.

$R. S. = + 37 39.17 =$  reduction to the solstice.

Apparent altitude of the sun's centre, . . . . .  $56 49 51.12$

Refraction, . . . . . — 36.93

Parallax, . . . . . + 4.67

Reduction to the meridian, . . . . . + 3 22.32

„ to solstice, . . . . . + 37 39.17

Latitude of the place of observation, . . . . .  $55 57 15.67$

Sun's latitude south, . . . . . + 0.10

Solar equation and reduction to January 1st, . . . . . + 0.70

Lunar equations  $\left\{ \begin{array}{l} \Omega \text{ . . . . . } - \text{ 0.73} \\ v \text{ . . . . . } + \text{ 0.10} \end{array} \right.$

Mean obliquity, Jan. 1st, 1834 = Sum\* —  $90^\circ = 23 27 36.19$

Bessel gives, . . . . .  $23 27 39.26$

Error of one day's observations, . . . . . — 3.07

By a mean of ten days observations reduced in this manner, the obliquity was  $23^\circ 27' 41''.64$ , and the difference

\* When the declination is of a contrary name to that of the latitude, the sum is the polar distance of an opposite name, and must be subtracted from  $90^\circ$ .

of this from Bessel's is  $+ 2''\frac{1}{3}$ , and from the author's, derived from the Greenwich observations,  $1''\frac{1}{3}$ , a very small error, considering the nature of the problem, and size of the instrument.

(To be continued.)

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ARTICLE V.

Catalogue of Plants collected at Bombay.

BY JOHN GRAHAM, ESQ.

(Continued from page 115.)

172. *Datura fastuosa*. Common.
173. *Dracæna ferrea*. In flower pots only.
174. *Dimocarpus litchi*. In gardens, though not common. It bears fruit here but not equal to that obtained from China.\*
175. *Dalbergia arborea*, native name Carunj. A very pretty tree; leaves deciduous in the cold weather.
176. „ *Sissoo*. Black wood used extensively in making furniture.
177. „ *scandens*.
178. *Dolichos tuberosus*.
179. „ *cultratus*. A species of *Dolichos* is much cultivated and eaten like French beans, the *tetragonolobus*.
180. „ *pruriens*.
181. *Dioscorea sativa*. Common yam.
182. „ *bulbifera*.
183. *Diospyros Ebenum*.
184. *Daemia reticulata*.
185. *Dillenia speciosa*.
186. *Diospyros montana*.
187. „ *hirsuta*
188. *Daphne Bholua*.
189. *Dendrobium*,? On the Ghauts.
190. *Dombeya palmata*. In gardens only.
191. *Exacum bicolor*.
192. *Evolvulus hirsutus*.†

\* The *Litchi* forms the favourite fruit in Chinese deserts. It resembles somewhat the fruit of the Maple (*Acer Campestre*) in external appearance. The tree grows in a wild state in French and Danes' Islands, Whampoa.—EDRR.

† Is this not a variety of *E. alsinoides*, a common plant in China?—EDRR.

193. *Euphorbia Tirucalli*. Common milk bush.  
 194. „ *antiquorum*.  
 195. „ *tithymaloides*. Used for edgings instead  
 of box.  
 196. „ *neriifolia*.  
 197. „ *hirta*. A common weed.  
 198. *Eugenia jambos*. Jambler, rose apple.  
 199. „ *Malaccensis*.  
 200. *Erythrina Indica*. A deciduous tree. It flowers in  
 March and makes a very showy appearance.  
 201. *Eupatorium Zelonis*.  
 202. *Eclipta prostrata*.  
 203. *Elephantopus scaber*.  
 204. *Feronia elephantum*. Wood apple, a large hand-  
 some tree.  
 205. *Ficus Carica*. In gardens only.  
 206. „ *religiosa*. Pepul tree.  
 207. „ *Indica*. Banyan tree.  
 208. „ *elastica*.  
 209. „ *racemosa*.  
 210. „ *pubescens*.  
 211. *Flacourtia sapida*. In gardens only.  
 212. „ *sepiaria*. Elephanta.  
 213. „ *inermis*.  
 214. *Guazuma ulmifolia*.  
 215. *Gardenia radicans*. In gardens only, cultivated for  
 its beautiful, white, sweet smelling flowers.  
 216. *Gardenia lucida*. Elephanta.  
 217. „ *dumetorum*.  
 218. „ *esculenta*.  
 219. *Getonia floribunda*.  
 220. *Grewia orientalis*.  
 221. *Gomphrena globosa*. In gardens only, cultivated  
 for its flowers.\*  
 222. *Gloriosa superba*. Common during the rains.  
 223. *Guilandina bonduccella*.  
 224. *Gärtnera racemosa*.  
 225. *Garcinia Cowa*. Common in the Concan.  
 226. *Grewia Asiatica*.  
 227. *Gerardia delphinifolia*.

\* Indigenous to China.—EDD.

228. *Gmelina arborea*.  
 229. „ *Asiatica*.  
 230. *Gossypium herbaceum*.  
 231. *Glycine Sinensis*.  
 232. *Galega purpurea*.  
 233. *Garuga pinnata*.  
 234. *Grislea tomentosa*.  
 235. *Hoya carnosa*. Cultivated as an ornamental plant.  
 236. „ *viridiflora*.  
 237. *Hyperanthera Moringa*. Very common.  
 238. *Helicteres ixora*.  
 239. *Hibiscus populneus*. Bhendy tree.  
 240. „ *rosa Chinensis*. Cultivated as an ornamental plant.  
 241. „ *mutabilis*. Ditto ditto.  
 242. „ *Sabdariffa*. Iropille, used in making jellies, tarts, &c.  
 243. „ *esculentus*. Commonly cultivated.  
 244. „ *surratensis*.  
 245. „ *cannabinus*.  
 246. „ *tricuspis*.  
 247. *Hedysarum gyrans*.  
 248. „ *strobiliferum*.  
 249. „ *tuberosum*.  
 250. „ *vespertilionis*.  
 251. *Hemidesmus Indicus*.  
 252. *Ixora coccinea*.  
 253. „ *parviflora*.  
 254. *Ipomœa Quamloquit*. Cupid's flower.\*  
 255. „ *fragrantissima*.  
 256. „ *tuberosa*.  
 257. *Impatiens Balsamina*.†  
 258. *Inula Indica*.  
 259. *Jasminum Sambac*. Mogrel, native name. extensively cultivated for its flowers.  
 260. „ *odoratissimum*.  
 261. „ *latifolium*.  
 262. „ *undulatum*.  
 263. „ *auriculatum*.

\* This plant is indigenous to Danes' Island, China.—EDIT.

† This species occurs in China.—EDIT.

264. *Justicia picta*. Common in flower pots.  
 265. *Justicia nervosa*.  
 266. „ *bivalvis*.  
 267. „ *montana*.  
 268. *Jonesia pinnata*. On Salsette.  
 269. *Jatropha curcas*. Used for forming hedges.  
 270. „ *manihot*. In gardens only, very rare.  
 271. „ *multifida*. In gardens, as an ornamental plant.  
 272. *Kydia fraterna*.  
 273. *Kyllingia umbellata*. Grass.  
 274. *Loranthus*. Several species.  
 275. *Lawsonia inermis*. Used for forming hedges.  
 276. *Laurus cinnamomum*. In gardens only.  
 277. „ *Persea*. In gardens only.  
 278. *Limonia monophyllum*.  
 279. „ *trifoliata*.  
 280. *Lagerstroemia regina*. In the Concan.  
 281. „ *Indica*.  
 282. „ *parviflora*.  
 283. *Lantana purpurea*.  
 284. *Lepidagathus cristata*.  
 285. *Menyanthes cristata*.  
 286. „ *Indica*.  
 287. *Mussaenda frondosa*. On the Ghauts.\*  
 288. *Morinda Indica*.  
 289. „ *citrifolia*.†  
 290. *Mirabilis Jalapa*. In gardens.  
 291. *Mangifera Indica*.  
 292. *Mimusops elengi*.  
 293. „ *hexandra*.  
 294. *Murraya exotica*. In gardens only.  
 295. *Melia azidiraltea*. Neem tree.  
 296. *Myrtus communis*. In gardens.  
 297. *Maumea America*. In gardens, rare.  
 298. *Michilea champaca*.  
 299. *Momordica charabee*. Commonly cultivated as an article of food.

\* Abundant on French Island, Whampoa, China.—EDIT.

† A native of China.—EDIT.

- 300. *Menispermum cordifolium*.
- 301. *Musa paradisaica*. Plaintain.
- 302. *Musa*,? On Ghauts.
- 303. *Mimosa pudica*.
- 304. ,, *cinerea*.
- 305. ,, *Arabica*. Babool tree; common; in extensive use as firewood.
- 306. ,, *scandens*. On the Ghauts.
- 307. ,, *Sirissa*.
- 308. ,, *glauca*.
- 309. ,, *dulcis*.

(To be continued.)

ARTICLE VI.

*On the Differences of Temperature between the Granite and Slate in the Cornish Mines.* By W. J. HENWOOD, F.G.S. London and Paris, Hon. M.Y.P.S., Assay Master of Tin in H. M. Duchy of Cornwall.

*To the Editor of the Records of General Science.*

SIR,—Having, in an examination of the mines of this county, made many observations, which, I think, clearly show a difference in the temperatures of the granite and slate, permit me a page of your valuable Journal for a brief abstract of them.

GRANITE.

Depth.		Number of Observations.	Temperatures.
Surface to 50 fathoms; average	31 fathoms,	7	51°·6
50 ,, 100 ,,	79 ,,	15	59°·0
100 ,, 150 ,,	133 ,,	11	65°·4
150 ,, 200 ,,	,,	·	·
200 and beyond	237 ,,	3	81°·3

SLATE.

Depth.		Number of Observations.	Temperatures.
Surface to 50 fathoms; average	35 fathoms,	21	57° 0
50 ,, 100 ,,	73 ,,	19	61°·3
100 ,, 150 ,,	127 ,,	29	68°·0
150 ,, 200 ,,	170 ,,	21	78°·0
200 and beyond	221 ,,	5	85°·6

These observations have been, in all cases, made on jetting and running streams of *water*, immediately on their issuing from the *unbroken* rock; a mode, which, I think, more

likely to approach the true temperature of the earth at, and near the place of observation, than if inserted in holes bored in the sides of the galleries, or in rubbish long unmoved, as both these are unquestionably much affected by the air circulating in the vicinity, which, also, being itself exposed to so many disturbing causes, cannot be regarded as giving more than a very distant approximation to the real temperature of a given spot.

Confirmatory of the prevailing opinion of an increase of temperature in descending—the following observations refer to the *water* as running or jetting out of the *unbroken rock* at the “adit level,” and of that pumped to the same gallery from the deepest part of the mines.

GRANITE.

Depth.	Number of Observations.	Temperatures.
From Rock in adit 22·5 fathoms, . . . . .	6	50°·4
Pumped to adit from 110·0 fathoms, . . . . .	12	57°·2

SLATE.

Depth.	Number of Observations.	Temperatures.
From Rock in adit 27·0 fathoms, . . . . .	4	53°·4
Pumped to adit from 113·0 fathoms, . . . . .	17	60°·0

These observations are also confirmatory of the difference between the temperatures of the granite and slate.

I have the honour to remain,

Sir,

Your very faithful, humble Servant,

W. J. HENWOOD.

1, Merrab Place, Penzance,

4th August, 1836.

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ARTICLE VII.

*The Art of Dyeing.*

(Continued from page 64.)

BLUEISH GRAY, FROM COCHINEAL AND IRON MORDANT.

The production of a darker shade is combined with many difficulties, as iron mordants which contain the iron in the state of peroxide form with the colouring matter of the

cochineal, not a grayish blue but a grayish brown. It is necessary, therefore, to employ a protoxide salt as a mordant. In this instance, the usual difficulty occurs, to form with these mordants uniform grounds. When the calico is impregnated with a mordant consisting of 10 lbs. sulphate of iron, 40 lbs. vinegar, 40 lbs. water, and 10 lbs. sugar of lead; allowed to dry in heated air; washed with water equally after the drying, and dyed with cochineal, a very clear grayish blue is obtained, but highly unequal and full of spots. So soon as the calico becomes dry, before the dyeing, a quantity of clear matter appears grouped together, produced by the action of the oxygen of the air, in consequence of which, the mordant is spread irregularly over the calico. This may also be observed after the dyeing is completed.

It is best to produce this colour with the pyrolignite of iron. The matter which it contains prevents its rapid change in the air, so that it remains equally absorbed by the calico. Pyrolignite of iron is also thicker; hence, this will be an obstacle to its flowing. It is important that the calico should be rapidly dried and dyed equally afterwards, without allowing it to hang long. The same occurs in printing as the thickening prevents the action of the air. The calico, in this case, is purified by rinsing in running water. A hot cow-dung bath acts, in this instance, very injuriously.

To form a dark blueish gray, 7 lbs. of mordanted cloth are used for 1 lb. of cochineal. The cochineal should be sufficiently pulverized by passing it through a coffee mill.

If the cochineal powder is introduced into a lukewarm solution, and the cloth placed in it, many dark spots are formed. The cochineal should, therefore, be boiled strongly with a little water, and this decoction added to the vat. The water should be very pure; especially it should be free from lime. This colour does not improve either by soap or other means.

*Properties of cochineal blue-gray.* This colour withstands the action of light and air longer than the cochineal red. Therefore, in this respect, it cannot be termed a fleeting colour. But in soap-suds it is completely changed. When a piece of calico, dyed of a dark shade, is boiled for a

quarter of an hour in soap suds, consisting of 1lb. soap in 300 lbs. water, the colour is changed into a gray-brown, resembling quercitron-brown.

*Solution of potash* makes a faint whitish gray spot which vinegar nearly dissolves.

*Ammonia* dissolves the dye and makes it violet.

*Lime-water* and *vinegar* have no action.

*Lime-juice* produces a yellowish spot with reddish borders, which becomes grayish-white by the action of ammonia.

*Tin mordants*, No. 1 and 2, printed upon it produce a very brilliant crimson, which is very clearly distinguished upon the blue-gray ground.

*Chloride of lime* printed upon it gives a dark nankeen colour.

#### COPPER MORDANTS.

Sulphate of copper does not combine with the calico fibre, and is useless as a mordant.

Acetate of copper, formed from sulphate of copper and sugar of lead, is not much better, as it only enters into combination with the fibre in very small proportion.

On the contrary, however, ammoniuret of copper combines readily with the calico. To produce this combination 8 lbs. of sulphate of copper are dissolved in 26 lbs. of solution of ammonia of specific gravity 0.897, the vitriol being added gradually, and stirred, until it is all dissolved. The azure-blue solution is then to be well covered and allowed to clarify.

This mordant is termed **COPPER MORDANT, No. 1.**

It answers when thickened with gum for printing a light copper-blue and the metal green.

When the calico is impregnated with this mordant and washed, after remaining some days in the air, it is found to be so strongly mordanted as to give very dark colours; unfortunately, however, they are not regularly produced, and have mostly an earthy, dull appearance.

The object is, therefore, better obtained by employing the mordant diluted with water, and without allowing the calico (impregnated with it and passed between two rollers) to dry, to wash it immediately. In this case, the copper mordant combines equably with the calico.

It does not signify much in what proportion the copper mordant is diluted with water, for it is remarkable, that calico is not more strongly mordanted by solutions of 1 to 4, than of 1 to 16.

It is, therefore, more profitable to dilute it to the last state; or to add to 10 lbs copper mordant No. 1, 160 lbs. of water. This is termed **COPPER MORDANT No. 2.**

A cheaper copper mordant to be used on colourless grounds is obtained by the following formula:—(1.) Let 3 lbs. of lime be dissolved of a thin liquid consistence, and while yet warm, be well mixed and stirred with a solution of 12 lbs. sulphate of copper in 200 lbs. water. (2.) Let 3 lbs. of lime, in the same state, be mixed with a solution of 5 lbs. sal-ammoniac in 600 lbs. water, well stirred and covered. After twelve hours, both mixtures should be poured together with the sediment collected. The blue solution now formed is ammoniuret of copper or copper mordant No. 2.

#### BLUE FROM COPPER MORDANT No. 2, AND LOGWOOD.

When a darker colour cannot be obtained by employing a stronger mordant, it is necessary in order to obtain darker shades, to mordant twice and dye twice. The light blue is first dyed; this is again impregnated with the copper mordant No. 2, and re-dyed with logwood. The dyeing solution contains 1 lb. logwood, 3 lbs. bran, for 10 lbs. of mordanted cloth. The dyeing is performed by a steady heat and long boiling. At first, the solution is very blue, but is gradually removed. Boiling soap-suds renders the colour of the blue vat similar.

*Properties of Logwood Blue.*—Boiling this colour with soap-suds for a quarter of an hour is rather advantageous than prejudicial.

*Solution of potash* produces white spots, which vinegar completely removes.

*Lime-water, ammonia, and vinegar* have no action upon it.

*Lime-juice* makes orange-yellow spots, which ammonia so far dissolves as to make the blue only a little lighter than formerly.

*Both tin mordants*, when printed on it, discharge a purple red.

*Solution of chloride of lime*, in the proportion of 1 to 40 water, discharge a pure white.

## MIXED MORDANTS.

Alum and iron mordants can be mixed in all proportions, and so produce numerous gradations in colour. In order to form such colours properly, the following mordants may be used:—

## ALUM MORDANT CONTAINING IRON.

Let 20 lbs. alum and 10 lbs. iron alum be dissolved in 160 lbs. of boiling water, and when the solution has somewhat cooled add 30 lbs. sugar of lead. Stir frequently and allow it to clarify. This mordant remains permanent for months.

## IRON MORDANT CONTAINING ALUM.

Let 20 lbs. of iron alum, and 10 lbs. alum be dissolved in 160 lbs. of boiling water, and when the solution has somewhat cooled, add 30 lbs. sugar of lead. Stir it and allow it to clarify. This mordant soon changes by the precipitation of oxide of iron; only a small stock of it, therefore, should be kept: or in place of one half of the water substitute vinegar. The mordanting of dark grounds is performed as with alum and iron mordants.

The drying must be quickly performed, as the iron mordant readily separates from the alum mordant and runs into the borders. Hence, these become mostly darker in dyeing. This property can be exhibited easily, on a small scale, by allowing a drop of the alum mordant containing iron to fall upon a piece of calico, to dry slowly, and then to dye it with tannin. The border appears almost black, while the spot itself is dyed grayish yellow.

The light colours or light grounds are formed with the same mordant in the same manner: only that they are not dried, but, after similar drying and pressing between the rollers, they are immediately washed. The same mordants may also be employed successively, instead of in the form of mixture, to produce the different shades.

After the cloth impregnated with the mordant, for producing dark brown red, has been allowed to hang several days, it should be passed through a hot cow-dung bath.

The cloth prepared for light brown-red will be equally dyed after rinsing, without the use of cow-dung.

To form both dark shades—let 10 lbs. of mordanted cloth be used for 10 lbs. Avignon madder. Hot soap-suds gives the colour a more saturated aspect.

*Properties of madder-red brown.*—Boiling for 10 minutes with soap-suds (1 part soap to 500 water) gives the dark shades more lustre, and a tendency to purple-red without the depth.

*Solution of potash* produces a scarcely appreciable darker colour, which completely disappears by the action of vinegar.

*Lime-water, ammonia, and vinegar* produce no action.

*Lime-juice* produces yellow-brown spots, which ammonia completely dissolves.

*Tin mordant, No. 1*, printed upon it, discharges a splendid yellow-red.

*Tin mordant, No. 2*, discharges a dull brown-red.

*Solution of chloride of lime* produces pink coloured spots.

#### YELLOW-BROWN FROM QUERCITRON AND IRON MORDANT CONTAINING ALUMINA.

This is a lighter colour than that formed by the other mixed mordant.

After the cloth impregnated with the mordant has been allowed to hang several days, it should be passed through a hot cow-dung bath. The cloth prepared for light yellow-brown should not be treated with cow-dung, but equally dyed after rinsing.

To form both dark shades, 10 lbs. of mordanted cloth should be used for 4 lbs. of powder of quercitron. The dyeing should be performed by slowly bringing the solution to the boiling point. Lime should not be added to the solution.

*Properties of quercitron yellow-brown.*—Boiling for 10 minutes with soap-suds removes the yellowish colour, and converts the dye into a saturated brown.

*Solution of potash* forms dark brown spots, which vinegar does not completely destroy.

*Lime-water* renders it brownish, which vinegar removes.

*Vinegar* gives the colour more of a greenish shade.

*Lime-juice* discharges it white. Ammonia restores the colour.

*Tin mordant*, No. 1, printed upon it, forms a clear dark yellow.

*Tin mordant*, No. 2, forms a clear light yellow.

*Solution of chloride of lime* produces a slight brownness.

With these mordants an infinite number of shades may be produced, partly by altering the proportions of iron to the alumina, or vice versâ, and partly by employing other colouring matters, besides madder and quercitron. Galls, tannin, oak and willow bark, especially, give clear colours.

GREEN-YELLOW FROM ALUMINA, IRON MORDANT, AND QUERCITRON.

A light shade is produced by immersing the cloth in alum mordant No. 3; pressing it; and then, before drying, rinsing it; and then treating it in the same manner with a solution of iron alum.

A dark colour is communicated to the calico mordanted with alum mordant No. 1. After being allowed to hang several days and well washed, it should then be impregnated with iron mordant No. 2; pressed and then properly washed before drying.

A light colour is produced by 4 lbs. quercitron powder to 24 lbs. mordanted cloth. A deep yellow tinge by 8 lbs. quercitron powder and 24 lbs. mordanted cloth.

No lime should be added to the solution, and the dyeing should be performed at a boiling temperature.

Hot soap-suds do not improve this colour. On the contrary, when passed through a mixture of 20 lbs. strong vinegar and 200 lbs. water, it acquires an agreeable green shade.

*Properties of Quercitron Yellow-Green.*—By boiling for 10 minutes with soap-suds the greenish-yellow colour alters, and is converted into a gray of the same depth.

*Solution of potash* produces brown spots which vinegar removes.

*Lime-water* shews some action, when a drop is placed upon the cloth which acquires a brown-colour.

*Ammonia* makes the colour brownish.

*Vinegar* makes green spots.

*Lime-juice* forms yellowish white spots which are not completely removed by ammonia.

Both tin mordants printed upon it discharge a pure yellow. Solution of chloride of lime forms brown spots.

*Remark.* A similar yellow-green, but more permanent which gives a clearer yellow when discharged by tin mordants, is obtained by means of the same mordant, by dyeing in a solution of Persian berries.

Light green-yellow is produced by 2 lbs. of berries to 24 lbs. mordanted cloth, and dark green-yellow by 4 lbs. berries and 24 lbs. mordanted cloth.

#### BROWN-RED FROM ALUMINA AND IRON MORDANT WITH MADDER.

A light shade is produced by impregnating first with a solution of alum, (10 lbs. alum in 600 lbs. water,) pressing and then rinsing equably. It should then be treated in the same way with a solution of iron alum (10 lbs. iron alum in 600 lbs. water). A dark colour is formed by first dyeing the calico impregnated with alum mordant, No. 1, red, impregnating it with the solution of iron alum (10 lbs. iron alum in 300 lbs. water) pressing it and washing it uniformly. The light colour requires the following: 24 lbs. mordanted cloth, 4 lbs. Avignon madder, 12 lbs. bran; the dark colour, 24 lbs. mordanted cloth, 20 lbs. Avignon madder, and 60 lbs. bran.

The madder and bran are first boiled with a small quantity of water; then more is added; the dyeing performed gradually, and lastly, terminated at a boiling temperature.

*Properties of Madder brown-red.*—Boiling with soap-suds for 4 hours produces only a slight alteration on the dye. Light and air have but an inconsiderable action, and lime-water is in a similar predicament.

Ammonia and vinegar have no action.

Lime-juice forms light yellow spots which ammonia completely dissolves.

Tin mordants, Nos. 1 and 2, discharge yellow-red.

Solution of chloride of lime makes whitish spots without bleaching completely.

#### EXHIBITION OF THE ALUM IN COMBINATION WITH COPPER MORDANT PARTLY BEFORE, PARTLY AFTER THE DYEING.

When alum and sulphate of copper are dissolved together and precipitated by sugar of lead, a useful mordant is not obtained. The acetate of copper spreads unequally over

the calico, while it crystallizes and is deposited in greater quantities on some places than on others. The greater proportion is also carried away by washing. It is, therefore, necessary to proceed in a different mode. The calico impregnated with acetate of alumina should be treated with copper mordant. Here a remarkable property is exhibited.

The calico saturated with the acetate of alumina after hanging for 6 or 8 days, and being well washed in running water, when brought into the copper mordant, No. 2, and allowed to remain there for a quarter of an hour, takes up such a quantity of oxide of copper that the colour becomes as dark as Prussian blue.

A piece of unmordanted calico of equal size when treated in the same manner, takes up only so much of the copper mordant as to acquire a blueish shade.

A considerable affinity thus appears to exist between the alumina and copper, which increases in proportion to the smallness in quantity of ammonia which the copper mordant contains. In the last case, so much oxide of copper is collected upon the calico that it lies upon it as a powder, by which the lustre is deteriorated.

The proper proportion of the ammonia to the copper is 10 lbs. solution of ammonia to 200 lbs. copper mordant, No. 2. The calico impregnated with the acetate of alumina should be treated with this mixture in the manner described.

Only a limited number of colouring matters answer for this kind of mordanting, while such as contain much tan, as nut-galls, tannin, &c., give powdery colours. Other dyes, as Persian berries, separate oxide of copper from the calico and form a yellowish-brown colour in the solution. Notwithstanding this, however, when 5 lbs. of cloth are employed for 1 lb. of berries, a saturated yellow-brown is formed.

The action of the madder solution is most remarkable upon cloth mordanted in this way. If we take two loths (1 oz.) of calico impregnated with the acetate of alumina mordant, divide it into two equal parts, leave the one portion as it is, and treat the other portion with the copper mordant as already described, and dye both in a solution of madder; the 2 loths thus take up so much as to become dark. The calico impregnated with the acetate of alumina mordant alone becomes a bright orange, while that im-

pregnated with the copper mordant becomes a little darker and brown-red. The solution of madder, on the other hand, contains much gummy matter, and the madder itself has a dark brown colour. It is obvious, therefore, that the solution of madder dissolves oxide of copper and thereby precipitates the greater portion of the colouring matter. Even treating the cloth before dyeing with cow-dung, produces no alteration. If both pieces of cloth are dyed at once with the same quantity of madder, no madder red is obtained on the one but a somewhat darker orange, and the other mordanted with copper mordant will be as dark again as it was before. For this reason; that on the first dyeing the madder is prevented by the oxide of copper from being dyed darker.

Hence, it is a rule with manufacturers not to introduce copper into alum mordants, intended for madder red. Even a small quantity of copper taken up by boiling the solution of alum in an untinned copper vessel, acts injuriously. For the same reason we cannot extract in a copper vessel the madder red from madder with muriate of alumina. It precipitates with the oxide of copper.

*(To be continued.)*

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#### ARTICLE VIII.

##### *A Theory of Accidental and Complementary Colours.*

By CHARLES TOMLINSON, Esq.\*

1. If we close one eye, and place before the other a disk of coloured glass, green, for instance, and through this medium view a sheet of white paper, the sudden removal of the disk from the eye will cause the paper to assume a bright red appearance; † and if the disk be instantly re-

\* This paper would have appeared long since in continuation of my paper, vol. ii. page 283, had it not been for a revised publication, by M. Plateau, of the First Part of his theory, wherein he repudiates his former publication as incomplete. I have waited many months, and the Second Part of his publication has not appeared. I, therefore, venture to publish my theory without reference to M. Plateau. My papers on this subject, already published in the Records, will be found vol. i. page 439, vol. ii. pages 21 and 283. Mr. Cooper's papers appear in vol. ii. pages 108 and 172, and vol. iii. page 99.

† The change from green to red may be strikingly observed, by substituting a taper in a room where there are no other lights for the sheet of white paper. The former is best seen by night, and the latter by day. In both cases, a pair of dark green spectacles is, perhaps, preferable to a green disk.

placed before the eye, the latter will be unable to distinguish any thing for a second or two. The order, therefore, will be as follows:—green—red—green—black. This can be repeated any number of times, and with any coloured disk, provided the colour be sufficiently deep.

2. The rationale of this experiment, I assume to be as follows:—White light is composed of red, yellow and blue; green is composed of the two latter, which the green disk transmits easily and readily, while it opposes the transmission of the red rays: the surface of the glass, therefore, next the eye, transmits green light, and the opposite side is receiving and reflecting red; if the glass be suddenly removed, the red rays, whose path was obstructed by the disc enter the eye; and, during this process, if the green disk be restored, we view the three colours red, yellow and blue by super-position; hence, the result is black.

3. In order to prove that the red rays are reflected, we have only to reverse the experiment. Take a disk of green glass, sufficiently transparent to allow reflection from its under surface,—let the observer's back be opposite to the window, so as to get the window frame reflected; the bars will appear of a strong and decided green; this reflection is obtained from the under surface of the glass—a faint image of the bars, &c., will be obtained from the first surface, which will be red. When the two surfaces are not quite parallel, this effect is most apparent, as the two images are separated to distances varying from each other, thus indicating, very clearly, the surface from which either colour is produced; accordingly, the Perichromascope, (*Records*, vol. ii. p. 24,) which depends wholly on this principle, is more satisfactory, when the coloured glass and the plane mirror form an angle with each other. The reason why the complementary colours are so seldom seen in a piece of common stained glass is, that its small thickness prevents a sufficient separation of the images from the two surfaces.

4. The rationale of the experiments (1, 3,) derive additional support from the following experiment. When the sun is shining on the window, place a prism so as to throw a well defined spectrum upon the ceiling of the room. Arrange the Perichromascope so that the coloured

disk may form an acute angle with the plane mirror—this arrangement will separate the two images of the spectrum, which is now to be observed. Suppose a green disk be employed: the image of the spectrum, obtained from the second surface of the green disk, will be entirely deficient in red rays; it will be, in fact, a spectrum deprived of red and orange; while the image obtained from the reflection of the first surface will be entire.

5. The first surface of coloured glass, *considered by itself*, is simply a reflecting surface; and objects presented to it are returned unaltered in colour. This occurs when the reflection from the first surface is obtained in such a manner, that the second surface does not act; but, in every case, when white light falls upon the first surface, a portion enters the glass and is absorbed, while another portion is reflected unaltered from the first surface: but suppose the coloured glass be arranged in a manner favourable to the attainment of reflections from the two surfaces, and this is eminently the case in the Perichromascope, and an object illuminated by white light is presented to the coloured glass; of the white light so impinging on the first surface, a principal portion is at once chromatically divided into two complementary portions; one portion, which is homochromatic with the glass, enters readily and reflects the image of the same colour from the second surface; another portion, complementary in colour, does not enter the glass, but is reflected by the first surface, mingled, however, with a small portion of *undecomposed* light which did not enter the glass: this gives the image from the first surface the faint grayish appearance, which has been already noticed by Mr. Cooper.—*Records*, vol. ii. p. 176.

6. It is natural, then, to expect that if white light be chromatically divided on entering the glass, that the colour of the reflection from the second surface, combined with the colour reflected from the first surface, will, in every case, afford *all* the colorific rays necessary to constitute white light: and such, indeed, is the case; the primary colour and its complement always affording red, yellow, and blue. These I assume to be the fundamental colours. Were I to adopt Mr. Cooper's theory, which supposes them to be red, green, and violet, I confess I should be unable

to account for most of the phenomena of accidental and complementary colours; especially, as I cannot admit with him that the accidental colour of red is blue; of violet yellow, &c. (*Records*, vol. ii. p. 110, et seq.) The following Table will show that with seven primary colours and their complements, red, yellow, and blue, are in each case afforded.

Table of the Seven Primary Colours with their Complements and Composition.

Composition.	Primary.	Complementary.	Composition.
Simple	Red 1	Green	2 Yellow & 3 Blue
1 Red & 2 Yellow	Orange	3 Blue	Simple
Simple	2 Yellow	Indigo or Purple	1 Red & 3 Blue
2 Yellow & 3 Blue	Green	1 Red	Simple
Simple	3 Blue	Orange	2 Yellow & 1 Red
1 Red & 3 Blue	Indigo or Purple	Orange 2 Yellow	Yellow & Red
Blue & Red	Violet	Yellow Green	2 Yellow, 1 Red, & 3 Blue

I shall now proceed to notice other cases of accidental colours and to offer an explanation, reserving for the close of the next paper a general and brief analysis of my theory.

7. "If we place a bright red wafer upon a sheet of white paper and fix the eye steadily upon a mark in the centre of it, then if we turn the eye upon the white paper we shall see a circular spot of blueish-green light, of the same size as the wafer."—(*Brewster's Optics*, p. 304.) This effect is explained by supposing the sensibility of the eye to red light to be diminished, "and, consequently, that when the eye is turned from the red wafer to the white paper, the deadened portion of the retina will be insensible to the red rays which form part of the white light from the paper, and, consequently, will see the paper of that colour which arises from all the rays in the white light of the paper but the red; that is, of a blueish-green colour, which is, therefore, the true complementary colour of the red wafer."—*Brewster's Optics*, p. 305.

I have, in a previous paper, objected to those experiments which fatigue the eye, because, under such circumstances, the result is an unnatural one. Accidental colours can be abundantly seen together with the primary colour, naturally and easily; and upon such data a theory ought, I conceive, to be founded. I object to the explanation which imputes

to the fatigue of the eye the presence of the accidental colour, because if this principle were true and carried to its full extent, our eyes would be unable to endure the light of day, the vivid colours of nature, or even the mild light of the moon for many minutes together; we should be unable to judge of colour, or to contemplate coloured objects: our sense of sight would, like the hungry appetite soon be satisfied; farther than this would sicken and distress; and the eye, like the stomach, having taken food, would require long intervals of repose for digestion; and thus the most beautiful and useful of our senses would be almost valueless. The senses being formed so much at variance with each other and fitted for such opposite uses, we cannot, I think, compare their functions so minutely, as to say, with one writer on Chromatics, that the sensibility of the eye becomes diminished "in the same manner as the palate, when long accustomed to a particular taste ceases to feel its impression."—*Library of Useful Knowledge, Optics*, p. 47.

8. The great difference between my theory and all that I have hitherto seen, is, that mine depends almost entirely upon physical causes; other theories upon organic causes; the effect taking place with them only when the rays reach the eye without which accidental colours have no existence; but according to my theory the divisions and combinations of colour occur *before* they reach the eye—they are in short, physical effects before they can even be seen. To support this view, I suppose a principle of *Homo-chromatic Attraction* to exist in nature, by which *Colorific rays of a like kind attract each other; while those of an unlike kind repel each other.* Amidst all the attractions which regulate and perpetuate the operations of nature, it may be presumptuous to deny the existence of such an attraction as I speak of; but, if colour be known only as a state of matter, and, like heat and electricity, its *cause* is still an unsolved problem, surely, I do not share in the presumption in my endeavours to support this principle by facts, which have long clashed with each other most discordantly, and which, admitting the principle of homo-chromatic attraction, are united into one distinct branch of science, harmonious, beautiful and extensive.

9. When I place a red wafer upon white ground, and fix my eye upon it for a second or two, a green ring or segment of a ring begins to play around the wafer, and on directing my eye to another part of the white paper, I see a green circular spot. Now, by the principle of homo-chromatic attraction, the red colour of the wafer exerts an attractive influence upon the red rays of the white paper. I believe, that under such circumstances, there is an affinity of red for red, and that this affinity extends to a considerable distance. If such be the case, the explanation is easy. The red rays immediately around the wafer are attracted to itself and leave the contiguous white paper deprived of red—hence, the green ring appears—the red colour of the wafer gradually becomes more sombre, because we see the red wafer through a faint accidental green, and green and red by super-position appear black: but there is only sufficient of the green to give a sombre appearance to the red, and not enough to make it black. The eye is removed to another part of the paper, and for a few seconds perceives a green spot; the red wafer has exerted its influence upon the white ground, decomposed it, in fact; retained a large portion of the red rays, and left the paper in a condition to send green to the eye. Thus, the red wafer exerts a collective influence over the red rays of the white paper, collects a number of them, and becomes more red itself,\* by depriving the white ground of a portion of its red rays,† the retina by persistence of impression retains the form, and the decomposed ground gives it a new colour; or, rather the eye had received the accidental impression from the ground immediately contiguous to the wafer, and bears the impression to any other part of the same ground. This impression lasts only for a short time, and in order to produce a new effect, the same process must be repeated.

\* While the eye is gazing steadily at a red wafer upon white ground, without shifting the eye, place another red wafer by the side of it, the second wafer will appear of a very light red, compared with the first, although the two tints are precisely the same.

† If we observe a green ground covered with white spots, (such as a lady's gown, &c.) the spots appear red; if the ground be blue and the spots white, the latter appear yellow, and so on. White stripes upon coloured grounds present similar appearances, and these effects strike the eye the moment the observations are made.

10. When the eye is regarding a wafer upon white ground, the accidental impression can be observed upon another white ground, remote from that upon which the wafer is placed. Thus, suppose a red wafer upon white ground be viewed for a sufficient time, and the eye be directed to the ceiling of the room, or to a contiguous sheet of white paper, a green image of the same form as the red wafer will be perceived. This is due to the persistence of the impressions of the retina which retains the image; and the colour is derived from the white ground on which the wafer is placed, the red of which ground is absorbed by the wafer, and the eye bears a portion of green, equal in extent to the size of the wafer, also by persistence of impression for a second or two, upon any other white ground. The accidental image then, is always the size of the red wafer; and while we regard the latter, and include a portion of the white ground, which it is almost impossible not to do, the accidental image overlaps the primary one, and we see a green segment on the white ground, and the red wafer is darker in tint because the accidental image is super-posed. If we move the eye away from the wafer upon the white ground, the accidental image moves off the wafer, and sometimes the primary image is lost sight of altogether, and the spectrum alone is retained. This often happens with me, and it may be that the primary image, in such case, falls upon the base of the optic nerve.

11. The experiments of M. Plateau may be objected to me, where the accidental colour is obtained when a coloured object, such as the red wafer is placed on a dull black ground instead of white. I admit, that in such cases, a very faint accidental image can be obtained, and do not consider such effects as objections to my theory; for as the wafer is seen by reflected light the incident light impinging on the wafer, is by the latter deprived of a portion of its red rays, and we see the reflected light yellow and blue forming green.

12. As it is my wish to strengthen this theory by facts observed under very different circumstances, I may be allowed to introduce the few following experiments, which, apart from all theory, will probably be found not altogether wanting in novelty and interest.

13. Let a number of tubes of bright coloured paper be formed by wrapping the paper *once* round upon a cylinder of wood, the edges of the paper being secured with paste; the coloured side of the paper forming the interior of the tubes. The dimensions may be about ten inches in length and one inch in diameter. If one of these tubes be applied to one eye, the other being closed, and the white ceiling be observed, the white circular spot will immediately become the accidental colour of that of the tube.

Suppose a blue tube be employed, a circular orange coloured spot will be seen upon the ceiling. Decomposition results as before: the white light from the ceiling enters the tube; the blue is retained, and the red and yellow rays enter the eye and produce the impression of orange.

14. Some beautiful illusions may be contrived and explained on the principle of chromatic attraction. (1.) Provide several pieces of paste-board about seven inches in length, and about four inches in breadth. Cover one side of each board with coloured paper, and arrange them in pairs, such as green and red; orange and blue; indigo and yellow, &c. Take one pair, and place them back to back; curve them a little outwards at each end, and place the other ends of one pair upon the top of the nose extending up the forehead: look at the white ceiling along the coloured boards in such a manner as that the right eye shall not see the colour along which the left eye views the ceiling, and vice versâ. If the pair of colours be well chosen, that is, complementary to each other, the colours will appear vividly depicted upon the ceiling, *and they will have changed places*. Thus, suppose the boards be indigo for the right eye, and yellow for the left, the ceiling will be divided into two parts; the division being mid-way between the two eyes; on the right hand will be a flood of yellow light, and on the left hand an equal portion of indigo. The observation is best made with the back to the window, and the observer should be seated, the head thrown back and kept steady.

(2.) Let one of the boards be covered with white paper and another with black. Cut out two disks, about half an inch in diameter each, one white and another black: place the white disk on the centre of the black board, and

the black disk on the centre of the white board : place the boards back to back as before, so that one eye may see the black and the other the white board, and on making the observation as before (except that instead of viewing the ceiling the disks must be viewed) the disks will appear to have changed places. Both these illusions, (1) and (2), are very perfect ; the effects are immediately obtained, so much so, that many persons to whom I have shown them attribute them to a species of natural magic, rather than simple physical experiments.

(3.) The black board may be employed alone, and the observations made, as has been just described, when the white disk will appear on the other side.

15. A white disk upon black ground becomes black and a black disk upon white ground becomes white, or in other words, white and black are complementary. These facts are easily explained on the principle of iso-chromatic attraction. A white disk on black ground reflects rays of all the three colours, and is surrounded by black which reflects nothing. The white is absorbed by the black, and we do not see the white disk at all, or we see it with black super-posed, (10) in which latter case, the white is changed into a deep colour, not quite black but approaching to it. A converse explanation, of course, applies to the black disk on white ground. The theory of the coloured boards is this : the coloured rays absorb homo-chromatic rays while the rest enter the eye, so that the right eye, for instance, looking along a red board at the ceiling sees green thereon, because the white light from the latter before entering the eye is decomposed ; the red of the board absorbing the red rays from the ceiling and yellow, and blue rays enter the eye. The left eye looking along a green board at the ceiling sees red : in this case, the green absorbs yellow and blue rays ; and the red rays are unobstructed in their passage to the eye.

16. Accidental impressions of colour present themselves to our notice in the majority of cases, under the form of coloured shadows. These phenomena are so extensive and so beautiful that they ought to claim by far the largest share of the attention of any one who would study accidental and complementary colours with ease, and under circumstances so well adapted for observation ; I shall, therefore,

in my next paper, which will appear in the ensuing number of the *Records*, state, first, the various means by which coloured shadows can be procured; and, secondly, the application of my theory to their explanation.

*Salisbury, November, 1835.*

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#### ARTICLE IX.

##### *The Atmosphere in relation to Malaria.*

THAT diseases, possessing a variety of types, originate from the existence of matter in the air we breathe, has been believed from the remotest ages. This is most distinctly true in woody or marshy countries, when attempts at colonization are first made. The fine town of Amaga, in Antioquia, Boussingault, (*Ann. de Chim.*, lvii. 150,) tells us, was founded in a forest. For six years, the population remained stationary; but, at last the roots and branches of the trees, having been completely removed in the lapse of a short time, the place became gradually more healthy, and it is now one of the most important little towns in the province. Panama, which was covered with wood, when the Spaniards first settled in it, possessed a deadly climate; but, at the present day, with the exception of the marshes of Chagres, the Isthmus is as healthy as any part of the coast of the Pacific in these latitudes. The influence of marshes in producing disease seems to be increased, when the water of the ocean becomes mingled with fresh water. Thus, Via Reggio, at the foot of the Appenines, in 1733, was so unhealthy, that the population did not exceed 330 inhabitants. But, in 1743, the sea-water was shut out from the lakes in the interior; and the consequence has been, that the town has become so salubrious, that the population, in 1823, amounted to 4000 souls. The town of Caitia, near Venezuela, from the same cause, is so unhealthy, that the negroes, when unable to pay their debts, make use of it as a sanctuary, well assured that their creditors will not have the temerity to pursue them thither. In such situations as we have described, where the climate is characterized by a high temperature and moist soil, the diseases of most

common occurrence possess a highly putrid type, as yellow and intermittent fevers; but where the principal defect in the climate is the existence of great accumulations of vapour in the atmosphere, scurvy is the prevalent malady. Thus, in Choco, where rain constantly falls, it is an uncommon circumstance to meet with an individual who is not affected with this complaint.

On the elevated plains of the Andes, on the contrary, where the atmosphere is extremely dry, the inhabitants are subject to violent attacks of ophthalmia, as the children of the African desert.

In all marshy countries, the inhabitants recommend the same precautions, in order to prevent the accession of the endemic disease. From America to India, the traveller is directed carefully to avoid exposing himself to the evening dew.

Moscatti took advantage of his knowledge of this fact, and endeavoured to insulate the miasma dissolved in the air over the rice fields of Tuscany, by condensing it along with the dew. He observed in the condensed liquid small flocks, which possessed all the properties of animal matter, while the water, in the course of a few days, began to putrify.

In 1812, M. Rigaud de l'Isle made a series of experiments in the marshes of Languedoc with the same object in view. He condensed the dew upon glass. It exhibited the same appearances as that examined by Moscati; but, in addition, it afforded a precipitate with nitrate of silver, which rapidly assumed a purple colour. But he endeavoured to prove that such dew was hurtful to animals when taken internally, which was attempting too much, because it is sufficiently obvious, that cattle pasture on marshy herbage without suffering the slightest inconvenience.

In 1819, Boussingault remarked, while engaged in a geological excursion through the department of Ain, that sulphuric acid, when placed in the immediate vicinity of a marsh, became speedily black, while at a distance from the putrifying source it was not altered. At this period, fever was raging in the neighbourhood, and, hence, he concluded, that the change in the acid was produced by the agency of

organic matter, which existed in the air. Impressed with the idea, that sulphuric acid was a delicate test of the presence of organic matter in the atmosphere, on his departure to America, he consulted M. Humboldt, who approved highly of the suggestion. When in company with M. Rivero, he arrived on the banks of the lake Taricagua, the dry season had set in, the waters of the lake had greatly subsided; the ground, which had been inundated by the rains, was now emitting abundance of effluvia, and dire fever raged among the unfortunate natives. Thus was an excellent opportunity presented of ascertaining the accuracy of the opinion in reference to the agency by sulphuric acid. A quantity of very pure acid was, therefore, exposed to the air for 12 hours. At the termination of that period it had acquired a deep black tinge. No inference could, however, be deduced from this experiment, because the striking effect which he observed might be produced by the numerous insects which swarmed in the air.

In 1829, he operated in a different manner. He was then at Cartago, in the valley of the river Cauca, which in its sluggish course gives origin to several Lagoons, from whence, during the prevalence of the south wind, the malaria is propelled to the town, and produces abundance of disease: such was the state of matters when Boussingault visited the place. A little after sunset, he placed two watch glasses on a table in the middle of a marshy meadow. Into one of the glasses he poured warm distilled water, in order to moisten its surface, and to raise its temperature above that of the air. The other glass being cold was speedily covered with dew, the warm glass received no condensed liquid. When a drop of distilled sulphuric acid was added to each glass and evaporated by the heat of a spirit lamp, a trace of carbonaceous matter was always detected in the dew glass, while none was observable after volatilizing the acid alone. He repeated his experiments, during several evenings, but was then laid on a bed of sickness, by the agency of this very substance which he was endeavouring to detect. These trials shewed the presence of organic matter; the next object was to ascertain its quantity. Admitting that miasma as other organic substances contained hydrogen, he considered that

if the quantity of this gas were appreciated, the great step would be gained. For this purpose, in 1830, he passed a given weight of impure marshy air through a red hot glass tube, and collected the water formed in a tube filled with chloride of calcium. A quantity of air weighing from 4697 grs. to 4774 grs. in different trials, afforded  $\cdot77$  gr. water, which is equivalent to  $\cdot077$  gr. hydrogen. When the air, previous to being passed through the hot tube, was brought in contact with sulphuric acid, it yielded no organic matter. Hence, he concludes, that it is very probable, that the malaria is a flocky matter, and that the employment of a veil as a precaution against the influence of malaria may be efficacious.

The preceding experiments, did not apparently indicate the presence of hydrogen, when it had been previously washed with sulphuric acid. To determine this precisely, it was necessary to experiment with great nicety. A vessel with sulphuric acid was made to communicate on one side, with a gasometer filled with common air, and on the other led into a tube 8 or 10 feet long, filled with chloride of calcium. To the latter, was joined a glass tube filled with asbestos, moistened by sulphuric acid; a tube filled with copper turnings and passing through a furnace followed next, and conducted the air into another tube filled with asbestos, soaked in sulphuric acid. In the last tube the water was condensed, when any hydrogen was contained in the air submitted to experiment.

From 1609·6 grs. were obtained water= $\cdot0172$  gr. hydrogen  
 ,, 5482·4 ,, ,, = $\cdot0443$  ,,  
 ,, 6498·8 ,, ,, = $\cdot0477$  ,,

The proportion of hydrogen formed in the air on different days is represented in the following table :

*Hydrogen in one part of Air.*

	Weight.	Volume.
2nd and 3rd April, 1834,	0·000008	0·00013
4th and 5th ,,	0·000007	0·00012
23rd ,,	0·000002	0·00004
28th ,,	0·000005	0·00008
31st May,	0·000003	0·00005

Boussingault, having thus, as he conceives established the

fact, (although this inference might be considered premature) proceeds to speculate upon the nature of the compound, of which the hydrogen forms a constituent. He considers it to be in union with carbon, forming carburetted hydrogen.

Saussure inferred from his experiments that an inflammable gas existed in the atmosphere with a base of carbon. Boussingault ascribes its origin to exhalation from the earth, in the decomposition of vegetable matter, and also from mineral sources.

Near the falls of Niagara there is a fertile source of it in the *Burning Spring*. In Italy and Sicily it occurs in abundance, and in China, about Kratnig-fou there are within 50 square leagues, no less than 10,000 salt pits, from all of which, inflammable gas is emitted. Near the town of Kioung-tcheou, there is a pit of  $5\frac{1}{3}$  feet deep and 16 in diameter. The gas proceeding from this pit, when set on fire, forms such a blaze that the whole country is illuminated during the night to a great extent. Near Bakow, likewise, according to Imbert, there are some extensive sources. Discharges of the same nature frequently occur in this country. Dr. Thomas Thomson has described one, which existed in the neighbourhood of Glasgow, where the gas burst forth in different parts of a hill side. When examined, it was found to contain only 12 per cent. of common air.

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ARTICLE X.

*State of the Austrian and Hungarian Mines.* By Messrs. FOY, HARLE, and GRUNER.\*

THE *Tyrol* contains very few mines which are now worked, although the country is rich in copper and silver.

The mines of Schwatz, on the Inn, are abandoned.

At Kitzbübel, 1200 quintals† of copper are procured annually from pyrites in veins in clay slate.

The *Tyrol* produces only 35 marcs (22 troy lbs.) at Zell.

The annual product of iron manufactured is 10,000 quintals., and of steel 2000. At Hall, there is a large saline deposit, from which 200,000 quintals of salt are raised, at 1s. 10½d. per quintal, and sold at 10s. 10d. per quintal. Besides,

\* Ann. des Mines, Tom. V. † A quintal is equivalent to 109·81 lbs. Avord.

sal-ammoniac and carbonate of magnesia are manufactured from urine.

At *Salzburg*, as in the Tyrol, the brown hematites are fused with a feeble heat. The quantity produced is 15,000 quintals. At one time, a number of gold mines existed here, in the central chain of the Alps, but at present there are only two, Bockstein and Rauris, which yield 100 to 200 marcs ( $62\frac{1}{2}$  to 125 lbs.) of gold by amalgamation, and 600 to 800 marcs of silver (375 to 500 lbs.) extracted by the fusion of sulphur minerals. Salzburg contains several deposits of salt, which rival those of Bavaria. The springs which they supply are at Hallein, Hallstadt, &c.

*Carinthia* produces iron, steel, lead and 200 or 300 quintals of copper. Lead is produced from galena in a calcareous matrix at Bleyberg to the amount of 60,000 quintals, by means of a reverberatory furnace, and is extremely pure and free from silver. 300,000 quintals of iron are cast, which is all converted into soft iron or steel, either in Carinthia or Carniola. This casting is operated in six high close furnaces. That of Treibach is 35 feet high and 8 in diameter. Its daily produce is sometimes from 250 to 300 quintals. The ore comes from Hüttenberg, and is partially decomposed sparry iron-stone, very rich in mica, and found in mica slate. It is often accompanied with sulphate of barytes, which is frequently used to adulterate ceruse. Steel is prepared by the *brescienne* method, and the soft iron by the Styrian method at a single fusion. The manufacture of iron has made great progress here, if we may judge from the attempts made by Rosthorn on the puddling process, by means of wood and lignite. 180,000 quintals of soft iron, and 70,000 of steel are raised annually, at the price of 17s. 6d. per quintal.

*Carniola* produces mercury at Idria. 5000 to 6000 quintals of mercury are obtained by distillation. 100 quintals are extracted from a slate containing native mercury, and, besides, 1000 quintals of cinnabar are formed. The mercury sells at 16s. 8d. per quintal. and upwards; the cinnabar at £1. and above per quintal.

*Styria* is the province which raises the greatest quantity of soft iron and steel, both of a superior quality. In this province there are 37 high furnaces, only three of which

are open; but the others are close, with a height of from 19 to 36 feet. At Voderberg there are 14 furnaces, and at Eisenerz four, which derive their carbonate of iron from the same mine, in a mountain 2000 feet high, consisting of sparry iron-stone. The mine is in the open air, and from the different parts of the mine, the ore is carried by railways. One of these is 3 leagues and the other 5, in length. Charcoal is used for fuel. The quantity of iron and steel produced is 400,000 quintals., at 16s. 8d. per quintal.

In *Hungary*, at Schemnitz and Kremnitz, where the mines have been worked for 1100 years, the ores occur in thick veins, in dioritic porphyry surrounded by trachyte. The neighbourhood of Schemnitz, though poor in running water, is well supplied with ponds and canals, which collect the rain water from the neighbouring mountains. A steam engine is in action here. For several years, this mine cost more than it produced, but that of Kremnitz affords a profit. The annual produce in Lower Hungary is 35,000 marcs (22,737 lbs.) of silver, and above 600 marcs of gold. Upper Hungary and Nagybania produce annually 30,000 marcs.

Lower Hungary affords nearly 6000 quintals. of copper, Upper Hungary 18,000 quintals., the Bannat 6000. Hungary yields but little iron.

*Bohemia*, rich in all the metals, exhibits most of its mines in an abandoned state. Gold is procured in the rivers Zasava and Wattawa, near Prague, and there is a mine of it in the Eulengebirge. 1000 marcs of silver are raised at Ioachimsthal.

The mine at Przibram yields 22,000 marcs of silver, and 20,000 quintals. of lead. At Miess, 10,000 quintals. are raised. Bohemia furnishes 200 quintals. of copper, besides white arsenic and blue cobalt. There are 40 or 50 high furnaces in this country which produce 400,000 quintals. of iron; 500 quintals. of tin are raised. Oil of vitriol is manufactured to a considerable extent here, by calcining blue vitriol obtained from the washings of the slates.

The principal manufactures are at Radnitz, and the annual product is 25,000 to 30,000 quintals. of fuming acid. Porcelain is manufactured. The coal is much neglected from the abundance of wood. There are two great rail-

roads; one from Linz to Budweis, and the other uniting Prague with Bavaria; a third is projected from Linz to Trieste. *Moravia* produces 60,000 quintals of soft iron.

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## ARTICLE XI.

### ANALYSES OF BOOKS.

*On the Poisonous Properties of the Hemlock and its Alkaloid Conia.* By ROBERT CHRISTISON, M. D., F. R. S., E., &c.—*Transactions of the Royal Society of Edinburgh*, Vol. XIII.

THIS paper is devoted to some important investigations with regard to the active principle of conium, particularly as to its chemical and physiological properties.

The well known hemlock of our woods and hedges, or *Conium maculatum* of botanists, has been long employed in medicine in the form of extract and tincture; but strange though it may appear, the nature of the efficient principle in these preparations was only explained a few years ago. In 1827, Giseke succeeded in concentrating the active principle of the plant in a compound with sulphuric acid of such power that two grains killed a small animal in 55 minutes. But it was not till 1831, that Geiger succeeded in separating an oily alkaloid from the extract. He effected this, by distilling with water and caustic potash or lime, neutralizing the liquid with sulphuric acid, and distilling the liquid over. The residuum consists of sulphate of conein, sulphate of ammonia, and resinoid matter; the resin and ammonia being produced by the decomposition of part of the conein. In order to obtain the latter, the mass is subjected to the action of a mixture of two parts of rectified spirit and one of sulphuric ether, which leaves the sulphate of ammonia undissolved. And then the ether and alcohol being distilled carefully off, the remaining sulphate of conein is gently heated with a little water and caustic potash, upon which, there is obtained in the receiver a watery solution of conein in the lower part, and floating on this a layer of nearly pure conein, colourless, transparent, and presenting the appearance of an oil. A little ammonia and water are still present; the former may be removed by exposing it under the vacuum of an air-pump as long as bubbles of gas continue to escape. This alkaloid exists in greater proportion in the seeds than in any other part of the plant. Geiger ascertained that the dried leaves of hemlock, and some extracts of their juice do not contain any conein. Dr. Christison has also been able to corroborate these statements. He has found extracts unquestionably well prepared at first, entirely destitute of conein in the course of a short time. A remark, he observes, "which applies even to the superior extract prepared by Mr. Barry, of London." This is also in consonance with our own experience of the London extracts. The only extract we have found to keep, is that described in *Records*, iv. 72. Of the various extracts examined by Dr. Christison, the largest proportion of conein

was afforded by one prepared from the ripe seed by alcohol. 220 grs. gave 5 grs. of colourless hydrate of conicin. There cannot be a doubt that the substance thus obtained is the active principle of the conium, as the smell and physiological properties are identical. It acts strongly as a local irritant. It has an acrid taste. When dropped into the eye or on the peritoneum it causes redness or vascularity, and always produces pain, whatever be the texture to which it is applied. The effects which ensue, are swiftly spreading palsy of the muscles, affecting first those of voluntary motion, then the respiratory muscles of the chest and abdomen, lastly, the diaphragm, and thus ending in death by asphyxia. The paralytic state is usually interrupted by slight convulsive twitches of the limbs and trunk. When neutralized by an acid its energy is rather increased. Hence, Dr. Christison considers that the discovery of an antidote is problematical. The most reasonable method of attempting to overcome its power appears to us to be its decomposition, since we find that it is very liable to be converted into ammonia and other products. It is extremely rapid in its action, approaching prussic acid more nearly than any other substance with which we are acquainted. A single drop put into the eye of a rabbit killed it in nine minutes; three drops used in the same way killed a strong cat in a minute and a half; five drops poured into the throat of a small dog began to act in thirty seconds, and in as many more, motion and respiration had entirely ceased. When 2 grs. were injected into the vein of a dog, in two or three seconds, respiration ceased. Hence, Dr. Christison concludes, that this poison acts most probably through the medium of the circulation, or by absorption, whence it is conveyed to the spinal cord, although the rapidity of the death renders it necessary to suspect, he conceives, that the inner membrane of the blood vessels communicate the action of the poison to the nervous system. The recent experiments of Tiedemann, however, set aside the necessity of this explanation, as from them we have proof that many poisons actually are exhaled by the respiration almost instantly after they have been injected into the vessels of the posterior legs of animals. It is quite obvious, therefore, that these bodies must traverse the blood vessels with the most astonishing velocity.—*See Brit. and For. Med. Rev.*, No. I.

Dr. Christison terminates his paper with an interesting inquiry into the history of hemlock. He concludes that the poison which terminated the existence of Socrates was not our hemlock, or, at least, the description given of the symptoms produced by the action of the poison do not correspond with those of the hemlock known to us. Plato says, "when he felt his limbs grow weary, he lay down on his back, for so the man had told him to do, and at the same time the person who administered the poison went up to him, and examined, for a little while, his feet and legs, and then squeezing his foot strongly, asked him whether he felt him do so? Socrates replied, that he did not. After this the man did the same to his legs,\* and proceeding upwards in this way, shewed that he was cold and stiff. And he approached him and said

\* Knees—*κνημας*?—EDIT.

to us, that when the effects of the poison should reach the heart, Socrates would depart. And now the parts about the lower belly were cold, when he uncovered himself, (for he was covered up), and said, which were his last words, 'Crito, we owe Æsculapius a cock, pay the debt, and do not forget it.' 'It shall be done,' replied Crito, but consider whether you have any thing else to say.' Socrates answered not, but in a short time was convulsed.

The following were the phenomena observed when conein was administered by Dr. Christison: "6 drops of conein were allowed to fall into the back of the throat of a young active puppy 10 weeks old. In 30 seconds there was sudden convulsive respiration and some stiffness of the hind legs, immediately followed by great feebleness of these legs. In a few seconds the fore legs became also very feeble. In 60 seconds from the time the poison was introduced the breathing ceased. Slight convulsive tremors followed for a single minute more." The author concludes, that the ancient poison is still unknown to us.

Since the publication of this excellent memoir, Messrs. Charlard and O. Henry have made considerable additions to the history of conein, having ascertained that it forms crystallizable salts with some of the acids.—*See Ann. de Chim.*, lxi. 337, *Journ. de Pharm.*, June, 1836, and the present volume of the *Records*, page 230.

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## ARTICLE XII.

### SCIENTIFIC INTELLIGENCE, &c.

#### I.—*British Association for the Advancement of Science.*

*Bristol, 26th August, 1836.*

THE Association began its Sixth Meeting on Monday, 22nd August, here. In consequence of the unavoidable absence of the Marquis of Landsdowne, the chair was occupied by the Vice Presidents in rotation, viz., the Marquis of Northampton and the Rev. Mr. Conybeare. It would be impossible to give any adequate view of the proceedings of the various sections with which we are more immediately connected, from the very short time we have had to prepare even this brief notice. (Several of the sections, indeed, have not concluded their proceedings.) We, therefore, confine ourselves to the operations of the Chemical section. This section sat in the Grammar School, Unity Street.—*President*, Rev. Professor Cumming.—*Vice Presidents*, Dr. Dalton, Dr. Henry.—*Secretaries*, Dr. Apjohn, Dr. C. Henry, W. Herapath, Esq.—*Committee*, Dr. Barker, Professor Daubeny, Mr. C. T. Coathupe, Rev. N. Vernon Harcourt, Professor Hare, Mr. Johnston, Mr. Lowe, Dr. Thomas Thomson, Dr. R. D. Thomson, Dr. Turner, Mr. T. Thomson, Jun., Mr. H. H. Watson, Mr. W. West, Rev. W. Whewell, Colonel Yorke, Mr. Scanlan.

*Monday, 22nd August.*—1. *Mr. Watson* read a paper on Phosphate of Soda, in which, he directed his attention to the Atomic Weight of this salt. No allusion, however, was made to the purity

of the re-agents. He observed, that when the pyro-phosphate of soda and the phosphate were precipitated by lime-water, the pyro-phosphate of lime, when ignited, assumed a black colour, while the common phosphate remained white. The author also mentioned, that, contrary to what was supposed, pyro-phosphate of soda might be kept in solution for, at least, a year and a half without being re-converted into the usual phosphate.

2. *Mr. Ettrick* illustrated and explained an Improvement in the Blowpipe, by which the blast of the common blowpipe was made as equable as that by water pressure.

3. *Mr. Herapath* gave the results of the Analyses of Two Mineral Waters in the neighbourhood, and detailed his mode of analysis.

4. The same gentleman read a paper on the theory of the Aurora Borealis. He stated that as far as he had observed this phenomenon, it was always situated low in the sky, and in connexion with clouds. Hence, he inferred that it is occasioned by electricity passing from the clouds. This paper gave rise to a discussion, in which *Dr. Dalton* stated it as his conviction, from very numerous observations that the Aurora is always high in the atmosphere, and that the clouds referred to were not clouds but arches of darkness, through which the stars might be observed, and produced by the contrast of the light beneath.

*Tuesday, 23rd August.*—1. *Mr. Exley* read an exceedingly talented paper, developing a new theory of chemical combination deduced from mathematical data, and demonstrated mathematically. The reading of this paper excited a very great degree of interest. It promises to throw a great deal of light upon the theory of chemistry.

2. *Dr. Charles Henry* read an account of some experiments made with a view to determine the mode in which certain gases act in preventing the action of spongy platinum upon a mixture of oxygen and hydrogen. The gases which he examined were carbonic, oxide and olefiant gas. He found that carbonic oxide was the most powerful, and that carbonic acid is always the result. Hence, it is evident that oxygen and hydrogen are prevented from combining by the superior attraction of the carbonic acid for the oxygen. Olefiant gas he found not to be decomposed, and hence the attraction which prevents the combination is not sufficiently powerful to form any other. This explanation is corroborated by the fact that it requires a very great proportion of the olefiant gas to produce the effect.

3. *Mr. Herapath* stated some facts respecting the Arsenical Poisons, and illustrated them by experiment.

*Wednesday, 24th August.*—1. *Dr. Daubeny* read a very copious and interesting report on the present state of our knowledge with respect to thermal waters.

2. *Mr. Mushet* exhibited some specimens of metallic iron prepared by exposing the ore to long continued heat with a small quantity of fuel, and thus reducing it to the metallic state without fusion.

3. *Mr. Johnston* described paracyanogen and its compounds.

When cyanogen is prepared from the bi-cyanide of mercury, even though perfectly pure, there is always a black matter left behind in the retort. Mr. Johnston ascertained that the cyanogen given off was perfectly pure, being all absorbed by caustic potash. Hence, he inferred from the known composition of the bi-cyanide of mercury, that this black matter must have the same composition as cyanogen, and this both Leibig (whom he requested to submit it to analysis) and himself have confirmed by experiment. The same substance is also procured if the brown precipitate obtained, where the aqueous or alcoholic solution of cyanogen is kept, be heated to redness in a close vessel. Paracyanogen, as the author terms this compound, he describes as a black or brownish-black shining substance not acted on by any other menstruum but sulphuric acid. When thrown into boiling nitric acid, a solution of paracyanic acid is obtained, giving yellow or brownish salts, with persalts of mercury and salts of silver. The composition of paracyanic acid, Mr. Johnston states to be (2 Az 4 C) O, thus differing entirely from cyanic, which is (Az 2 C) O.

4. *Mr. West* submitted to the section a proposition for determining the quantity of various gases in the atmosphere by passing large quantities of air through various absorbent liquids. *Dr. Dalton*, in answer to this suggestion, stated that he considered the quantity of carbonic acid in the atmosphere always as uniform, even in towns. The quantity amounts to 1 in 1100. He stated that he had analyzed air from the top of a high mountain, and had found the same quantity of carbonic acid in it as in the air of the atmosphere. It was generally understood in the section that *Dr. Dalton* had procured the air by inverting a bottle of water; the residual water of course would absorb the carbonic acid, and it was in order to prevent this source of error that *Mr. West* proposed to operate upon large quantities.

5. *Dr. Hare* read a printed paper (which was distributed among the members of the chemical section at Edinburgh, two years ago) on the nomenclature of *Berzelius*.

*Thursday, 25th August.*—1. *Dr. Daubeny* stated that he had found that the sublimation, (as he had considered it at the last meeting of the association,) of carbonate of magnesia was entirely a mechanical process. Hence, the explanation of *Von Buch* of the formation of Dolomitic rocks, viz., by limestone becoming impregnated with magnesia through the agency of heat is incorrect.

2. *Dr. Dalton* made some observations on Atomic Symbols, in continuation of those made at the Dublin Meeting.

3. *Mr. Johnston* explained the use of some Chemical Tables, which he exhibited to the section.

4. *Dr. Thomson* read a paper on Mixtures of Sulphuric Acid and Water; in which he shewed that the theory of *Iroine* respecting specific heat cannot be true. He offered a very simple explanation of the cause of the evolution of heat when mixtures are made.

The paper will appear in the next number of the *Records*.

*Friday, 26th August.*—1. *Mr. Scanlan* exhibited and described a solid yellow body crystallizing in prisms, which rises when pyrolygneous acid is distilled over lime; the latter saturating the acetic

acid, pyroxylic spirit passes with the new compound. According to Dr. Apjohn, who analyzed it, 4 grs. afforded of Carbon 2·967, Hydrogen ·343, Oxygen ·790. Its composition agrees with  $C^{10}H^5O^2$ .

2. *Mr. Cross* made some remarkable observations on Atmospheric Electricity. He had constructed an isolated electrometer by passing a wire on the extremity of a long pole 110 feet high for a mile into the atmosphere. By means of this apparatus he had found that the atmosphere is always positively electrified. The electricity after sunrise is always at its maximum; the quantity of electricity from fogs is enormous. Every thunder-cloud, he found, consisted of positive and negative electricity in the form of zones, consisting of insulated particles of water. *Mr. Cross* had formed a young thunder-cloud by breathing on a pane of glass, and electrifying the middle by a spark from a Leyden phial, and thus formed zones. He has never observed any effect upon the wire by meteoric lights, nor by sheet lightning. He has constructed a battery in which no acid is used; the cells being insulated and the connecting medium being water. By means of this battery he has crystallized many minerals, as calcareous spar in 10 days, silica, &c. Between 7 and 10 A.M. the battery is at its maximum, and at its minimum between the same hours in the evening. Crystallization is more rapidly effected in the dark than in the light. If the shutters of the rooms be opened before 7 A.M. the maximum is lower.

3. *Dr. Trail* on the *Aurora borealis*.

## II.—Pharmacy, &c.

1. *Gamboge*.—According to *Dr. Graham* the substance is the product of the *Garcinia* or *Mangostana Morella*. *Dr. Christison* states, that the present gamboge of commerce does not come from Ceylon but from China, as in the time of *Bontius*. He found gamboge in sticks from Siam, to consist of resin 72·2, arabin 23, water 4·8. Gamboge in cakes from Siam, consisted of resin 6·48, arabin 202, fecula 5·6, woody matter 5·3, water 4·1. Gamboge from Ceylon, sent by *Mr. Walker*, contained, resin 70·2, arabin 19·6, fibres of wood and bark 5·6, water 4·6. Gamboge of Ceylon—resin 75·5, arabin 18·3, cerasin 0·7, water 4·8. He considers the resin to be the active agent, and concludes from the composition that the Siam cakes are not natural productions, but a manufacturing product, and that the gamboge of Ceylon, being free from woody matter, has the same composition as the gamboge of Siam, and both are consequently from the same plant.—*L'Institut*, 166.

2. *Canella Bark*.—According to *Nees von Esenbech*, there are three trees from which canella is derived; viz., *Laurus cinna momum* affording the canella of Ceylon; *Laurus cassia* producing the Chinese canella, and the *Laurus Malabathrica*, the bark of which is nothing else than the *Cassia lignea*.—*Guibourt Hist. des drogues*, vol. ii.

3. *Concin*.—According to the experiments of *Geiger*, *Christison*,

Charlard, and Henry, is an oily liquid of a yellowish colour. It is completely soluble in ether and alcohol, and is lighter than water which dissolves a small portion of it. Its smell is strong, resembling that of the hemlock, or tobacco, or mice. Its taste is very acrid and corrosive. It proves fatal to animals in very small doses and with great rapidity. It readily dissolves in acids, and produces with sulphuric, phosphoric, nitric, and oxalic acids, combinations which crystallize in the form of prisms. During the saturation the liquor assumes a bluish green tint which passes into reddish brown, and when they are evaporated, either at a gentle heat, in vacuum or in the open air, they lose, as the ammoniacal salts do, a part of their base, the smell of which is very distinct; the salts of conein attract water very quickly from the atmosphere, and are soluble in alcohol. The nitrate of conein decomposed in the fire gives origin to brown pyrogenous products. When placed in vacuo with bodies which attract much water, it partly volatilizes and leaves a reddish residue, which appears to be a hydrous conein. The vapour of conein is inflammable and produces white fumes in a tube filled with muriatic acid gas. The solutions of the salts in water form a cheesy precipitate with tannin, soluble in alcohol. A white precipitate is also formed when a solution of conein in alcohol of 30° is mixed with iodic acid. According to Liebig, conein consists of carbon 66.91, hydrogen 12.00, azote 12.80, oxygen 8.29. Charlard and Henry erroneously state that this is the first instance of a vegetable alkaloid being liquid. They have entirely overlooked *Nicotin*, the narcotic principle of tobacco, which has been shewn by Mr. E. Davy to be a liquid.—(*Records*, ii. 204.) Conein may be obtained by distilling green hemlock with caustic soda, and receiving the product into a receiver containing a little dilute sulphuric acid.

4. *Pectic acid in Gentian*.—M. Bussy took 8 ounces of the root of the *Gentiana lutea* in rough powder, and poured over it 8 ozs. of pure water. In a quarter of an hour, he placed the powder thus moistened in a suitable apparatus. He then poured pure water over it in small portions at a time, taking the precaution not to add any more until the first was absorbed. In the course of a few minutes, a very deep brown liquid, of a thick consistence and transparent aspect, strained through into the receiver. He continued the washing until he had obtained 12 ounces of the liquid; when the colour became less deep, he changed the receiver and continued the washing, for which purpose, 3 kilogrammes of water were employed. The powder possessed then an insipid taste and slight colour. In 12 hours, the liquid in the first receiver had acquired the consistence of very firm jelly. It was completely soluble in a solution of carbonate of potash. From this solution it was precipitated in the form of a jelly by alcohol, sugar, &c., shewing that it was pectic acid. Bussy subsequently extracted it from gentian by the same process, followed by Braconnot in separating it from carrots. He suggests that the pectate of soda in solution may be substituted for eggs in clarifying sugar, and for gelatin in clearing wine, &c.—*Journ. de Pharm.*, June, 1836.

5. *Guaiac Wood (Guaiacum Officinale)*.—According to Righini,

the wood of this plant, when treated with alcohol, gives a resin with an aromatic odour. 2. When the alcohol is distilled, the resin is found to be mixed with a liquid like coffee and milk, which, when evaporated, forms an extract, which, when melted with the peculiar resin, forms a homogeneous substance. 3. The white liquor remaining in the alembic contains a bitter principle of an oily consistence held in suspension by the gummy extractive matter. If this be made into a paste with magnesia, and distilled from a glass retort, a limpid aromatic liquid comes over. Dividing the *caput mortuum* by means of pure water, and in decomposing by dilute sulphuric acid, sulphate of magnesia is formed, and a white substance is deposited, which, when dissolved in alcohol, and evaporated, forms a body susceptible of crystallizing in needles. The author terms this substance *Guaiacic acid*, which is quite distinct from *benzoic acid*. 4. The brown resin of guiac, when heated in a retort on hot coals, assumes a red appearance, and disengages an oily fluid, possessing the taste and smell of creosote. When purified, it constitutes the creosote of Reichenbach.—*Journ. de Chim. Medic.*, July, 1836.

6. *Pellitory Root (Pyrethrum)*.—According to Koene, this root, when subjected to successive treatment with ether, alcohol, cold and boiling water, afforded, 1st, a very acrid brown resinous substance, insoluble in a solution of caustic potash 0.59. 2nd. A fixed oil of a deep brown colour, acrid and soluble in potash 0.60. 3rd. A yellow oil, acrid, equally soluble in potash 0.35. 4th. Traces of tannin. 5th. Gummy matter 9.40. 6th. Inulin 57.70. 7th. Sulphate, muriate and carbonate of potash, phosphate and carbonate of lime, alumina and silica, oxides of iron and manganese 7.60. 8th. Woody matter 19.80. 9th. Loss 2.6.—*Journ. de Pharm.*, Feb., 1836.

7. *Creosote*.—Koene has succeeded in obtaining a large product of this substance from coal-tar. He distils the tar in a retort supplied with a large wide beak, under which he places a capsule. At first, a light oil comes over, but by changing the receiver from time to time, a heavy oil is obtained; the distillation is continued, elevating the temperature until the naphthaline is condensed in the neck of the retort. A certain quantity of creosote remains in the beak united to the naphthaline; the product falls into the capsule on applying heat. It is then exposed to a cold mixture, and the naphthaline taken up by expression. To obtain the whole of the creosote, the expressed naphthaline is heated with its weight of pyroligneous acid until it melts. On cooling, the naphthaline crystallizes, and may be separated by pressure from the liquor, which is then saturated with sub-carbonate of potash. The heavy oils obtained are treated successively by  $\frac{1}{40}$ th of their weight of phosphoric acid, and by an equal volume of water. It is then rectified. The product is dissolved in potash, and the free creosote isolated by a slight excess of dilute phosphoric acid. It is again rectified, the water separated, and pure creosote obtained.—*Ibid.*

### III.—*Impurity of Zinc and Sulphate of Zinc.*

WITTSTEIN found in the zinc of commerce at Munich, zinc 98.76, lead 0.91, cadmium 0.16, iron 0.17. The sulphate of zinc, he found

to consist of water 42.20, sulphuric acid 29.48, oxide of zinc 22.20, oxide of manganese 1.58, magnesia 1.88, iron 1.7.—*Central blatt.*, June, 1836.

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#### IV.—Utility of Carbonate of Barytes in Analyses.

DR. ABICH has ascertained that carbonate of barytes fuses at a white heat and loses its carbonic acid, and, in consequence of this property, it affords an excellent means of decomposing aluminous minerals. It is not necessary that the mineral to be fused with it should be converted into a fine powder; it is only necessary to bruise the mineral in a steel mortar into a rough powder, to mix it with from 4 to 6 times its weight of carbonate of barytes, and expose the mixture in a platinum crucible for 15 or 20 minutes to a white heat; a mass is produced which is quite soluble in muriatic acid. In this way, Abich readily decomposed cyanite, staurolite, andalusite, cymophane, zircon and felspar. He recommends that the platinum crucible should be introduced into a large Hessian crucible, which should be covered with an appropriate lid, luted to the body of the crucible. It should then be placed on a proper support, in a blast furnace, (one of Sturm's, termed Swedish forge, and described in Berthier's *Traite des Essais*, is to be preferred,) surrounding the crucible with charcoal and coke.—*Ann. de Chim.*, lx. 369.

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#### V.—Adulteration of Nitrate of Silver.

AN apothecary of Giessen has found this salt adulterated with nitrate of lead and oxide of zinc, to the extent of one half.—*Ann. der Pharm.*, xvii. 87.

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#### VI.—Combination of Sulphuret of Lead with Chloride of Lead.

HÜNEFELD states that when we bring a dilute solution of acetate of nitrate of lead, rendered acid with muriatic acid, in contact with sulphuretted hydrogen water, a reddish yellow precipitate is formed, which is converted into a brown or black state by an excess of sulphuretted hydrogen. The same occurs with a solution of chloride of lead. Analyses of this precipitate gave 45 sulphuret of lead and 35 chloride of lead. He considers it a chemical compound.—*Journ. für praktische Chemie*, vii. 27.

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#### VII.—Chloro-Sulphuret of Antimony.

ACCORDING to Cenedella, when sulphuret of antimony and chloride of calcium are heated together, a dark lead-coloured sublimate of shining needles is obtained, consisting of  $\text{Sb Su}^2 \text{Cl}^2$ . The *Panicea Cinnaberina* he finds to consist of  $\text{Hg Su}^2 \text{Cl}$ ; and Mosaic gold he states contains, besides bisulphuret of tin, some chlorine.—*Gazetta Eclett.*, 1835, 23.

VIII.—*Gallic Acid in Crystals.*

MURATORI obtains gallic acid in crystals by forming a decoction of nutgalls, filtering, adding  $\frac{1}{5}$  or  $\frac{1}{6}$  of its weight of alcohol, in a well closed flask, and allowing it to remain for 15 or 20 days in a cool place. Gallic acid falls to the bottom in the form of a light yellow crystalline mass; the alcohol should be distilled off, the solution concentrated,  $\frac{1}{5}$  or  $\frac{1}{6}$  of alcohol again added, and a new quantity of gallic acid thus obtained.—*Central blatt.*, Feb. 1836.

IX.—*Action of Oxalic Acid upon Sulphate of Iron and Sulphate of Copper.*

VOGEL, of Munich, has found that as oxalic acid decomposes gypsum in consequence of its affinity for lime, it also completely separates from the sulphuric acid, salts of iron and copper: consequently, it appears to have a greater affinity for these metallic bases, than even the sulphuric acid itself. It is probable, also, that oxalic acid decomposes such sulphuric acid salts as have a base of oxide of zinc, manganese, cadmium, &c., as Rose obtained precipitates from solutions of these salts. The protoxalate of iron is a yellow powder, almost insoluble in water, leaving, when heated in close vessels, a plumbago-looking matter. The oxalate of copper is a blueish white powder, insoluble in water, leaving behind, when heated, metallic copper, with a small quantity of suboxide of copper.—*Journ. fur Prakt. Chim.*, iii. 343.

X.—*Ferrocyanodide of Ammonia.*

It is commonly stated that this salt may be obtained in octahedrons by digesting pure Prussian blue in ammonia. Bunsen has found this not to be the case, and recommends digesting ferrocyanodide of lead with carbonate of ammonia. It is white or yellowish, translucent; easily soluble in water but not in alcohol. The aqueous solution is decomposed by boiling, and by exposure to the air at common temperatures. When the salt is mixed with chloride of sodium; ferrocyanodide of ammonia, acid muriate of ammonia, and ferrocyanodide of sodium are formed. It consists of ferrocyanogen 31·93, cyanodide of ammonia 50·92, water 17·15. The salt described by Berzelius has a different crystalline form and contains only one atom of water.—*Central blatt.*, Feb. 1836.

*Ferrocyanodide of Ammonia and Muriate of Ammonia* is obtained by digesting Prussian blue with ammonia and sal ammoniac, or by crystallizing a solution of sal ammoniac and ferrocyanodide of ammonia. The best proportions are 2 parts Prussian blue, 2 parts sal ammoniac, and 12 parts water; boil the solution, and filter it. To obtain the salt completely pure, it should be evaporated in vacuo over sulphuric acid. The crystals are sharp rhombohedrons with lateral angles =  $96^{\circ}52$ , and the angles of the base =  $82^{\circ}40$ . The colour is wine or citron yellow. When heat is applied, oxide of iron alone remains. It consists of ferrocyanogen 25·68, cyanodide of ammonia 38·01, muriate of ammonia 25·66, water 10·65—*Ibid.*

### XI.—*New method of reducing Litharge, employed at Freiberg.*

M. HARLE describes this method as follows :—the furnace is made to pass from a cupellation furnace into a small furnace, filled with charcoal, where combustion is kept up by a natural draught. The reduced lead runs into a vessel, adapted for its reception, and may be cast in moulds. In the silver works of *Barnaul*, in Kolywan, Siberia, where this method was first devised (*Karsten's Arch. Metall.* v. 1832.) the reducing furnace is 3 feet high, 16 inches broad, 30 inches long above, and 20 below. The opening, which allows the lead to escape, measures 4 inches by 6. There are besides in the anterior part of it three apertures, 2 inches in diameter for the admission of the draught. Several experiments were made at Freiberg, in 1833, upon this process, and the results were so satisfactory that the method was adopted. The small furnace was built of brick, and the cupellation furnace was placed behind it, so as to form a back to it. To the height of 2 feet, the bricks were cemented, but above this they were simply placed one above the other. When the litharge is pure, it is allowed to run upon the charcoal where it is immediately reduced. The only precaution required, is to see that the litharge falls on the centre of the charcoal. The proportions of the reduced metal to the charcoal burned is 1 quintal (109.81 lbs.) to 2 cubic feet. The expense of the new process is less than a half of the old method.—(*Ann. des Mines*, vi. 189.)

### XII.—*Absorption of Oxygen, by Platinum and Iridium.*

ACCORDING to Döbereiner these two metals may be obtained in a state of extreme division, by mixing their solution in sulphuric acid with certain organic substances, and then exposing the mixture to the action of light. He has found that when these metals are exposed to the air, for the purpose of drying them, the metallic matter absorbs from 200 to 250 times its volume of oxygen, without combining with it chemically, and that the latter is condensed with a force equivalent to a pressure of 800 or 1000 atmospheres. This strong mechanical capacity of these metals for oxygen, is an anomaly in chemistry.—(*Poggen. Ann.* B. xxxi.)

### XIII.—*Compounds of Bromine and Oxygen.*

It is well known that it is difficult to produce a combination between bromine and oxygen. Balard of Montpellier, states that this difficulty is overcome by the action of chloride of bromine upon the alkalies, or of bromine alone upon these bodies. Bromine as well as chlorine is capable of acting differently upon the metallic oxides. There are some upon which it has no action, as the peroxides and most of the metals; with the protoxides, again, it acts by decomposing a part of the body, transforming it into a perbromide, or by separating hydrogen from the water and converting it into perhypobromate. From peroxide of barium, it disengages the additional atom of oxygen, and converts it into the pure alkali. The alkaline oxides,

some earthy oxides, the oxides of copper, mercury and silver are decomposed and by it transformed into bromides, and hypobromites or the bromine is converted into hypobromous acid. The hypobromites are readily converted into bromates and bromides. Thus it appears, that the action of chlorine and bromine upon the metallic oxides is analogous.—*L'Institut, December, 1834.*

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XIV.—*Triple Compounds of Osmium, Iridium, and Chloride of Platinum, &c.*

1. *Triple Salt of Osmium and Chloride of Iridium, with Chloride of Potassium.*—When the natural mixture of osmium and iridium, as it occurs in the Uralian sand, is mixed with chloride of potassium, and heated with chlorine, a combination is found which crystallizes in octahedrons, consisting of iridium 26·6, osmium 13·4, chlorine and chloride of potassium 60·0.

2. *Triple Salt of Iridium and Chloride of Platinum, with Muriate of Ammonia.*—This salt is procured in the manufactory of St. Petersburg, by evaporating the solution from which the platinum has been separated by muriate of ammonia. It consists of iridium 31·76, platinum 10·59, palladium 1·25, chlorine and muriate of ammonia 56·40.

3. *Triple Salt of Iridium and Chloride of Platinum, with Chloride of Potassium.*—When a mixture of sesqui-oxide of iridium and platinum-chloride of potassium is digested with muriatic acid, a smell of chlorine is emitted, and a salt crystallizing in octahedrons is produced on evaporation. It consists of iridium 8·, platinum 32·, chlorine and chloride of potassium 60·.—*Ibid.*

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XV.—*Carbonate of Iron of Traversella.*

THIS mineral is accompanied with quartz and pyrites. S. Sismonda endeavours to explain the mode in which the carbonate loses its acid from the presence of the pyrites. The latter decomposes and forms a sulphate of iron with excess of acid. This excess acts upon the carbonate. Neutral sulphate of iron is the result, which is immediately decomposed by the lime, which exists in an isomorphous state, as it is called. Now, according to the atomic weights of these bases, the lime which in the carbonate re-places part of the iron, is not sufficient to decompose the whole of the sulphate of iron. It is possible, that this excess of sulphate of iron is dissolved by water, or decomposed by earths which come in contact with it, but it is more probable, that the proto-sulphate of iron, decomposes the water to become peroxidized, and that the hydrogen disengaged abstracts a portion of oxygen from the sulphuric acid which then escapes in the form of sulphurous acid.

He ascertained that the lime exists in the mineral in the state of sulphate, according to his theory. He submitted the carbonate of iron with its natural mixture of pyrites to a gentle galvanic current. after four hours, the carbonate of iron in contact with the pyrites,

and the pyrites itself, were changed into peroxides, which, when examined, were found to contain sulphate of lime, proving that sulphuric acid was formed during the action.

The crystals of Traversalla are almost all rhomboidal. The colour is grayish passing into brown. The mineral occurs in veins in the mountains. To this form of crystals, Haüy applied the terms *epigenous* or false crystals.—*L'Institut*, 80.

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#### XVI.—Method of detecting minute quantities of Sulphur.

A SMALL bit of paper, coloured as if by sulphur having been wrapped up in it, was submitted to M. Boutigny for examination. He scraped off the yellow matter carefully, and triturating it in an agate mortar with two grains of very pure nitrate of potash, projected it into an ignited porcelain capsule. Deflagration took place. After cooling, the saline matter was dissolved in distilled water. The solution was poured into a tube. A drop of a solution of chlorine of barium was added, but no precipitation was produced indicating the absence of sulphuric acid. To determine if this was a correct method of proceeding, he repeated the experiment with  $\frac{1}{20}$ th of a grain of sulphur, and he obtained a precipitate of sulphate of barytes insoluble in nitric acid. He collected it, mixed it with soda, and submitted it to the action of the reducing flame upon charcoal. The product of this operation was placed upon a moistened plate of silver. Sulphuretted hydrogen was disengaged in a quantity sufficient to be appreciable by the smell and to leave a black stain on the silver.—*Journ. de Chim. Med.*, i. 6.

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#### XVII.—Tin discovered in Brittany.

ON the road from Rennes to Vannes, near the town of Roc St. André, there are remains of excavations in a vein of white quartz. Tradition states these to have been made for the use of a glass house, which at one time existed here. M. Bellevue lately observed a brown substance in this quartz, and sent specimens of it to M. Guillon, at Blavier and Lorieux. The latter found that the mineral was oxide of tin. The purest specimens afforded 73 to 78 per cent. magnetic tin with the  $\frac{1}{100}$ th of its weight of metallic iron. The brown specimens in large crystals contain 19 per cent. of per-oxide of iron which may be separated by muriatic acid.

The quartz vein is about 30 feet wide, running in the direction of N. 8° W., and possessing an inclination of 25° E. It passes through granite which is fine grained, and contains much yellowish white or reddish felspar. Greywacké occurs at no great distance from the vein, along the canal from Nantes to Brest. The oxide of tin, which occurs generally in well formed crystals, is only to be seen to a very small extent. It is generally accompanied with mica, and frequently also with *arsenical iron*, *hematite*, and *emerald*. At a short distance from the tin vein, to the E. and S. E., ferruginous sand-stone, rolled quartz, yellow flints, with sand and clay appear.—*Ann. des Mines*, vi. 381.

XVIII.—*Employment of Iron in Suspension Bridges.*

By M. E. MARTIN.

M. MARTIN has concluded from his observations that the employment of bars of iron in suspension bridges is more proper than that of wire cables, as being more solid, more durable, and more economical. The experiments of the French marine prove that in relation to resistance to fracture by extension, there are two distinct classes of iron, soft and hard iron. The soft is the only one proper for the manufacture of ship cables. In consequence of its softness it can suffer an elongation of one-fifth of its length before breaking. This quality of iron answers well for suspension bridges. The security of cables would appear to depend upon their property of not breaking until they have suffered great elongation. Hemp cables, from experiments made at the hydraulic press of the forge of Guerigny, undergo a much less elongation before breaking than chain cables. It appears that iron chain cables are cheaper than wire cables for suspension bridges. The greatest objection against the employment of iron is its tendency to oxidation; but this is remedied, in a great measure, by the mode of suspension, so as that the chains may be placed in a condition to last, in proportion to the other parts. The Langon, on the Garonne, is referred to by Martin as an example of iron lasting well.—*Ann. des Mines*, v.

XIX.—*Oil of Canella.*

THERE are two kinds of this oil, one of which comes to us from China the other from Ceylon; both are obtained by distilling the bark of canella. The root of canella contains camphor. The Chinese oil possesses a reddish yellow brown colour, and a disagreeable smell. The Ceylon oil has a sweet smell, and is much more valuable, in a commercial point of view, than the other. Dumas and Peligot prepared a pure oil by distilling some good Chinese canella bark after digestion for 12 hours in water saturated with salt. A milky liquid was procured, from which an oil separated. This, when digested with muriate of soda, may be considered as pure. The water, from which the oil is separated in the course of a few hours, contains needle lamellar crystals.

The oil of canella strongly resembles common camphor, by the action which concentrated nitric acid has upon it, forming a true salt. Muriatic acid gas also combines with it. It combines completely with ammonia, producing a crystalline product.

Oxygen gas is rapidly absorbed by the oil, giving origin to a new acid, termed *cinnamic acid*. When the oil is submitted to the action of hot nitric acid, a smell of bitter almonds is disengaged, and benzoic acid formed. When the oil is boiled with a solution of chloride of calcium much benzoic acid or benzoate of lime is formed. Liquid potash has no effect on the oil, but the hydrate, when heated with it, disengages pure hydrogen, and cinnamate of the oil is formed.

Chlorine at first forms a substance resembling *chloride of benzoyl*, which is gradually transformed into a substance like *chloral*.

Dumas and Peligot term the oil, hydrate of cinnamyle from its analogy to the oil of bitter almonds. The composition is,

Carbon . . . .	81.6
Hydrogen . . . .	6.2
Oxygen . . . .	12.2
	<hr/>
	100.0

*Cinnamic acid* crystallizes in large yellow prisms which dissolve in boiling water, but are almost insoluble in cold water.

It is formed of,

Carbon . . . .	78.0
Hydrogen . . . .	4.9
Oxygen . . . .	17.1
	<hr/>
	100.0

The atomic weight was determined by precipitating a solution of the neutral cinnamate of ammonia by nitrate of silver. The results were,

Cinnamic acid . . . .	54.8
Oxide of silver . . . .	45.2
	<hr/>
	100.0

By this analysis we may infer that the atomic weight of the acid is 17.75 (17.65 according to Dumas). The authors consider that the oil of canella loses 2 atoms of hydrogen and gains an atom of oxygen in order to form an hydrous cinnamic acid. Pure cinnamic acid is colourless. It melts at 120°, (248° F.,) boils at 293°, (559° F.,) the barometer being at 29.75 inches. It distils over without leaving any residue. Its vapour excites coughing. Alcohol dissolves it, from which solution it is precipitated by water. Its salts are very similar to the benzoates.

*Chlorocinnose.* The authors give this name to the compound formed by the action of chlorine upon the oil. When dissolved in boiling alcohol, in cooling fine white needles are formed, upon which sulphuric acid and ammonia have no effect. It consists of,

Carbon . . . .	40.5
Hydrogen . . . .	1.5
Chlorine . . . .	52.1
Oxygen . . . .	5.9
	<hr/>
	100.0

The exact nature of this compound they have been unable to make out.

*Nitrate of oil of canella* consists of oblique prisms with a rhomboidal base, which are decomposed by water and which acquires the smell of bitter almonds.

*Hydrochlorate of oil of canella* is formed by exposing the oil to muriatic gas. It consists of,

Oil . . . .	78.8
Muriatic acid . . . .	21.2
	<hr/>
	100.0

*Ammoniuret of oil of Canella* appears in the form of a dry

powder, when the oil is exposed to the action of ammoniacal gas ; consisting of,

Oil	89.0
Ammonia	11.0
	100.0

XIX.—Table exhibiting the comparative weight of Sea Water at different places in Holland.

THE following table is extracted from a work in the Dutch language upon Acetic Acid, entitled "*Handleiding tot het vinden der ware sterkte van het Acidum Aceticum door middel van de digtheid door A. Van der Toorn.*" *Gravenshage*, 1824, 4to, which the author's kindness has enabled me to consult. The first column contains the names of the localities from which the water was procured, and the other three exhibit the weight in grammes of a cubic decimetre (= 61.028 English cubic inches, the gramme = 15.38 grains) of sea water, at different periods in 1820, at the temperature of 39° Fah., pure water being reckoned 1000.0.

Places from which the water was taken.	1 August,	21 Sept.	1 Nov.	
Zeeland, . . .	Breskens, . . .	1025.1	1025.7	1025.9
	Scharrendijke,	1022.9	1024.2	1024.9
South Holland,	Ter Heide, . . .	1021.8	1019.1	1025.2
	Katwijk, . . .	1022.6	1019.4	1024.0
North Holland,	Kalantsoog, . . .	1023.8	1022.2	1022.8
	Zandvoort, . . .	1022.0	1021.5	1019.6
	Huisduinen, . . .	1024.5	1022.6	1023.4
Vriesland, . . .	Wierum, . . .	1027.6	1025.4	1022.3
	Makkum, . . .	1021.1	1018.6	1016.7
Gelderland, . . .	Molquerum, . . .	1018.2	1013.3	1015.3
	Harderwijk, . . .	1008.3	1007.3	1010.2
North Holland,	Huizen, . . .	1011.7	1011.4	1012.4
	Enkhuizen, . . .	1014.0	1013.6	1015.9

XX.—Essence of Turpentine.

DUMAS and Peligot infer from their experiments, that the essences of turpentine, basil (*Ocymum basilicum*), and of cardamoms (*Cardamomum minus*) are identical in their composition, which is represented by,

Carbon, . . . . .	63.6
Hydrogen, . . . . .	11.4
Oxygen, . . . . .	25.0
	100.0

XXI.—NEW BOOKS.

LEHRBUCH der Chemie und Pharmakologie für Aerzte und Thierärzte. Von C. G. H. Erdmann. 8vo. 1836.—*Berlin*, 1st Band.

# Meteorological Journal, -- For July.

Kept at the Manse of the Parish of Abbey St. Bathans, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea.  
By the Rev. JOHN WALLACE.

DATE.	THERMOMETER.				HYGROMETER (Leslie's.)				BAROMETRICAL (at 32° of Fahrenheit.)				Rain in Inches Weekly.	Direction of Wind at A.M.	REMARKS.
	A.M.	X.	P.M.	X.	A.M.	U.	P.M.	X.	A.M.	X.	P.M.	X.			
1833.															
Fr.	57	66	64	58	0	3	12	0	29.485	29.494	29.482	29.481		S. W.	Gentle breeze, slight rain in the morning, soft clouds floating all day.
Sa.	61	54	63	54	35	35	38	4	29.460	29.448	29.401	29.340		W.	Gentle breeze, cloudy, occasional tendency to rain, in the evening rain.
Su.	58	58	59	53	17	17	16	5	29.480	29.493	29.441	29.618		W. by S.	Gentle breeze, cloudy, some slight showers, evening clear.
Mo.	60	64	69	65	23	22	34	11	29.632	29.637	29.642	29.616		S. W.	Calm, soft white clouds floating over a blue sky, evening nearly cloudless.
Tu.	62	68	66	62	7	18	7	0	29.518	29.544	29.509	29.448		W.	Very calm, A.M. lowering with sultry heat, P.M. and evening a thunder storm.
We.	61	60	63	50	18	15	40	10	29.478	29.489	29.503	29.528		W.	Gentle breeze, A.M. light rain, P.M. gradually clearing, evg. clear, deposition.
Th.	58	58	57	51	27	22	13	3	29.581	29.593	29.557	29.603		W.	Calm, overcast, with some rain.
Fr.	54	57	59	52	18	22	20	11	29.713	29.730	29.692	29.638		N. E.	Calm, A.M. cirri with heavy clouds, P.M. overcast, evg. cirros. & cirrocumulus.
Sa.	57	58	64	57	6	12	9	7	29.520	29.516	29.404	29.389		SW by W.	A.M. calm, cloudy and tending to rain, P.M. showery, evg. windy, cirrostratus.
Su.	61	62	65	60	20	20	22	11	29.358	29.354	29.411	29.394		W. N. W.	Strong wind, cumuli prevalent, evening calm and cloudy.
Mo.	63	62	64	53	19	16	21	3	29.383	29.381	29.275	29.173		S. W. W.	Gentle wind, cloudy and tending to rain, evening calm.
Tu.	53	56	54	50	25	32	16	9	29.021	29.029	29.061	29.095		W. N. W.	Strong wind, clouds floating below an upper stratum of cirrostra, sky troubled.
We.	54	53	58	51	20	11	12	13	29.147	29.153	29.123	29.070		S. W.	Strong wind, heavy masses of cloud prevalent, evening calm.
Th.	55	55	58	49	23	23	37	3	29.102	29.117	29.152	29.240		W. by S.	A.M. strong wind, cumuli prevalent, evening calm and overcast.
Fr.	55	56	56	45	6	10	16	0	29.059	29.049	28.937	28.932		S. E.	Calm, cloudy or overcast, P.M. thunder, evening clear, copious deposition.
Sa.	55	56	57	50	20	24	36	0	28.976	28.992	28.995	28.974		W. by N.	Calm, cirri and cirrostratus prevalent, in the evening clear.
Su.	55	56	57	46	32	35	35	15	28.992	29.000	29.045	29.233		W. N. W.	Very strong wind, sky nearly cloudless, in the evening wind abated.
Mo.	53	54	55	47	34	33	24	14	29.216	29.223	29.207	29.294		W. N. W.	Very strong wind, P.M. sky of an unsettled aspect, cymai.
Tu.	44	45	52	45	1	3	10	8	28.875	28.966	28.976	29.007		NW by N	Gentle wind, A.M. rain, P.M. sky breaking up, 3 P.M. hail shower, evg. clear.
We.	53	54	55	47	36	32	34	8	28.891	28.881	28.834	28.838		W. N. W.	Gentle wind, cumuli floating on a blue sky, evening calm and cloudy.
Th.	53	54	54	48	15	18	20	0	28.957	28.980	29.037	29.131		W. N. W.	Calm, A.M. cumuli prevalent, P.M. frequent showers.
Fr.	51	53	57	49	19	17	39	3	29.243	29.290	29.294	29.403		W. N. W.	Calm, A.M. cumuli floating on blue sky, P.M. gradually overcast, light showers.
Sa.	53	49	49	49	17	20	20	7	29.494	29.500	29.446	29.397		W. N. W.	Very calm, cloudy or overcast, with tendency to rain.
Su.	49	49	49	48	0	0	0	0	29.115	29.088	29.081	29.158		E.	Gentle wind, rain with fog on the hills, wind rising in the evening.
Mo.	50	50	52	48	0	0	9	0	29.363	29.354	29.424	29.448		N. E.	Calm, A.M. foggy, P.M. clearing, cirrostratus and cirrocumulus prevalent.
Tu.	54	55	65	54	0	0	23	7	29.347	29.342	29.325	29.381		S. W.	Gentle wind, A.M. rain, P.M. clearing, cirrostra, and cirrocum., evg. cloudy.
We.	58	59	61	56	21	23	11	0	29.347	29.341	29.317	29.268		S. W.	A.M. gentle breeze, sky overspread, P.M. wind rising, rain, evg. fair but cloudy.
Th.	58	58	61	59	4	0	0	0	29.264	29.264	29.335	29.178		NW by N	Brisk wind, sky overcast, soft clouds floating below, showery, evening rain.
Fr.	51	51	48	46	0	0	0	0	28.886	28.884	28.879	28.923		NW by N	A.M. brisk wind, fog and rain, P.M. wind boisterous with very heavy rain.
Sa.	49	51	56	48	7	11	34	13	29.231	29.227	29.369	29.603		NW by N	A.M. brisk wind, sky covered with heavy clouds, P.M. clearing, evg. cirrostr.
Su.	53	54	56	48	26	31	27	0	29.752	29.732	29.725	29.592		NW by N	Calm, overcast, occasionally lowering, in the evening rain.
Means.	55.5	56.5	58.7	51.4	16	17	21	5	29.286	29.294	29.286	29.307			

Means { Therm. 54° } Mean temperature of spring water 46° ½  
 for the { Hygr. 11° } Mean point of deposition 49.9  
 Month { Bar. 29.3 in. } Moisture in a cubic inch of air = .00239 grs.

# RECORDS

OF

## GENERAL SCIENCE.

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### ARTICLE I.

*Biography of M. Le Comte Lagrange.\** By M. DELAMBRE.

JOSEPH LOUIS LAGRANGE, one of the founders of the Academy of Turin, Director during twenty years of the Academy of Berlin, for the physico-mathematical sciences, Foreign Associate of the Academy of sciences of Paris, member of the Institute of France and of the board of longitude, Senator and Count of the Empire, Grand Officer of the Legion of Honor and Grand Cross of the imperial order of the re-union, was born at Turin on the 25th of January, 1736. His father was Joseph Louis Lagrange, Treasurer of War; his mother, Maria Theresa Gros, only daughter of a wealthy physician of Cambiano.

His great-grandfather, captain of cavalry in the service of France, had gone over to that of Emmanuel II., King of Sardinia. Through the latter he was fixed at Turin, by marriage with a lady *Conti*, of an illustrious Roman family: he was of Parisian extraction, and relative of one Maria Louisa, tire-woman of the mother of Louis XIV., and afterwards wife of François Gaston de Bethune.†

These details are of no importance to the illustrious Geometer, whose renown dispenses with shewing forth a genealogy, but not so to France. She is eager to recal him, and re-establish him under her ancient sovereignty. His own name, and that of his mother also, attest a French origin; all his works are written in French; the city

\* From Silliman's American Journal, xxx. 64.

† Eulogy of Lagrange by Cassali. Padua, 1813.

which saw his birth too had become French. France then, has incontestably the right of being proud of one of the greatest men who has honoured the sciences.

His father was wealthy, had made an advantageous marriage, but was ruined by hazardous enterprises. Let us not hence pity M. Lagrange. He himself received this misfortune as the first cause of all what afterwards befel him most happily. *S'il avait eu de la fortune*, said he himself, *il n'eût probablement pas fait son état des mathématiques*. And in another career, what advantages could he have found, that had entered into comparison with those of a calm and studious life, with that brilliant train of success, uncontested in a department reputed eminently difficult, and with that personal esteem, which he saw increase till his last moment.

A taste for mathematics, however, was not that which he first manifested. He had a strong passion for Cicero and Virgil before being able to read Archimedes and Newton. Soon he became an admirer no less passionate of the geometry of the ancients, which he at first preferred to the modern analysis. A memoir which the celebrated Halley had long before composed, expressly to show the superiority of analysis, had the glory of converting M. Lagrange, and revealed to him his true destination.

He then gave himself up to this new study with the same success which he had obtained in synthesis, and which had been so marked, that at the age of sixteen\* years he was professor of mathematics in the royal school of artillery. The extreme youth of a professor is for him but a greater advantage, when he has shown extraordinary talents and at the same time his élèves are not children. All those of Lagrange were older than himself and were not thence less attentive to his *lessons*. He selected some of them whom he made his friends.

From this association sprang the Academy of Turin, which published in 1759 a first volume, under the title of *Actes de la société privée*. We therein see Lagrange directing the physical researches of Doctor Cigna, and the works of the *Marquis de Saluces*. He furnished to Foncenex the analytical part of his memoirs, at the same time leaving to

\* Others say fifteen or nineteen.

him the care of developing the arguments on which his formulas rested. In effect, we notice already in these memoirs this pure analytical step which afterwards characterized the great productions of Lagrange. He had found a new theory of the lever. It constituted the third part of a memoir that had much success. Foncenex in return, was put at the head of the navy which the king of Sardinia was then forming. The two first parts seem of the same style and from the same hand. Are they alike from Lagrange? He has not positively claimed them. What, however, can direct our conjectures to the real author, is, that Foncenex soon ceased to enrich the collections of the new Academy, and that Montucla, ignorant of what has been revealed to us by Lagrange at his last moments, is astonished that Foncenex, after being so favourably announced, broke off researches that could have obtained for him a great name.

Lagrange abandoning to his friend isolated solutions, published at the same time under his own name some theories which he promised to follow out and develop. Thus, after having given new methods for *maxima* and *minima* of every kind, after having shewn the insufficiency of the known formulas, he announced that he would treat this subject, which otherwise appeared to him interesting, in a work which he was preparing, and in which, too, are seen deduced from the same principles all the mechanics of bodies, whether solids, or fluids. Thus, at twenty three years he had already laid the foundation of great works, which have since excited the wonder of philosophers.

In the same volume, he brings back to the differential calculus, the theory of recurring series, and the doctrine of chances, which, hitherto, had been treated only by indirect methods, and which he establishes upon the most natural and the most general principles.

Newton had undertaken to submit to the calculus the motions of fluids: he had made researches on the propagation of sound. His principles were insufficient and even defective; and his suppositions inconsistent with themselves: Lagrange demonstrated them to be so. Lagrange founded his new researches on the known laws of dynamics; by considering in the air only the particles found in a straight

line, he reduced this problem to that of vibrating cords, about which the greatest geometers were divided; he showed that their calculations were insufficient to decide the question; he undertook a general solution by an analysis new as it was interesting, since it permits of resolving at once an indefinite number of equations, and since it extends even to discontinued functions: he established more firmly the theory of the mixture of the simple and regular vibrations of D. Bernouilli: he shows the limits between which this theory is exact, and beyond which it is defective; *then he* arrives at the construction given by Euler, a true construction, although the author had arrived at it only by calculations which were not sufficiently rigorous: he answers objections raised by D'Alembert; he demonstrates that whatever figure we give to the cord, the duration of oscillations will be always the same, a truth of which for experiment D'Alembert had judged the demonstration very difficult or even impossible; he passes to the propagation of sound; treats of simple and compound echoes, of the mixture of sounds, of the possibility that they spread in the same space without disturbing one another, and demonstrates rigorously the generation of harmonical sounds; he announces lastly, that his object is to destroy the prejudices of those who still doubt if mathematics could ever shed true light on physics.

Euler felt the worth of the new method, and selected it for the object of his profound meditations. D'Alembert did not coincide. In his private letters, as in his printed memoirs, he proposed numerous objections, to which Lagrange has since answered, but which can at least leave this doubt; . . . How, in a science to which we grant universally the merit of exactness, can it be that men of the first order are divided among themselves, and for a long time dispute?

The first answer of Euler was to cause Lagrange to be associated with the Academy of Berlin. Upon announcing to him this nomination, on the 2nd of Oct., 1759, he said to him: "*votre solution du problème des isopérimètres ne laisse rien à désirer et je me réjouis que ce sujet, dont je m'étais presque seul occupé depuis les premières tentatives, ait été porté par vous au plus haut degré de perfection. L'importance de*

*la matière m'a excité à en tracer à l'aide de vos lumières, une solution analytique à laquelle je ne donnerai aucune publicité jusqu'à ce que vous-même ayez publié la suite de vos recherches pour ne vous enlever aucune partie de la gloire qui vous est due.*

If these delicate proceedings, and the testimonies of the highest esteem should flatter a young man who was not twenty four years old, they do no less honour to a great man, who, holding then the sceptre of mathematics, knew how to receive in this manner the work which pointed out to him his successor.

But these eulogies are contained in a letter: hence we might think that the great and good Euler may have suffered himself to go on in some of the exaggeration permitted in the epistolary style; let us see then how he afterwards expressed himself in the dissertation which his letter announced. Here is the beginning:

“After I had long and vainly fatigued myself in seeking for this integral, (postquam diu et multum desudassem..... nequicquam inquisivissem) what was my astonishment (penitus obstupui) when I learned that in the Memoirs of Turin, this problem is found resolved with as much ease as excellence. This fine discovery caused me the more admiration as it is the more different from the methods which I have given, and as it surpasses them considerably in simplicity.” It is thus that Euler begins the memoirs, in which he explains with his usual clearness, the reasons of the method of his young rival, and the theory of this new calculus, which he has called the *calculus of variations*.

To render more sensible all the different motives which gave birth to the admiration that Euler showed with such noble candour, it will not be useless to recur to the origin of the different researches of Lagrange, such as he gave it himself two days before his death.

The first attempt to determine the maximum and minimum in all indefinite integral formulas, had been made on account of the curve of the swiftest descent, and the isoperimeters of Bernouilli. Euler had reduced them to a general method, in an original work, which exhibits throughout a deep knowledge of the calculus; but, however ingenious his method was, it had not all the simplicity which we can desire in a work of pure analysis. The author himself

came to the same conclusion ; he perceived the necessity of a demonstration independent of geometry and of analysis.

In an appendix to the volume having for its title *du Mouvement des projectiles dans un milieu non resistant*, he seemed wholly to distrust the resources of analysis, and finishes by saying *Si mon principe* (it is that which Lagrange has since named the principle of the last action) *n'est pas suffisamment démontré, comme cependant il est conformé à la vérité, je ne doute pas qu'au moyen des principes d'une saine métaphysique on ne puisse lui donner la plus grande évidence, et j'en laisse le soin à ceux qui font leur état de la métaphysique.*

This appeal, to which metaphysicians did not answer, was understood by Lagrange who excited their jealousy.

In a short time the young man found the solution of which Euler had despaired. He found it by analysis ; and in giving an account of the way in which he had been led to this discovery, he said positively, to answer the doubts of Euler, that he viewed it, not as a metaphysical principle, but as a necessary result of the laws of mechanics, as a simple corollary of a more general law, which he afterwards made the base of his *Mecanique Analytique*. (See this work, page 246 of the second edition, or 189 of the first.)

This noble spirit that excited him to triumph over difficulties regarded as insurmountable, and to rectify or complete theories still imperfect, appeared to have constantly directed Lagrange in the choice of his subject.

D'Alembert had thought it impossible to submit to the calculus the motions of a fluid contained in a vessel, if this vessel had not a certain figure. Lagrange demonstrated on the contrary, that there would be no difficulty except in the case when the fluid is divided into many portions. Yet then we can determine the places where the fluid ought to be divided into many portions, of which we can determine the motions as if they were isolated.

D'Alembert had thought, that in a fluid mass such as the earth might have been originally, it was not necessary that the different layers should be on a level : Lagrange shews that the equations of D'Alembert were themselves only those of strata on a level.

In opposing D'Alembert with all the respect due to a

geometer, of that order, he often employed very fine theorems which he owed to his opponent; D'Alembert, on his side, added to the researches of Lagrange. "Your problem appeared to me so fine," wrote he to him, "that I have sought another solution for it; I have found a more simple method of arriving at your elegant formula." These examples, which it would be easy to multiply, prove with what courtesy these celebrated rivals corresponded. Vying with each other incessantly, conquered as well as conquerors, they found at every moment, in their discussions themselves, reasons to esteem one another the more, and each supplied for his antagonist opportunities that were to lead him to new triumphs.

The Academy of sciences of Paris, had proposed for the subject of one of its prizes, the theory of the libration of the moon; that is to say, it asked the cause why the moon, in turning around the earth, always shows the same face, with the exception of some variations observed by astronomers, and of which Cassini I. had well explained the mechanism. The point was, to find the means of calculating the phenomena, and of deducing them analytically from the principle of universal gravitation. Such a chance was an appeal to the genius of Lagrange; one, which was held out to him of applying his principles and his analytical discoveries. The hope of D'Alembert was not blasted. The piece of Lagrange is one of his highest titles of glory. Therein are seen the first developments of his ideas and the germ of the *Mecanique Analytique*. D'Alembert wrote to him; *j'ai lu avec autant de plaisir que de FRUIT votre belle pièce sur la libration, si digne du prix qu'elle a remportée.*

About this time he turned his attention to the theorems of Fermat, on the properties of numbers. Many geometers, undoubtedly, practised upon the theorems of Fermat, but not one ever succeeded. Euler alone had made some progress in this difficult path, wherein have since distinguished themselves M. Legendre and M. Gauss. Lagrange, upon demonstrating or correcting some attentive glimpses of Euler, resolved a problem which appeared to be the *knot* of all the rest, and from which he derived a useful result, that is to say, the complete resolution of equations of the second degree, with two unknown quantities which

must be entire numbers. The memoir, printed like the preceding among those of the Academy of Turin, is nevertheless dated at Berlin, the 20th September, 1768. This date points out to us one of the events, (few indeed,) which show that the life of Lagrange is not all in his works.

His residence at Turin pleased him little. He saw there no one who cultivated mathematics with success: he was impatient to see the philosophers of Paris with whom he corresponded. M. de Caraccioli, with whom he lived in the greatest intimacy, had just been nominated to the embassy of England, and was to pass through Paris where he purposed making a short stay. He proposed this journey to Lagrange. Lagrange consented to it with joy, and as might have been expected, was welcomed by D'Alembert, Clairaut, Condorcet, Fontaine, Nollet, Marie, and other philosophers. Having fallen dangerously sick in the course of a dinner, when Nollet had served to him only dishes prepared *a l'Italienne*, he could not follow his friend, M. Caraccioli, to London, who suddenly received an order to repair to his post, and was obliged to leave him in a furnished hotel, to the care of a confidential person, directed to supply all his wants.

This event changed his purpose. He dreamed of nothing but of returning to Turin. He gave himself up to mathematics with a new ardour, when he learned that the academy of Berlin was threatened with the loss of Euler, who was intending to return to St. Petersburg. D'Alembert spoke of this intention of Euler in a letter to Voltaire, the 3d of March, 1766; *j'en serais fâché*, added he, *c'est un homme peu amusant, mais un tres-grand géomètre*. It was of little consequence to D'Alembert that the *homme peu amusant* should remove seven degrees from Paris towards the pole. He could read the works of the *great geometer* in the Transactions of the Academy at St. Petersburg, as well as in those of the one at Berlin. What troubled D'Alembert was, the fear of seeing himself called upon to re-place him; and the embarrassment of replying to offers which he was well resolved not to accept. Frederic, in fact, proposed anew to D'Alembert the place of president of his academy, which he held for him in reserve after the death of Maupertuis. D'Alembert suggested to him the idea of

placing Lagrange in the place of Euler; and if we believe the secret history of the court of Berlin (tom II., p. 474), *Euler had already pointed out Lagrange as the only man capable of following in his track.* And, indeed, it was natural that Euler, who wished to obtain leave to quit Berlin, and D'Alembert who sought a pretext for not going thither, should both, without corresponding with each other, have cast their eyes upon the man most fit to sustain the glory which the labours of Euler had shed upon the Academy of Prussia.

M. Lagrange was engaged with the title of director of the Academy in the physico-mathematical department. We cannot fail to be astonished that Euler and Lagrange, placed successively in the place of Maupertius, should have received but half of the salary which the king wished to give, apart from every thing to D'Alembert. The reason is, that this prince, who, in his leisure, cultivated poetry and the fine arts, had no idea of the sciences, which he thought himself obliged, notwithstanding, to protect as a king: the reason is, that in reality he *placed* little value upon geometry, against which he sent three pages of verse to D'Alembert himself. D'Alembert delayed answering him until the end of the siege of Schweidnitz, because *ce serait trop d'avoir a-la-fois l'Autriche et la géométrie sur les bras*; and, notwithstanding the immense reputation of Euler, we see by the correspondence with Voltaire, that Frederic designated him only by the qualification of his *géomètre borgne, dont les oreilles ne sont pas faites pour sentir les délicatesses de la poésie*: to which Voltaire added; *nous sommes un petit nombre d'adeptes qui nous y connaissons, le rest est profane*: a remark more witty than fair, and which Euler, in speaking of geometry, might have been able to retort against Voltaire and Frederic. We see plainly that Voltaire who had so *worthily lauded Newton*, sought in this expression to flatter Frederic. He entered out of courtesy into the ideas of a prince. For Frederic wished to put at the head of his Academy an individual only, who had at least some reputation in literature, under the fear that a geometer would not take sufficient interest in the direction of literary works; and at the same time, that a man of letters would not be more out of place at the head of a society, composed

in part of persons whose language he did not understand. He was then right in dividing the office in order that it might be completely filled.

We must not be expected to follow Lagrange step by step, in the learned researches with which he has filled the Memoirs of Berlin, after his arrival in that city, on the 6th November, 1766, and even some volumes of the Academy of Turin, that owed to him, in all respects, its existence. But we cannot omit pointing out, at least in a few words, the most remarkable which they contain. He wrote the following Memoirs in the Transactions of the Berlin Academy:—

1. A great memoir wherein are found the demonstration of a curious proposition that Euler could not demonstrate, a new extension given to this theorem and direct proofs of many other propositions, to which Euler had arrived only by way of induction, and in which, after having enriched the analysis of Diophantus and Fermat, the author passes to the theory of equations, with partial differences explains a striking paradox noticed by Euler, makes known an entire class of equations of which there were only some isolated examples, and puts out of sight the paradox by showing to what belong, both the complete integral of these equations, and the *singular solution* which is not comprised in this integral.

2. A formula for the return of series, remarkable by its generality and the simplicity of the law, of which he makes a happy application to the problem of Kepler, and thence succeeds in rendering sensible the convergency of the analytical expression of the equation of the center, a convergency which we had always supposed, without being able to demonstrate.

3. An important memoir on the solution of numerical equations, containing also new remarks on that of algebraical equations. This work served as the basis of a treatise which he afterwards published, under the same title, and of which he gave two editions.

4. Another memoir, no less important, and still more original, where he reduces to operations of pure algebra, every process of the differential and integral calculus, which he separates from every idea of infinitely small, of fluxions,

of limits and of vanishing, and demonstrates the lawfulness of the abbreviations permitted in these two *calculs*, which he also frees from all difficulties, and from all paradoxes that had sprung up in an imperfect and suspected *meta-physique*.

5. The demonstration of a curious theorem on primal numbers; a demonstration that no one had been able to find, and the more difficult, as we know how to express algebraically propositions of this kind.

6. The integration of partial differences of the first order, by a fruitful principle, sufficient for the greater part of cases where this integration is possible.

7. A purely analytical solution of the problem of the rotation of the body of any figure, wherein he at last surmounts difficulties that had long stopped him, and by which geometers seemed to expect, with curiosity, some ulterior developments, that they hoped to find in the second volume of his new *Mechanique Analytique*.

In addition to these, he wrote many memoirs on the obscure and difficult theory of probabilities, wherein we admire the integral that forms its base, the number and importance of the problems it resolves; the application that the author makes of it to the question, recurring every day in astronomy; of the degree of confidence that can be allowed to the mean result of a great number of observations; and wherein is found this remarkable property, and so favourable to the circles of Borda, that each of the even numbers states as probable, by the odd number immediately above, that the error will be comprised within certain limits. M. le Comte Laplace had on his part laboured on the same theory. M. Lagrange resumed it, on his part, by means which extend to equations of all orders. Of these, they give finite integrals, and facilitate, in all cases, the determination of arbitrary functions.

He made then a similar attempt for the problem of eclipses; he found that the methods, somewhat prolix, of Dusejou, had neither the simplicity nor the facility that ought to have been expected from the actual state of analysis. He exhibits, in this work, all his resources and all his address.

(To be continued.)

## ARTICLE II.

*Experiments on the Combination of Sulphuric Acid and Water.*

By THOMAS THOMSON, M.D., F.R.S., L. & E., &c., Regius Professor of Chemistry in the University of Glasgow.

(Read before the Chemical Section of the British Association for the Advancement of Science, Aug. 25th, 1836.)

It is well known, that when sulphuric acid and water are mixed in any proportion whatever, a change takes place in the temperature, by the evolution of heat, while a chemical combination is formed between the acid and the water. The heat evolved in this case has been universally ascribed to a change in the specific heat of the compound, which, being less than the mean specific heat of the two constituents, requires less heat than before to maintain its temperature, and must in consequence appear hot.

I am not aware of any attempt to investigate this curious subject, except the experiments of M. Gadolin published in the Memoirs of the Stockholm Academy for 1784.\* But as Gadolin nowhere specifies the specific gravity of the acid which he employed, and as his experiments were made upon different weights of the acid and water, without any reference to the atomic proportions, it is impossible to deduce any legitimate conclusions from these experiments. This is to be regretted, as the experiments of Gadolin appear to have been carefully made, and to have been sufficiently varied. But as the atomic theory was quite unknown in 1784, it was impossible for Gadolin to select such proportions of acid and water as would throw the requisite light upon the combinations.

To obtain pure sulphuric acid of the requisite strength for my purpose, I distilled a quantity of Nordhausen acid, which had been long in my possession, and which had absorbed so much moisture from the atmosphere that its specific gravity was reduced to 1.8375.† Nordhausen acid,

\* *Rön och anmärkningar om Kroppars absoluta Varmé.* Af Johan Gadolin. Kong. Vetens. Acad. Handlingar, 1784, p. 218. An abstract of these experiments has been inserted in the Appendix to Crawford's Treatise on Animal Heat, p. 457.

† The author stated in the Section, in answer to a question from Dr. Dalton, that English sulphuric acid will not answer for these experiments, in consequence of its containing nitric acid. It contains also arsenious acid, *Records*, ii. 73; and muriatic acid, *Ibid.* iv. 152.—EDIT.

as is well known, is always opaque, and of a blackish colour. This is owing to the presence of some vegetable matter charred by the acid. For when the acid has been kept for some time boiling, it loses its dark colour, and becomes as colourless and transparent as the purest British acid. Suspecting at first that this dark colour might be owing to the presence of selenium or iodine, or even bromine, I examined it for each of these bodies, but unsuccessfully. The only foreign body whose presence was discovered, was the sulphate of lime; of which the acid I employed contained  $\frac{1}{5000}$ th part of its weight. It was suspended in the acid in white flocks, and greatly facilitated the distillation, by preventing the violent ebullition which is apt to take place when we attempt to distill concentrated sulphuric acid. I continued the distillation till the specific gravity of the acid which came over was as great as that of the acid in the retort. It was obviously needless to continue the process farther; because all farther concentration of the acid became impossible.\* The acid remaining in the retort had a specific gravity of 1.8422 at the temperature of 59°. It was obviously a compound of

1 atom acid, . . .	5
1 atom water, . . .	1.125
	6.125

and its atomic weight was 6.125. It was this acid that was employed in all the following experiments. The only impurity which I detected in it was (as above stated)  $\frac{1}{5000}$ th of its weight of sulphate of lime; a quantity so small, that it could not materially affect the results. It was impossible to get rid of this sulphate, while the acid retained its

\* In a first process, the acid put into the retort had a specific gravity of 1.8376, and was composed of

Real acid, . . .	77.85	or 1 atom
Water, . . .	22.15	1.264 atom
	100.00	

The acid that came over had a specific gravity of 1.6598 (or was very nearly a compound of 1 atom acid + 3 atoms water.) The specific gravity of the acid remaining in the retort was 1.8396 at 64½°.

strength. But after it had been diluted with a certain proportion of water, the sulphate of lime precipitated to the bottom, and the pure acid was drawn off. This happened, when the acid was so much diluted, as to be composed of

1 atom acid, . . .	5
5 atoms water, . . .	5.625
	10.625

### 1. *Specific Gravity of different Atomic Compounds of Sulphuric Acid and Water.*

As an atom of sulphuric acid of the specific gravity 1.8422 weighs 6.125, and as it is a compound of 1 atom acid and 1 atom water, it was easy, by mixing determinate weights of this acid and water, to obtain compounds of 1 atom acid with 2, 3, 4, 5, 6, 7, 8, 9 and 10 atoms water. The following table exhibits the specific gravities of all these strengths.

Acid.	Water.	Specific Gravity		Difference.
		By experiment.	By calculation.	
1 atom	+ 1 atom	1.8422		
	+ 2 "	1.7837	1.7114	+ 0.0723
	+ 3 "	1.6588	1.6158	+ 0.0430
	+ 4 "	1.5593	1.5429	+ 0.0164
	+ 5 "	1.4737	1.4854	- 0.0117
	+ 6 "	1.4170	1.4389	- 0.0219
	+ 7 "	1.3730	1.4006	- 0.0276
	+ 8 "	1.3417	1.3684	- 0.0267
	+ 9 "	1.3105	1.3410	- 0.0305
	+ 10 "	1.2845	1.3174	- 0.0329

The third column in the preceding table was calculated on the supposition, that the bulk of the acid and water united underwent no change, but continued the same after combination as before it. By comparing this column with the second, which shows the actual specific gravities of the various compounds, we see the change of volume which actually takes place. We see that when an atom of *oil of vitriol*\* combines with 1, 2 and 3 atoms water, the bulk of the compound diminishes, or the specific gravity is greater

\* I employ this term to denote a compound of 1 atom acid and 1 atom water.

than the mean. The condensation is greatest when 1 atom of oil of vitriol combines with 1 atom of water amounting to  $\frac{1}{24\frac{2}{3}}$ , that of 1 atom oil of vitriol with 2 atoms water is  $\frac{1}{38\frac{1}{2}}$ , while that of 1 atom oil of vitriol with 3 atoms water is only  $\frac{1}{9\frac{1}{2}}$ .

When we combine 1 atom of oil of vitriol with 4, 5, 6, 7, 8, 9 atoms water the specific gravity of the compound is below the mean, so that the bulk increases instead of diminishing, and this increase augments with the number of atoms of water. The following table shows the amount of the expansion.

Oil of Vitriol.	Water.	Expansion.
1 atom	+ 4 atoms	$\frac{1}{12\cdot7}$
	+ 5 "	$\frac{1}{65\cdot7}$
	+ 6 "	$\frac{1}{50\cdot7}$
	+ 7 "	$\frac{1}{51\cdot2}$
	+ 8 "	$\frac{1}{43\cdot9}$
	+ 9 "	$\frac{1}{40}$

From this remarkable change from condensation to expansion it is reasonable to infer, that a change takes place in the nature or intimacy of the combinations. The combination of 1 atom oil of vitriol with 1, 2, 3 atoms water is probably more intimate than the remaining 6, which are accompanied by expansion. Accordingly we shall find, that most of the heat evolved, when oil of vitriol and water are united, is the consequence of the union of 1 atom oil of vitriol with 1, 2 or 3 atoms water.

These experiments were repeated two or three times; but the specific gravities were found always to vary, sometimes in the second decimal place, and always in the third. This I ascribe to the great avidity of oil of vitriol for moisture. The experiments related in this paper occupied a considerable time, about two months of the summer of 1836. The first part of this summer was very dry in Glasgow, and the latter part very wet. Now, during the weighing of the oil of vitriol (which occupied at least 5 minutes) it was of necessity exposed to the atmosphere. Hence, it would imbibe moisture, and more would be imbibed when the air was damp than when it was dry. No doubt the

highest specific gravities in all cases approached nearest the truth. But as I was obliged to strike a mean in all the other parts of the investigation, I thought it better to strike a mean also in the specific gravities. Because, by so doing, I exhibit the specific gravities of the very acids which correspond with the specific heats and evolutions of heat given in a subsequent part of this paper. No doubt in some cases (perhaps in all) the acid may be a little weaker than I have stated; but the error must be too small to affect the correctness of the general results.

2. *Heat evolved when an atom of Oil of Vitriol is mixed with from one to nine atoms of Water.*

To determine this point, I sometimes mixed 24 cubic inches of oil of vitriol with the quantity of water necessary to constitute the number of atoms required; but in general I took 1000 grains of oil of vitriol, which was previously weighed in a glass cylinder, and poured into a small glass cylinder containing the requisite quantity of water. The bulb of a thermometer was previously inserted into the water. It had a scale ascending to 320°, which I knew by previous trials to be sufficient for my purpose. The mixture was stirred with the thermometer and the temperature noted. It was then set aside for 24 hours, covered with a glass plate; and the specific gravity was determined next day.

The following table exhibits the weights of oil of vitriol and water used in each experiment, together with the rise of the thermometer or the heat evolved.

Oil of Vitriol.	Water.	Weight of		Thermometer rose from	Heat evolved
		Acid. Grains.	Water. Grains.		
1 atom	+ 1 atom	1000	183.6	60° to 245°	185°
	+ 2 „	1000	367.3	67 „ 286	219
	+ 3 „	1000	550.9	60 „ 268	208
	+ 4 „	1000	734.6	60 „ 263	203
	+ 5 „	1000	918.3	60 „ 238	178
	+ 6 „	1000	1102.	59 „ 222	163
	+ 7 „	1000	1285.7	59 „ 207	148
	+ 8 „	1000	1469.3	59 „ 198	139
	+ 9 „	1000	1653.	59 „ 188	129

The greatest rise of temperature took place when 1 atom of oil of vitriol was mixed with 2 atoms water. But 1 atom oil of vitriol with 1, 2, 3 and 4 atoms water occasioned considerable evolution of heat.

But in order to be able to judge of the real source of this heat, it was necessary to make another set of experiments. I prepared nine mixtures of oil of vitriol and water in the proportions of 1 atom oil of vitriol, and 1, 2, 3, 4, 5, 6, 7, 8, 9. These were put into nine cylindrical glass jars covered each by a small flat glass. The acid in each weighed 1000 grains. They were left 24 hours in order to acquire the temperature of the room, which happened (in most cases) to be 63°. Then a quantity of water, equivalent to one atom, was mixed with each, and the augmentation of temperature was noted. The following table exhibits the result of these trials :

	Acid.	Mixture. Water.	Water.	Thermometer rose from.	Heat evolved.
1.	1 atom	+ 1 atom)	+ 1 atom	60° to 245°	185°
2.	(1	+ 2)	+ 1 „	65 to 135	70
3.	(1	+ 3)	+ 1 „	64 to 110	46
4.	(1	+ 4)	+ 1 „	60 to 95	35
5.	(1	+ 5)	+ 1 „	63 to 76	13
6.	(1	+ 6)	+ 1 „	63 to 72	9
7.	(1	+ 7)	+ 1 „	63 to 70	7
8.	(1	+ 8)	+ 1 „	63 to 69	6
9.	(1	+ 9)	+ 1 „	63 to 67	4
10.	(1	+ 10)	+ 1 „	63 to 66	3

It is obvious that the greatest rise of temperature takes place when 1 atom of oil of vitriol is mixed with 1 atom of water. The acid of the strengths indicated in Nos. 2, 3 and 4, also produced a considerable augmentation of temperature, but rapidly sinking, till the increase, when an atom of the weakest acid of all was mixed with an atom of water, was only 3 degrees.

If we calculate what the specific gravities of the above mixtures ought to be, on the assumption that no alteration in the bulk takes place by the union of the acid and water, and compare it with the actual result of experiment, we shall find that an expansion takes place in all cases except the first, when an atom of oil of vitriol is combined with an

atom of water. This will be made apparent by the following table :

Acid.	Water.	Water.	Specific Gravity By Expe- riment.	Specific Gravity By Cal- culation.	Difference.
(1 atom	+ 1 atom)	+ 1 atom	1.7837	1.7114	+ 0.0723
(1	+ 2)	+ 1 „	1.6588	1.6783	— 0.0195
(1	+ 3)	+ 1 „	1.5593	1.5807	— 0.0214
(1	+ 4)	+ 1 „	1.4737	1.5000	— 0.0263
(1	+ 5)	+ 1 „	1.4170	1.4251	— 0.0081
(1	+ 6)	+ 1 „	1.3730	1.3805	— 0.0075
(1	+ 7)	+ 1 „	1.3417	1.3430	— 0.0013
(1	+ 8)	+ 1 „	1.3105	1.3169	— 0.0064
(1	+ 9)	+ 1 „	1.2845	1.2889	— 0.0044

The expansion is a maximum when an atom of acid composed of (1 atom acid + 4 atoms water) unites with an atom of water. It amounts to  $\frac{1}{37}$ th of the whole. The least takes place when an atom of acid composed of (1 atom acid + 7 atoms water) unites with an atom of water. It amounts only to  $\frac{1}{1033}$  of the whole.

### 3. *Specific heats of various Atomic Compounds of Sulphuric Acid and Water.*

The method which I employed to determine the specific heats of a compound of 1 atom sulphuric acid with from 1 to 10 atoms water, was this. I procured a thin elipsoidal flask capable of holding about 26 cubic inches of any liquid. It terminated in a narrow neck constituting its mouth. To this a perforated cork was fitted, through which a thermometer passed, so that its bulb was very nearly in the centre of the flask, while the whole scale was without the mouth. This flask was placed on a wooden circle which grasped it about two inches from the bottom, while all the rest of the flask was free, and surrounded only by the atmosphere. The stand was fixed on a table near a window at the western extremity of a large laboratory, at a great distance from any fire, and only two individuals were present to manage the observations. 24 cubic inches of each liquid were put into the flask. It was then either by means of a spirit lamp or a sand bath raised to a temperature 80° higher than the air of the room, and the time which elapsed, while the thermometer sunk the first 40 of these

degrees, was marked by means of a watch, which pointed out seconds. Every trial was repeated three times. After the experiments were concluded, the whole results were compared together, and where any anomaly occurred two other repetitions took place. The numbers given in the following table are always the mean of at least three experiments, and four of them are the mean of five experiments. Hence, it is presumed, that the numbers given denote, with very considerable accuracy, the time which acid of each strength took to cool  $40^{\circ}$ , the commencement of cooling always taking place when the heat of the liquid was  $80^{\circ}$  above that of the room in which the experiments were made. The time occupied with these experiments was about eight weeks, from the 16th of May to about the 12th of July, and the thermometer in the room varied from  $59^{\circ}$  to  $67^{\circ}$ .

	Time of cooling $40^{\circ}$ .
Empty Flask, . . . . .	215 <sup>''</sup> ·5
Water, . . . . .	5720·7
1 atom acid + 1 water, . . . . .	3860
+ 2 „ . . . . .	4837·7
+ 3 „ . . . . .	4587·2
+ 4 „ . . . . .	4702·7
+ 5 „ . . . . .	4831·7
+ 6 „ . . . . .	4967·3
+ 7 „ . . . . .	5075
+ 8 „ . . . . .	5164·3
+ 9 „ . . . . .	5267·7
+ 10 „ . . . . .	5307·5

It will be observed in looking over this table, that 24 cubic inches of a compound of (1 atom acid + 2 atoms water) took a longer time to cool  $40^{\circ}$  than acid composed of

- 1 atom acid + 3 atoms water
- 1 „ + 4 „
- 1 „ + 5 „

This was so unexpected an occurrence, that I repeated the experiment six times and varied the acid to guard against any accidental alteration ; but still the anomaly continued. I then determined the specific heat by mixing a pound of hot glass in powder, with a pound of the cold acid, and observing the alteration of the temperature. But, not-

withstanding all these trials, the anomaly continued. I cannot, therefore, consider it as a mistake in the experiments; but must conclude, that it is owing to something peculiar in the nature of the compound.

As the liquids experimented upon were always contained within the same flask, it is clear, that in order to obtain correctly the time of cooling of each liquid, we must subtract from the time as given in the above table, the number of seconds which the empty flask took to cool  $40^{\circ}$ , which amounted to  $215''\cdot5$ .

This is done in the following table :

	True time of cooling.	Specific Gravity.	Sp.heatsof equal wts.	Sp. heats water=1.000
Water, . . . .	5505'' $\cdot$ 7	1.0000	5505	1.0000
1 acid + 1 water	3644 $\cdot$ 5	1.8422	1978	0.3593
+ 2 ,,	4622 $\cdot$ 2	1.7837	2591	0.4707
+ 3 ,,	4371 $\cdot$ 7	1.6588	2635	0.4786
+ 4 ,,	4487 $\cdot$ 2	1.5593	2878	0.5228
+ 5 ,,	4616 $\cdot$ 2	1.4737	3132	0.5690
+ 6 ,,	4751 $\cdot$ 8	1.4170	3353	0.6091
+ 7 ,,	4859 $\cdot$ 5	1.3730	3539	0.6429
+ 8 ,,	4948 $\cdot$ 8	1.3417	3688	0.6699
+ 9 ,,	5052 $\cdot$ 2	1.3105	3855	0.7003
+ 10 ,,	5092	1.2845	3964	0.7201

The numbers in the second column exhibiting the time of cooling of 24 cubic inches of the various liquids are obviously proportional to the specific heats of equal volumes of these liquids. If we divide the numbers in the second column by the specific gravities of the respective liquids exhibited in the third column of the table, the quotients will represent the specific heats of equal weights of these liquids. These quotients are given in the fourth column of the table. But, as the specific heat of water is usually received uniting, a fifth column has been added, representing the specific heat of the various compounds of sulphuric acid and water, on the supposition that the specific heat of an equal weight of water is 1.0000. This column is deduced from the fourth by a simple and obvious arithmetical calculation.

To put it in our power to judge how far the opinion first started by Dr. Irvine, and afterwards supported by Dr. Crawford, and, indeed, by every chemist who has since

written on the subject; namely, that the heat evolved when sulphuric acid and water are mixed, is owing to the formation of a new compound, whose specific heat is less than the mean of that of the two constituents; we must compare the specific heats as above given with the mean specific heats of the compound, supposing no change whatever to have taken place. This is done in the following table :

	Sp. heats by experiment.	Mean sp. heats.	Differences.
Water, . . . . .	1·0000		
1 acid + 1 water	0·3593		
+ 2 ,,	0·4707	0·4587	+ 0·0120
+ 3 ,,	0·4786	0·5326	— 0·0540
+ 4 ,,	0·5228	0·5869	— 0·0641
+ 5 ,,	0·5690	0·6306	— 0·0616
+ 6 ,,	0·6091	0·6660	— 0·0569
+ 7 ,,	0·6428	0·6952	— 0·0524
+ 8 ,,	0·6699	0·7197	— 0·0498
+ 9 ,,	0·7003	0·7405	— 0·0402
+ 10 ,,	0·7201	0·7585	— 0·0384

The slightest comparison of the second and third columns of this table is sufficient to show that the hypothesis of Dr. Irvine cannot be accurate. The specific heat of a compound of 1 atom oil of vitriol and 1 atom water is greater than the mean by about  $\frac{1}{40}$ th; yet, if we turn to the table in page 257, we see that the heat evolved amounts to no less than 185°. Now, it is impossible that this evolution of heat can be occasioned by a diminution of specific heat when no such diminution takes place. In all the other compounds there is a diminution of specific heat, but that diminution by no means corresponds with the quantity of heat evolved. The greatest diminution takes place when 1 atom of oil of vitriol is mixed with three atoms of water. It amounts in that case to 0·0641, or very nearly  $\frac{1}{15}$ th, and the heat evolved is 208°. But, when 1 atom of oil of vitriol is mixed with 2 atoms of water, the heat evolved is 219°, yet the diminution of specific heat is only 0·054 or about  $\frac{1}{10}$ th, and consequently less than when the evolution of heat is only 208°. When 1 atom of oil of vitriol combines with 5 atoms of water, the diminution of specific heat is

0.0569 or nearly  $\frac{1}{12}$ th, and the heat evolved is  $178^{\circ}$ . If Dr. Irvine's theory were correct, the heat evolved when 1 atom oil of vitriol is mixed with three atoms water, being  $208^{\circ}$ , that evolved when 1 atom oil of vitriol is mixed with 2 water should be  $178^{\circ}$  instead of  $219^{\circ}$ ; 1 atom with 5 should be  $155^{\circ}$  instead of  $178^{\circ}$ .

The same want of coincidence will be found, if we examine the whole table from beginning to end. It is clear then that the heat evolved when oil of vitriol and water combine, is not the consequence of mere diminution of specific heat.

It was observed, a good many years ago, by Dulong and Petit, that when the atomic weight of a simple substance is multiplied by its specific heat the product is a constant quantity. In a paper of mine published in the third volume of the *Records of General Science*, I have given a considerable number of examples of this law, and showed, by a pretty copious induction, that the constant quantity is 0.375. The obvious consequence deducible from this law is, that the same absolute quantity of heat exists in combination with every atom of a simple body, and that the difference of the specific heats of different simple substances is owing to a difference in the atomic weight.

In the same paper I have shown that when the atomic weight of a compound body is multiplied by its specific heat, the product is always a multiple of 0.375 by a whole number, which number depends upon, or at least is connected with, the number of atoms of which the compound body is composed. If the number multiplying 0.375 were equal to the number of atoms in the compound body, then it would follow that every atom of the compound body retains all the heat with which it was combined when in an isolated state, or that the compound retains all the heat that existed in its constituents. If the multiple be less than the number of atoms of which the body is composed, then it follows, that the compound contains less heat than existed in its elements before combination, and the difference between the multiple and the number of atoms gives us the proportion of heat which is wanting. On the other hand, if the multiple be greater than the number of atoms, the heat in the compound is greater than in its simple elements,

and the difference between the multiple and the number of atoms is the amount of the difference.

Let us multiply the atomic weights and specific heats of the compounds of oil of vitriol and water, in order to see whether this law will apply to them.

	Atomic Weight.	Specific Heat.	Product of Col. 2 & 3.	
Water, . . . . .	1·125	1·0000	1·125	=0·375 × 3
1 acid + 1 water	6·125	0·3593	2·201	=0·375 × 5·87
+ 2 „	7·25	0·4707	3·412	=0·375 × 9·09
+ 3 „	8·375	0·4786	4·008	=0·375 × 10·68
+ 4 „	9·5	0·5228	4·966	=0·375 × 13·24
+ 5 „	10·625	0·5690	6·046	=0·375 × 16·12
+ 6 „	11·75	0·6091	7·157	=0·375 × 19·08
+ 7 „	12·875	0·6429	8·277	=0·375 × 22·07
+ 8 „	14	0·6699	9·379	=0·375 × 25·01
+ 9 „	15·125	0·7003	10·592	=0·375 × 28·24
+ 10 „	16·25	0·7201	11·702	=0·375 × 31·20

The last column of the table shows to what number multiplied into 0·375, the product of the atomic weight by the specific heat, is equal. A slight examination of that column is sufficient to show that the respective multiples approximate to the following numbers.

	Multiples.
Water, . . . . .	3
1 acid + 1 water, . . . . .	6
+ 2 „ . . . . .	9
+ 3 „ . . . . .	11
+ 4 „ . . . . .	13
+ 5 „ . . . . .	16
+ 6 „ . . . . .	19
+ 7 „ . . . . .	22
+ 8 „ . . . . .	25
+ 9 „ . . . . .	28
+ 10 „ . . . . .	31

Or there is a regular increment by 3, except in the case of (1 atom acid + 3) and (1 atom acid + 4 water), when the increments are only 2.

If we admit that these numbers multiplied by 0·375 give the true products of the atomic weight into the specific

heat, we have it in our power to calculate the true specific heat of all these compounds. For it is clear that the product of these multiplied by the number 0.375, and divided by the atomic weight, must give the true specific heat. The following table has been calculated in that way.

	True sp. heat.	Mean sp. heats.	Differences.
Water, . . . .	1.0000		
1 acid + 1 water	0.3673		
+ 2 „	0.4655	0.4587	+ 0.0068 or $\frac{1}{68}$
+ 3 „	0.4925	0.5326	- 0.0401 or $\frac{1}{12}$
+ 4 „	0.5131	0.5869	- 0.0738
+ 5 „	0.5647	0.6306	- 0.0597
+ 6 „	0.6063	0.6660	- 0.0541
+ 7 „	0.6407	0.6952	- 0.0545
+ 8 „	0.6696	0.7197	- 0.0501
+ 9 „	0.6942	0.7405	- 0.0463
+ 10 „	0.7153	0.7585	- 0.0432

The differences between the mean specific heats and the true (supposing the second column to represent these) is somewhat diminished in the first six, and somewhat augmented in the last four numbers; but its nature is nowhere altered.

Sulphuric acid is a compound of 1 atom sulphur and 3 atoms oxygen; so that an integrant particle of it contains four atoms. There are two opinions respecting the constitution of water. The continental chemists, in general, consider it as a compound of 1 atom oxygen and 2 atoms hydrogen; while British chemists, with scarcely any exception, consider it as a compound of 1 atom oxygen and 1 atom hydrogen. According to continental chemists the weight of the atom of hydrogen is 0.0625, while according to British chemists it is 0.125 or twice as great. We have no means of determining which of these two opinions is the true one. Both are supported by very plausible arguments. But it will suit our present views better to adopt the continental opinion, and to admit that water is a compound of 1 atom oxygen + 2 atoms hydrogen, or that it is a triple compound.

We may draw up a table representing the number of atoms contained in an integrant particle of each of our

liquids, and place beside them the multiples of 0.375, which represent the specific heat of these liquids.

	Number of atoms	Multiples of 0.375.	Differences.	Heat evolved.
Water, . . . . .	3	3	0	
1 acid + 1 water	7	6	1	
+ 2 „	10	9	1	185°
+ 3 „	13	11	2	70
+ 4 „	16	13	3	46
+ 5 „	19	16	3	35
+ 6 „	22	19	3	13
+ 7 „	25	22	3	9
+ 8 „	28	25	3	7
+ 9 „	31	28	3	6
+ 10 „	34	31	3	4

From this table it appears that when an integrant particle of oil of vitriol is combined with an integrant particle of water, the specific heat of the compound, instead of being  $0.375 \times 7$  (the number of atoms), is only  $0.375 \times 6$ . So that one of the atomic constituents is deprived of the whole of its heat: in other words one-seventh part of the whole heat is disengaged. This portion, by experiment, amounts to  $185^\circ$ ; so that the whole heat in the oil of vitriol and water before combination must have been  $1295^\circ$ .

When one atom of (one acid + two water is mixed with one atom of water, the heat of the compound is less than  $0.375 \times 13$  (the number of atoms) by 2. But, the combination of 1 oil of vitriol and 1 water is less by 1. Hence, in this second combination, the real diminution is only 1; so that the heat evolved should be  $\frac{1}{13}$ th of the whole. I found it only  $70^\circ$ . But the experiment was made upon too small a scale. When the experiment was made by mixing 24 cubic inches of oil of vitriol with the requisite quantity of water to constitute a compound of (1 atom acid + 3 atoms water), the heat evolved was  $219^\circ$ . Now, if this be  $\frac{2}{13}$ ths of the whole heat as it ought to be from the table, the whole heat would amount to  $1423^\circ.5$ . By the second experiment in which the heat evolved was  $70^\circ$ , the whole heat would amount to  $1110^\circ$ . Now the mean of these two numbers is  $1266^\circ$ , which nearly agrees with the deduction from the mixture of oil of vitriol with one atom of water.

When an integrant particle of (1 acid + 3 water) is mixed with an integrant particle of water the heat evolved is  $46^\circ$ . In this the multiple of 0.375, which represents the specific heat, is less than the number of atoms present by 3. Therefore, 3 atoms of the constituents are destitute of heat. But two of these atoms are accounted for by the two preceding combinations. Hence, the diminution by mixing (1 acid + 3 water) with 1 water should be only  $\frac{1}{6}$ th; or in other words,  $\frac{1}{6}$ th of the heat should be evolved. But  $16 \times 46 = 736$ . The reason of this great diminution is, probably, the greater quantity of matter to be heated, while the absolute quantity of heat evolved is nearly the same. In the first experiment 1000 grains of oil of vitriol and 183.6 grains of water were mixed; in the second 1550.9 grains of acid of (1 acid + 3 water) were mixed with 183.6 grains of water. It is impossible that the sensible heat evolved can be the same when the quantity of matter to be heated is constantly increasing. It is not worth while to investigate the table farther.

The preceding experiments appear to me to show, in the clearest manner, that the whole theory of Dr. Irvine respecting the cause of the evolution of heat, when two liquids combine, is inconsistent with fact, and, therefore, erroneous. The quantity of heat evolved appears in all cases to be proportional to the energy and rapidity of the combination. The method which I have indicated in this paper, and a preceding one seems to lead to a pretty obvious way of determining what portion of the whole heat in a body is evolved when two liquids combine. But it would be requisite to vary and extend the experiments a good deal farther before we draw definite conclusions. Nitric acid and alcohol might be employed with advantage. As the avidity of these bodies for water is not so great as that of sulphuric acid, the experiments could be made with greater ease and accuracy; while the diminished increase of temperature would enable us to determine the heat evolved with greater certainty than can be done when the thermometer suddenly rises from  $60^\circ$  to  $286^\circ$ , as it did in one of my experiments.

ARTICLE III.

*Important Facts derived Mathematically from a General Theory, embracing many Results in Chemistry, which are denominated ultimate facts.* By THOMAS EXLEY, A.M.

(Communicated to the Chemical Section of the British Association, Aug. 23rd, 1836.)

It is not for one man to build the temple of science, many must be employed.

You, veterans in science, you have collected an immense mass of materials. Many have digged for a foundation, but every one yet examined has proved sandy. It has been my lot, through the guidance of the Great Architect, to find the rock on which you may safely build.

My object is to place chemistry under the domain of mathematical science, and to establish my new theory by easy calculation and mathematical proofs.

The two principles, which form the foundation, are these; viz.

1st. Every atom of matter consists of an indefinitely extended sphere of force, which varies inversely as the square of the distance from the centre; and that this force acts *towards* the centre, and is called attraction at all distances, except in a small concentric sphere, in which it acts *from* the centre, and is there called repulsion.

2nd. That there is a difference in atoms, arising from a difference in their absolute forces, or in the radii of their spheres of repulsion, or in both these respects.

The theories of Newton and Boscovich agree perfectly with this, as far as regards the attraction in the first principle: after that Newton and Boscovich go together in conceiving a series of alternate spheres of attraction and repulsion, governed by unknown laws, but, as regards change of direction the forces graduate into each other. Boscovich reaches the centre with a sphere of repulsion which varies inversely as the simple distance, making the force at the centre infinite; while Newton closes with a solid nucleus, which is only an infinite force long before we reach the centre. The new theory rejects all these hypothetical, unsubstantiated forces, and their feigned alterations; and, with the utmost possible degree of simplicity, admits of but one sphere of repulsion, in which, without interruption, the law of gravitation in the attractive

sphere is invariably continued down to the centre itself, where it terminates with the infinite force of Boscovich repeated an infinite number of times. The direction changes to the opposite one, *per saltum*, at the surface of the sphere of repulsion: and why not? It is quite as easy and more natural to conceive that it thus changes at once, than that it is always changing continuously backward and forward; but, which is a matter of great moment, the continuity of the quantity and of the law of force remains unbroken, preserving the delightful harmony of nature.

The inductive philosophy requires and demands this continuity in the law; unless the contrary could be shown in any instance, we have as much right to say that the law of gravitation does not exist in the infinity of places where no observations have been made, as to say it does not exist in the sphere of repulsion, that important space in which chemistry and its connate sciences produce all their phenomena.

Thus since the 1st principle as to attraction has long been established completely, by induction, and beyond the power of controversy, and since we know from facts that a central repulsion exists, the same induction obliges us to admit the same force in the sphere of repulsion, especially as not a single instance of repulsion acting according to any other law can be shewn to exist, as belonging to any atom of matter.

The 2nd principle is perfectly simple and natural, and is established by means of the first principle and induction from facts; for it is known that atoms do differ from each other, and the difference stated is in complete unison with the first principle, and quite sufficient to furnish all the variety of atoms yet observed, and an infinitely greater variety.

The material will of course be allowed, and we readily admit that the creator originally brought into existence, according to number, weight and measure, a quantity of every sort of atoms requisite for the purposes of his grand design in the structure of the universe.

Every variety of atoms may, according to the theory, be assumed; but to find what sorts really exist, phenomena should direct the assumptions.

In my "New Theory of Physics," it was stated that nature presents *two classes* of atoms; the one comprehending the elementary substances most generally known, such as

oxygen, hydrogen, carbon, &c., which, adhering with great tenacity, may (till a better name be found) be termed *tenacious atoms*. The other included such matter or atoms as manifest their existence by motions and actions, under a form which has been denominated ethereal, and hence they may be called *ethereal atoms*; to this class was assigned the electric fluid, caloric and light.

In the same work the atoms of electric fluid were considered as having a much greater absolute force, than those of caloric and light; and this has been abundantly confirmed by subsequent observations, entitling the electric atoms to the rank of an intermediate class. Hence, we have three classes of atoms, viz., tenacious, electric, and ethereal atoms. Of the 1st and 3rd classes there are many sorts, but probably only one sort of electric atoms; this division and arrangement will at least serve our present purpose.

The distinction of the classes is founded in a very great difference of the absolute force; that of the sorts in a moderate difference: thus, if the absolute force of an atom of oxygen be 16, and that of hydrogen 1, they will be two sorts of tenacious atoms; an electric atom must be considered very much less in absolute force than that of either of the former, and the several atoms of light and caloric perhaps many millions of times less than that of an electric atom.

In this paper the atomic weight of oxygen is 16, that of hydrogen being 1, as the unit of comparison. It appears to me exceedingly unfortunate that the British chemists have adopted 8 instead of 16: they tell us, which shews a want of confidence in their own arguments, that it is of no great consequence which of these opinions be adopted. This is indeed true as it regards many experimental determinations, but in theory it is exceedingly important. Is it of no consequence to know whether a compound contain in each particle 2, 3, 4, &c., atoms? If oxygen be 8, a particle or atom of ether contains 10 simple atoms, but, if oxygen be 16, it contains 15 atoms: would not such a difference alter all or most of its properties? A question so deeply scientific ought not to be treated with indifference.

I have not seen one argument in favour of 8 which has any great point or weight: in favour of 16 only one has met my notice which is a good one: it is this; the simple gases hydrogen, nitrogen, and chlorine, contain an equal

number of atoms in equal volumes, and oxygen is as much entitled to the character of a simple gas as any of them: hence, it is reasonable to conclude it is not an exception to the rule, but this would require 16 for its atomic weight.

The following arguments appear to me decisive.

1. Sulphurous acid is the sole gaseous product, when sulphur is burnt in dry oxygen gas, and the resulting volume is the same as that of the oxygen consumed.

2. Carbonic acid is the sole gaseous product when carbon is burnt in oxygen gas, and the resulting volume is the same as that of the oxygen consumed.

3. Steam is the sole gaseous product when oxygen is burnt in hydrogen gas, and the resulting volume is the same as that of the hydrogen consumed.

4. Sulphuretted hydrogen is the sole gaseous product when sulphur is burnt in hydrogen gas, and the resulting volume is the same as that of the hydrogen consumed.

Besides, these substances have all been obtained in the form of gases and limpid liquids; now the striking analogies before us prove, that they are formed after the same manner: but, in the opinion of all parties, the first two contain three atoms each; hence, the others contain three atoms each, and 16 is the atomic weight of oxygen.

Again, take sulphurous and hypo-sulphurous acids on the one hand, and water and deutoxide of hydrogen on the other; then, 1st. Sulphurous acid is formed by burning sulphur in oxygen gas, and the volume of oxygen is not changed; and the new gas may be passed through red hot tubes without decomposition: but, several substances which have a strong affinity for oxygen, as potassium, carbon, &c., decompose it: also by a slight pressure it becomes a limpid liquid. 2nd. Hypo-sulphurous acid contains twice as much sulphur as the sulphurous acid; it is easily decomposed, and cannot remain permanent at common temperatures.

Now the same sentence, with scarcely any variation, may be read for the analogous substances, water and the deutoxide of hydrogen, by merely substituting the names of these compounds and their elements. But sulphurous acid consists of two atoms oxygen and one sulphur, and hypo-sulphurous acid of one of each, which ever view of the subject be taken; hence, water must be allowed to be two

atoms hydrogen and one oxygen, and deutoxide of hydrogen consists of one of each; hence, 16 is the atomic weight of oxygen.

The same sentence will nearly apply to the following couple of compounds, viz., sulphuretted hydrogen and bisulphuretted hydrogen: these then must agree in composition with one of the former couples, which confirms the conclusion. Many similar compounds exist and testify the same thing. One additional instance will be abundantly sufficient, taken from carbonic and nitric oxides.

1. A volume of carbonic oxide is double that of its oxygen, and combined with another volume of oxygen, it becomes carbonic acid, without change of volume.

2. A volume of nitric acid is double that of its nitrogen, and combined with another volume of nitrogen it becomes nitrous oxide, without change of volume.

It follows from these analogies, that the substances before us are composed after the same manner: now, according to both views, carbonic oxide is one atom oxygen and one carbon; hence, nitric oxide is one nitrogen and one oxygen; but by weight the constituents are in the ratio of 14 to 16, and 14 is the atomic weight of nitrogen, therefore, 16 is that of oxygen.

The other atomic weights used in this paper are taken from Dr. Thomson's determinations, doubling some of them on account of using 16 for oxygen. They are inserted in the following table, and the numbers of Berzelius are annexed; the substances in italics are double the numbers given by Dr. Thomson.

Name.	Atomic Weight by		Name.	Atomic Weight by	
	Thomson.	Berzelius		Thomson.	Berzelius
Oxygen,	16	16·026	Arsenic,	38	75·329
<i>Sulphur,</i>	32	32·239	<i>Boron,</i>	16	21·793
Nitrogen,	14	14·189	<i>Carbon,</i>	12	12·250
Fluorine,	18		<i>Tellurium,</i>	64	129·243
Chlorine,	36	35·470	<i>Titanium,</i>	52	62·356
Bromine,	80	79·263	<i>Silicon,</i>	16	44·469
Iodine,	126	123·206	Hydrogen,	1	1
<i>Selenium,</i>	80		Mercury,	100	202·868
Phosphorus,	16	31·436	<i>Tin,</i>	116	117·839

Berzelius has, with great propriety, set down the results of his very accurate experiments without correcting them by theory; it would be well to give also the results thus

corrected. From the calculated and experimental specific gravities of 57 compounds in the table appended to the 8th proposition, it appears to me, that the numbers, as given by Dr. Thomson, are nearer the truth than those given by Berzelius; and this more particularly seems to be the case in respect to two or three, which I have examined more at large, as may be seen in respect to carbon from the ten compounds in the following table. The specific gravities are calculated by a rule drawn from the 8th proposition, namely to multiply the sum of the atomic weights of the elements by the specific gravity of hydrogen, when the elements combine in single groups, and by half that sum when they combine in double groups.

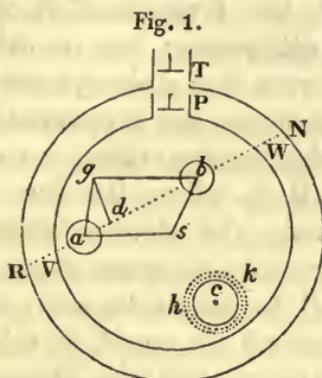
Name.	Atomic wt. of carbon.	Specific Gravity		Authority and result.
		By calculation.	By experiment	
Carbonic oxide	12	.9721	.9732	Thenard & Berzelius, mean; 1st .0011 defect, 2nd .0163 excess.
	$12\frac{1}{4}$	.9895		
Carbonic acid	12	1.5277	1.5213	Thenard & Gay Lussac, mean; 1st .0064 excess, 2nd .0238 excess.
	$12\frac{1}{4}$	1.5451		
Light carbur. hydrogen	12	.5555	.5590	Thomson; 1st .0035 defect, 2nd .0138 excess.
	$12\frac{1}{4}$	.5728		
Alcohol . .	12	1.5972	1.6133	Gay Lussac; 1st .0161 defect, 2nd .0186 excess.
	$12\frac{1}{4}$	1.6319		
Etherine . .	12	1.9444	1.9100	Faraday; 1st .0344 excess, 2nd .0691 excess.
	$12\frac{1}{4}$	1.9791		
Ether . . .	12	2.5694	2.5830	Gay Lussac & Depretz, mean; 1st .0136 defect, 2nd .0558 excess.
	$12\frac{1}{4}$	2.6388		
Naphtha . .	12	2.8472	2.8330	Saussure; 1st .0142 excess, 2nd .0663 excess.
	$12\frac{1}{4}$	2.8993		
Naphthaline	12	4.4444	4.5280	Dumas; 1st .0836 defect, 2nd .0032 excess.
	$12\frac{1}{4}$	4.5312		
Paranaphthal.	12	6.6666	6.7410	Dumas; 1st .0074 defect, 2nd .1860 excess.
	$12\frac{1}{4}$	6.9270		
Camphene . .	12	4.7222	4.7670	Dumas; 1st .0348 defect, 2nd .0420 excess.
	$12\frac{1}{4}$	4.8090		

In all these ten substances, if  $12\frac{1}{4}$  be taken for the atomic weight of carbon, the calculated specific gravity exceeds that found by experiment. In three of them it is so even when 12 is taken, and in the rest defect is very much less than the excess, except in naphthaline, which shews that 12 is much nearer the true atomic weight of carbon than  $12\frac{1}{4}$ .\*

\* I have shewn from considerations connected with its specific heat, that the atomic weight of carbon must be .75 or 12. and cannot be .764.—Records, vol. ii. 38.—EDIT.

*Prop.* 1. To determine the general effect of one, two, or more tenacious atoms placed in a vessel, in which ethereal atoms are compressed by a given force, so that of any two contiguous atoms, the centre of one is within the sphere of repulsion of the other; the tenacious atoms being separated by intervening ethereal matter.

Let the ethereal matter be compressed in a spherical vessel R T N (fig. 1.), as by a given force on the piston T,



and let *c*, a tenacious atom, be introduced. Now, for a moment, suppose the attraction of this atom not to act; on this supposition, the ethereal matter will continue to be uniformly diffused through the vessel, quite to the surface of the sphere of repulsion *h k*, within which the centres of the contiguous atoms are supported against the given pressure: let now the attraction of this atom have its full and proper effect; evidently the surrounding ethereal matter will be attracted towards it (1st prin.), and condensed on the surface of repulsion *h k*, and the change of tension in the neighbouring parts of the vessel will be quickly restored to its former state by the given pressure at T: hence, an atmosphere of ethereal matter, diminishing in density from the surface outward, will be accumulated, and retained on that surface, more or less dense, as the absolute force of the atom is greater or less, or the radius of its sphere of repulsion is less or greater.

When there are several sorts of ethereal matter in the vessel, those sorts which have the greatest absolute force, or the least sphere of repulsion, will occupy the lower strata of the atmosphere; because, whenever the equilibrium is disturbed, such atoms will be most easily moved

among the rest, by the action of  $c$ ; hence, electric atoms, if present, will form the lowest stratum.

When there are several tenacious atoms in the vessel, each will similarly retain an atmospherule on its surface.

Next, let there be two tenacious atoms,  $a$  and  $b$ , in the vessel, and let their forces on an ethereal atom at  $g$  be each resolved into two, one in  $R N$ , passing through their centres, the other in  $g d$ , perpendicular to  $R N$ . When  $d$  is between  $a$  and  $b$ , the forces in  $R N$  oppose each other, and act by their difference; but in other cases by their sum: again, the forces in  $g d$  always act according to their sum; and, as these forces are supported by equal forces on the opposite side, the constant effect is to condense ethereal matter on the line  $R N$ , where the most powerful ethereal atoms, and especially the electric atoms will be chiefly collected, for the reasons assigned above.

Hence, there will be an atom, as  $z$ , in  $a b$ , undisturbed in the middle, when  $a b$  are equal, in other cases, nearer to the less powerful atoms: and the atoms condensed in the line  $a b$  will be equally pressed and supported on all sides by the contiguous atoms.

*Cor.* When electric atoms are in the vessel, they also will retain small atmospherules of ethereal matter, which, although less dense than those of the tenacious atoms, will have considerable density if the spheres of repulsion of the electric atoms be very small, which is probable. It is also manifest, that the atmospherules of both the tenacious and electric atoms will be more dense, when the ethereal atoms are more compressed or crowded together.

*Prop.* 2. Things being as in prop. 1, the actions of any two atoms on each other, combined with the mutual actions of the whole mass on each of the two, will be a repelling force between them, inversely proportional to their distance.

Let  $s$  be the centre of the vessel in which ethereal atoms of one sort are compressed by a considerable force: then, since the absolute force of the ethereal atoms is very small, the distance between their centres will also be exceedingly small, constituting points in a sphere, such as in Newton's 73rd Prop. B. 1. Prin., and by that proposition any corpuscle or atom  $a$ , placed at any point of this sphere, will, by the mutual actions of the whole mass be attracted by a

force proportional to its distance from the centre  $s$ ; hence, if the atom  $a$  were left to the action of this resultant, undisturbed by any other influence or obstacle, it would move to the centre by a velocity determined by this law. The same reasoning applies to any other atom  $b$ , in the sphere; therefore, both would, in the absence of all obstacles, or other force on each other, approach, and at the same time meet in the centre, and always their distances from each other would be proportional to that of either from the centre: but this measures their accelerating force, which is, therefore, as their distance.

But, besides the mutual actions, which alone would produce the above motions, the atoms  $a$  and  $b$  act independently, and directly on each other, by an accelerating force, inversely, proportional to the square of their distance, (1st prin.); this must, therefore, be compounded with the former; thus, the force between them varies as the distance, directly, and as the square of the distance, inversely; that is, as the distance inversely.

Again, since one of the centres of every two contiguous atoms is within the sphere of repulsion of the other; the force, here investigated is a repelling force; which also appears from this, that if the compressing force were removed the atoms would separate: hence, the proposition is true, when the ethereal atoms are of one kind. But, if any number of these be removed, and their places supplied by other atoms, in such manner, that exactly the same equilibrium may be maintained, we shall still have the same conclusion.

*Prop. 3.* If the absolute forces or spheres of repulsion of the tenacious atoms be increased or diminished, the resultant repelling force, as determined in the last proposition, will not be altered: provided that none of the atmospherules of tenacious atoms are penetrated by the centres of others, so as to displace the atmospherules on the contiguous sides; that is, on the parts between the two tenacious atoms.

For their tendency to separate depends, not on their absolute forces, or spheres of repulsion, as is evident from the last proposition; but on the law of force, and the given pressure, and these remaining, the repelling force between the atoms  $a$  and  $b$  will also remain unaltered.

Or thus: let one of the atoms be increased in its absolute force in any ratio; then the force between it and every other atom in the vessel is increased in the same ratio: but the repulsion between it and contiguous atoms, and, consequently, between all contiguous atoms, is increased in that ratio: therefore, the equilibrium continues; that is, a variation in the absolute force produces no change of equilibrium, and their tendency to separate remains as before. The truth of the proposition is manifest, when the sphere of repulsion only is changed.

*Def. 1.* A single group of atoms is a collection of two or more tenacious atoms, such, that all their centres are within the sphere of repulsion of some one of them, as in fig. 4.

*Def. 2.* A double group of atoms is two tenacious atoms, or two single groups, or one atom or single group connected by a third atom or single group, such that the connecting atom or group displaces the greatest part of the ethereal and electric atoms between the two atoms or groups which it connects, and the parts of their atmospherules on the contiguous sides, as in fig. 5 and 6.

*Cor. 1.* Considering a single group as one atom, there will be always in equal volumes of different gases an equal number of atoms, the pressure being given.

For, 1st, when the tenacious atoms are distinct, and separate, and of the same kind; this follows from the 2nd and present propositions; since, being in the gaseous form, they are kept apart by intervening ethereal matter; and, since they are of the same kind, they will be uniformly arranged in the vessel; therefore, on the other hand, if two gases of two given sorts occupy equal volumes, and contain an equal number of tenacious atoms, the centres will be equi-distant; therefore, the separating forces (by this and the preceding proposition) will be equal; and hence, they will sustain the same pressure; therefore, when the pressure is given, the number of atoms is equal.

2nd. It is manifest from the same propositions, that a single group will occupy a volume equal to that occupied by a single tenacious atom; for, since the centres of all the atoms in the group are within the sphere of repulsion of one of them, the centre of gravity of the group may be considered as the centre of a single atom, and the contour

of the spheres of repulsion as a surface of repulsion of greater magnitude; hence, it will have a single distinct atmospherule, and will act as a single atom, and occupy (by this and the preceding proposition) the same volume; hence, the cor. is manifest.

*Cor. 2.* When two tenacious atoms are connected chemically, yet so as not to form a single condensed group, they will occupy, in a gaseous body, the same volume as they did before the connexion took place.

For, according to this and the last propositions, they are kept apart by the same force, as that by which they were before separated.

The connecting link will be considered afterward: such may be called cohesive combinations.

*Cor. 3.* A double group will occupy in a gaseous body exactly twice the volume of a single tenacious atom, or of a single group.

For the atom or single group connecting two others, as in def. 2, displaces the ethereal atoms, and the parts of the atmospherules between them; and, because of the given pressure, the same equilibrium will be maintained; so that the connecting atom will perform the effects of the displaced ethereal matter, and, therefore, will not alter the distance between the connected atoms; the same arguments apply to single groups as to single atoms.

*Cor. 4.* When gases are mixed, and no chemical union, or only cohesive combination occurs, the volume is not changed.

This is manifest from the proposition, since an alteration in the absolute force or sphere of repulsion does not alter the distance between the centres of the atoms, so that each still occupies the same volume.

*Remark.*—If an objection be made to this proposition and its cors. by an appeal to fact, that the specific gravity of sulphur vapour is 96, that of hydrogen being 1, while the atomic weight of sulphur is only 32; it is easily obviated; for there will be perfect agreement, if the vapour of sulphur consists of single groups of two atoms each; and this is likely, since sulphur has two fusing points, and the liquid is less limpid after the second than after the first, besides other peculiarities.

If the atomic weights of phosphorus and arsenic be 16 and 38, their vapours are in single groups of four atoms each, probably in tetrahedrons, rendering them isomorphous.

It is well known that experiment bears out these mathematical conclusions.

*Prop. 4.* Things being as in the preceding propositions, and the tenacious atoms in a gaseous state compressed in an inner concentric spherical vessel, which contains tenacious atoms, but admits a perfectly free communication to ethereal atoms; then the pressure on the exterior vessel being given, the density of the gas will vary as the compressing force on the inner vessel.

Let the inner vessel be  $VWP$ , having the same centre  $s$ , and let the tenacious atoms in it be compressed, as by a piston, at  $P$ , such, that it shall continue to retain the gaseous form.

Then the internal pressure of the tenacious atoms on a given surface, and, consequently, the compressing force on the same, as on the piston  $P$ , will be as the number of pressing atoms, and as the force of each; but the number of atoms on the given surface is inversely as the square of their distance (geom.), and the force of each is inversely as the distance (prop. 2); therefore, the compressing force is inversely as the cube of the distance, but the density is in that same ratio: therefore, the compressing force varies as the density.

*Cor.* The volume varies inversely as the pressure, because it varies inversely as the density.

*Def.* The pressure at  $T$  answers to the effect of temperature, and the pressure at  $P$  to that of the usual pressure on gases; it arises from the re-action of the included tenacious atoms; hence, the first at  $T$  may be called the pressure of temperature, and the other at  $P$ , the pressure or re-action; or, for conciseness, simply the pressure and the temperature.

*Prop. 5.* The pressure or re-action at  $P$ , (fig. 1.) being given; the volume will vary, as the pressure at  $T$ , or the temperature.

For the volume being given, the temperature or pressure at  $T$ , will vary as the resulting force determined in prop. 2, because that is the sole cause which brings the forces into

operation; but the pressure at P also varies as that resultant, because these forces sustain each other in equilibrium; therefore, the pressure at P varies as that at T, when the volume is given; but the pressure at P varies inversely as the volume, when that at T is given (prop. 4. cor.); therefore, the pressure at P varies as that at T directly, and the volume inversely: therefore, when the pressure at P is given, the temperature varies as the volume.

*Cor.* If any temperature T, and its corresponding volume V, be assumed, and  $v$  be the increment of one volume, for one degree of temperature, taken at T; also  $t$  a given increment of temperature taken from the same term T; then,  $T : T + T v t :: V : V + V v t = V (1 + v t)$  = the volume at the temperature T +  $t$ ; which is known, when V,  $v$ , and  $t$  are given.

This is a well known theorem, agreeing with experiment,

Example, if  $T = 32^{\circ}$  Fahr. then  $v = \frac{1}{480}$ ; let  $V = 20$ ,  $t = 30$ ; then  $V (1 + v t) = 20 \times \frac{510}{480} = 21.25 = \dot{V}$ , the new volume: had  $\dot{V}$  been given we should have had  $V = \frac{\dot{V}}{1 + t v} = \frac{21.25 \times 480}{510} = 20$ , in this case, the volume at  $32^{\circ}$  Fahr.

*Prop. 6.* If ethereal and electric atoms be compressed in a vessel, fig. 1. the pressure and temperature being given, and also the absolute force of a tenacious atom in it, while its sphere of repulsion is supposed to vary; then there will be a certain magnitude in its sphere of repulsion, at which it will collect and retain a maximum quantity of electric atoms on its surface of repulsion.

For, 1st, let the sphere of repulsion be extremely small, then the attraction at the surface is very great, (1st. prin.) therefore, the density of the ethereal matter at the surface is very great, (prop. 1.), and, therefore, the electric atoms at the surface obtain an increased atmospherule of ethereal matter (prop. 1, cor.); hence, the relative attraction of the tenacious and electric atoms is diminished, by their united actions on the adjacent ethereal matter; and this conjoined with the extremely small surface of repulsion will operate against the firm attachment of the electric atoms, which

will, therefore, the more readily pass off to other neighbouring tenacious atoms.

2nd. Next, let the radius of the sphere of repulsion be very large; then at the surface of repulsion, the force is very small, and the electric atoms are, therefore, loosely retained; and, hence, will on a disturbance of the equilibrium pass off to other tenacious atoms, having their absolute forces and spheres of repulsion better proportioned to retain them.

From these two parts, it follows, that when the absolute force of the atom is given, there is a certain magnitude of the sphere of repulsion, at which it will retain a maximum quantity of the electric atoms.

*Cor. 1.* When the pressure and temperature are given, there is a fixed and definite relation between the tenacious atoms of bodies, and those of the electric fluid attached to them, as to quantity.

*Cor. 2.* When the temperature is varied, the relation between the tenacious and the electric atoms, is in some degree altered.

For the densities of the ethereal atmospherules will be varied in different ratios, on account of the difference in their spheres of repulsion.

*Cor. 3.* From this it appears, that a variation of the temperature will alter the electrical relations of elements, and, consequently, their tendency to combine.

*Prop. 7.* When the elements of bodies combine chemically; the ratios by weight of the quantities are fixed and definite, and such as may be expressed in small whole numbers.

Since tenacious atoms in common circumstances, are always situated in a compressed body of ethereal matter; they will be encompassed by distinct atmospherules (prop. 1.): this evidently tends to keep them apart; and, hence, there can be no combination, unless either one or more of the atoms or groups penetrate the atmospherule of the other, so as to form a single group: or that the atoms are connected by some intermediate link, as either another atom, or intervening electric atoms, which may collect between them (prop. 1, and cor.). When this change is effected, the new particle will be invested with a distinct

common atmospherule, (prop. 3 and 1,): hence, if it can combine with an additional quantity of the same element, it will be with another atom or group, half an atom being impossible; after the compound has attained a certain state or quantity, it will not in general, on account of the mutual actions of the combined atoms and the ethereal matter, be in a condition to enter into new combinations, except by cohesion, or by the expulsion of some of its elements: hence, the proposition is manifest.

*Prop. 8.* Taking each elementary atom as representative of a volume; then in all strictly chemical combinations, that is, whenever there is a condensation, the resulting volume is always, without exception, either one or two volumes exactly, whatever number of volumes combine.

For, since the volume is diminished, the centre of some atom, or those of several atoms, have penetrated the atmospherule of some other (prop. 3 and cors.)

1. When the atmospherule of one atom or single group is penetrated by the centres of all the other atoms, the result is a single group, and, consequently, (prop. 3 and cors.) that result will be one volume exactly.

2. When one atom or single group combines with a single group, and all the centres do not rest within the sphere of repulsion of one of them; then one or more of the atoms will be brought, by their mutual actions, to the interval between the remaining atoms or single groups, which combine, and so situated, will (prop. 3 and cors.) supply the effect of the ethereal matter which it displaces; and the whole will form a double group, and (same cors.) will become two volumes exactly.

3. When one atom, or single group combines with a double group; the centres of the combining atoms of the single group, or that of the single atom, will penetrate the atmospherule of the double group; otherwise there would be only a cohesive combination; hence, the whole when combined will continue a double group, and will form exactly two volumes (prop. 3, cor. 3); except when the mutual actions bring all the centres within the sphere of repulsion of one of them, in which case, (prop. 3, cor. 1) they will become one volume: hence, still we shall have either a single or double group, so that evidently no other

case can occur: therefore, the resulting volume will be always exactly one or two, however, many volumes combine.

*Cor.* This proposition embraced the theory of volumes, which it explains, simplifies, and extends.

The important law contained in this proposition, in its whole extent, has not been before determined, although approaches towards it have been made in the theory of volumes: and after Gay Lussac had discovered that theory, a striking relation between the atomic weights of bodies, in the gaseous form, and their specific gravities could not long remain unobserved; and this was particularly noticed by Dr. Prout; but the exact and remarkable law just demonstrated, has, I believe, never been made clearly out in its generality, but many particular cases have been established. As far as experiments go they serve to confirm the above mathematical conclusion; and this demonstration would not have been the less valuable, had the law been established in its full extent by observation of facts: for even in that case, it could have been received only as an ultimate fact utterly inexplicable.

From this proposition and the new theory, we have the farther advantage of knowing in what manner the atoms may be combined: and that frequently the combinations of the same atoms admit of different forms, producing isomeric bodies: also the manner of combining being given, the theory foretells perfectly the resulting volume.

Having deduced this exact law from my two simple principles, it became to me exceedingly important to ascertain if this result is really true in fact. To determine this point, I have calculated from this law the specific gravities of a great number of compounds supposed to be in the gaseous form, having regard to the combinations as to their being only cohesive, or in single or double groups, and in doubtful cases, calculating the specific gravity both on the supposition of single, and that of double groups. I then collected all the substances I could find of which the specific gravities had been determined by experiment, and have put them to the amount of 57, in a tabular form, from which it appears that there is a complete agreement within the allowable errors of such experiments, except in boro-

chloric acid, which Dr. Thomson says requires farther investigation, and besides a small discrepancy in oil of turpentine, a substance not easily obtained in a perfect state; so that the rule appears to be without exception. The calculations are evidently easy and simple; all that is required, is to add together the atomic weights of the simple elements contained in the compound, and multiply the sum by  $\cdot 0694$  the specific gravity of hydrogen, which gives the specific gravity required, when the elements combine in single groups, and half that product is the specific gravity, when the combination is in double groups: but when there is only a cohesive combination, the same product is to be divided by the number of elements which combine.

These three are the only varieties of chemical combination which can occur.

Fig. 2.

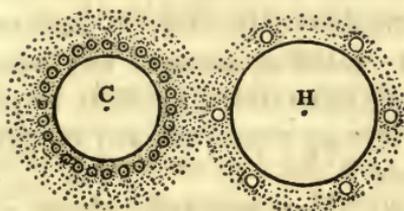


Fig 3.

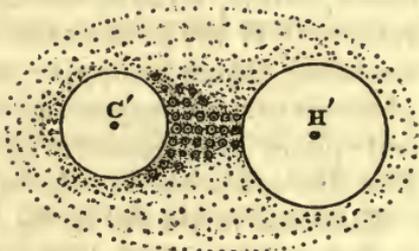


Fig. 4.

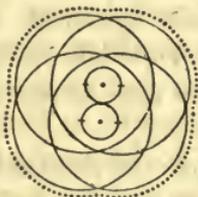


Fig. 5.

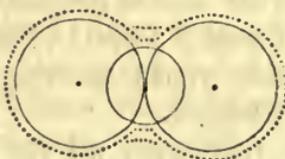
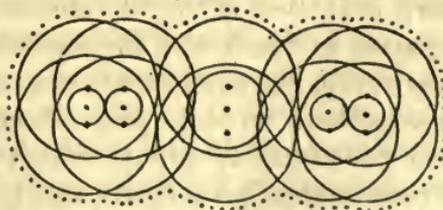


Fig. 6.



A cohesive combination is represented in fig. 3, one in single groups in fig. 4, and one in double groups in fig. 5,

and also in fig. 6. A mixture of hydrogen and chlorine in equal parts is represented in fig. 2, the chlorine C having a dense stratum of electric atoms, and H only a few of these; the circles denote limits of the spheres of repulsion which may be more or less; the absolute force of C is 36 times greater than that of H, but probably its sphere of repulsion is less; the atmospherule of C will be more dense, but the two will occupy equal spaces in the gas. While the gas is perfectly still, and not acted on from external causes, the mixture will continue a long time; but, the equilibrium of the electric atoms not being a stable one, when caloric, or light, &c., has access, the effect will be to bring the electric atoms between C and H, (prop. 1,) and the accumulation of part of them there will increase the effect on the rest; these electric atoms will then form an intermediate link, and hold C and H under one common atmospherule, forming muriatic acid as in fig. 3, where the centres C' H' are at the same distance as before. I believe no other theory can give any rational account of these phenomena. By supposing variations in the absolute force and sphere of repulsion, fig. 3 may represent any of the first five compounds in the table.

Etherine is shewn in fig. 4; there are two atoms of carbon with small, and four of hydrogen with large spheres of repulsion, the six centres being within the sphere of repulsion of the hydrogen, the whole occupies but one volume (prop. 3. cors). This is isomeric with olefiant gas, whose atom would be represented by one of carbon and two of hydrogen.

Fig. 5 represents all the compounds in the table, from number 26 to 33 inclusive, by supposing only an alteration in the absolute force and sphere of repulsion. Fig. 6 represents ether, it is two of fig. 4, connected by fig. 5. The connecting atom in fig. 5, and the connecting group in fig. 6, while they retain the whole under one atmospherule, and bind them together, do not (prop. 3. cors.) alter the distance between the atoms or groups which they connect. In the original communication a figure was given for every compound in the table.

A Table Containing the Three Varieties of Chemical Compounds.

I.—COHESIVE COMBINATIONS.

Name.	Elementary Atoms.		Wt of comp. atom.	Vol	Sp. gr. hyd = 1.	Sp. gr. air = 1.		Authority.
	Number.	Weight.				By Cal.	By Exper.	
1. Carbonic oxide	C + O	12 + 16	28	2	14	.972	.973	Thenard.
2. Nitric oxide	N + O	16 + 16	30	2	15	1.041	1.037	Ditto.
3. Muriatic acid	Cl + H	36 + 1	37	2	18½	1.284	1.248	Biot & Arago
4. Hydrobromic acid	Br + H	80 + 1	81	2	40½	2.812	2.731	Turner.
5. Hydriodic acid	I + H	126 + 1	127	2	63½	4.409	4.443	Gay Lussac.
6. Bisulphuret of mercury	S + 2 M	32 + 200	232	3	77⅓	5.370	5.384	Dumas.
7. Common air	O + 4 N	16 + 56	72	5	14½	1.	1.	The assumed unit.

II.—CHEMICAL COMBINATIONS IN SINGLE GROUPS.

Name.	Elementary Atoms.		Wt of comp. atom.	Vol	Sp. gr. hyd = 1.	Sp. gr. air = 1.		Authority.
	Number.	Weight.				By Cal.	By Exper.	
8. Cyanogen	N + C	14 + 12	26	1	26	1.805	1.806	Gay Lussac.
9. Dichloride of sulphur	S + Cl	32 + 36	68	1	68	4.722	4.70	Dumas.
10. Fluoboric acid	F + B	18 + 16	34	1	34	2.361	2.360	Davy.
11. Biniodide of mercury	I + Hg	126 + 100	226	1	226	15.694	15.67	Mitscherlich.
12. Bichloride of mercury	Cl + Hg	36 + 100	136	1	136	9.444	9.440	Ditto.
13. Bibromide of mercury	Br + Hg	80 + 100	180	1	180	12.500	12.360	Ditto.
14. Olefiant gas	C + 2 H	12 + 2	14	1	14	.972	.978	Henry.
15. Fluosilicic acid	Si + 2 F	16 + 36	52	1	52	3.611	3.600	Thomson.
16. Chloride of silicon	Si + 2 Cl	16 + 72	88	1	88	6.111	5.939	Dumas.
17. Nitrous acid	N + 2 O	14 + 32	46	1	46	3.194	3.177	Gay Lussac.
18. Hydrocarburet of chlorine	Cl + (C + 2 H)	36 + 14	50	1	50	3.472	3.443	Ditto.
19. Etherine	2 C + 4 H	24 + 4	28	1	28	1.944	1.91	Faraday.
20. Bicarburet of hydrogen	3 C + 3 H	36 + 3	39	1	39	2.708	2.776	Ditto.
21. Naphtha	3 C + 5 H	36 + 5	41	1	41	2.847	2.833	Saussure.
22. Naphthaline	5 C + 4 H	60 + 4	64	1	64	4.444	4.528	Dumas.
23. Camphene	5 C + 8 H	60 + 8	68	1	68	4.722	4.767	Ditto.
24. Oil of turpentn.	6 C + 8 H	72 + 8	80	1	80	5.555	5.013	Gay Lussac.
25. Arsenious acid	4 As + 3 O	152 + 48	200	1	200	13.888	13.67	Dumas.

## III.—CHEMICAL COMBINATIONS IN DOUBLE GROUPS.

Name.	Elementary Atoms.		Wt of comp. atom.	Vol	Sp. gr. hyd = 1	Sp. gr. air = 1		Authority.
	Number.	Weight.				By Cal.	By Exper.	
26. Water	O + 2 H	16+2	18	2	9	.625	.628	Gay Lussac.
27. Sulphuretted hydrogen	S + 2 H	32+2	34	2	17	1.180	1.19	Thenard.
28. Carbonic acid	C + 2 O	12+32	44	2	22	1.527	1.519	Ditto.
29. Sulphur. acid	S + 2 O	32+32	64	2	32	2.222	2.255	Berzelius.
30. Chloride of sul.	S + 2 Cl	32+72	104	2	52	3.611	3.67	Dumas.
31. Nitrous oxide	O + 2 N	16+28	44	2	22	1.527	1.522	Thenard.
32. Bisulphuret of carbon	C + 2 S	12+64	76	2	38	2.638	2.644	Gay Lussac.
33. Boro-chloric acid	B + 2 Cl	16+72	88	2	44	3.055	3.942	Dumas.
34. Deutoxide of chlorine	Cl + 2 O	36+32	68	2	34	2.361	2.346	Thenard.
35. Protochloride of mercury	Cl + 2 Hg	36+200	236	2	118	8.194	8.20	Mitscherlich.
36. Bromide of mer.	Br + 2 Hg	80+200	280	2	140	9.722	9.665	Ditto.
37. Hydrocyanic acid	H + (N + C)	1+26	27	2	13½	.937	.947	Gay Lussac.
38. Chlorocyanic acid	Cl + (N + C)	36+26	62	2	31	2.152	2.153	Dumas.
39. Ammonia	N + 3 H	14+3	17	2	8½	.590	.597	Biot & Arago.
40. Sulphuric acid	S + 3 O	32+48	80	2	40	2.777	3.000	Mitscherlich.
41. Inflam. gas of Dr. Thomson.*	C+2H)+3Cl	14+108	122	2	61	4.236	4.175	Thomson.
42. Phosphoretted hydrogen	2 P + 3 H	32+3	35	2	17½	1.215	1.214	Dumas.
43. Arsenuretted hydrogen	2 As + 3 H	76+3	79	2	39½	2.743	2.695	Ditto.
44. Chloride of phosphorus	2 P + 3 Cl	32+108	140	2	70	4.861	4.875	Ditto.
45. Chloride of arsenic	2 As + 3 Cl	76+108	184	2	92	6.388	6.300	Ditto.
46. Perchloride of tin	Tn + 4 Cl	116+144	260	2	130	9.027	9.199	Ditto.
47. Light carbu- retted hyd.	C + 4 H	12+4	16	2	8	.555	.559	Henry.
48. Perchloride of titanium	Ti + 4 Cl	52+144	196	2	98	6.805	6.856	Dumas.
49. Perphosphu- retted hyd.	3 P + 3 H	48+3	51	2	25½	1.770	1.761	Ditto.
50. Alcohol	Aq + 2 olef.	18+28	46	2	23	1.597	1.613	Gay Lussac.
51. Oil gas	3 C + 6 H	36+6	42	2	21	1.458	1.458	Henry.
52. Ether	Aq + 2 eth.	18+56	74	2	37	2.569	2.586	Gay Lussac.
53. Muriatic ether	M+Cl+2olef	37+28	65	2	32½	2.256	2.219	Thenard.
54. Hydriodic ether	H+I+2 olef.	127+28	155	2	77½	5.381	5.474	Gay Lussac.
55. Citrene	5 C + 8 H	60+8	68	2	34	2.361	2.383	Dumas.
56. Paranapthaline	15 C + 12 H	180+12	192	2	96	6.666	6.741	Ditto.
57. Chloro-carbonic acid.	2 Cl+(O+C)	72+28	100	2	50	3.472	3.472	Henry.

\* Sesquichloride of carbydrogen, a compound which Dumas, as too often happens at present, *re-discovered* 11 years subsequent to Dr. Thomson, and has termed its basis mothylene, instead of carbydrogen.

In attending a little to the preceding table, we observe that there are combinations of 2, 3, 4, 6, 7, 8, 9, 13 and 14 atoms or volumes, occupying exactly the space or volume originally occupied by one of them : this theory shows how this can be, but it will not be pretended that any other theory can show the manner of the composition ; so many solids cannot possess the space of one of them. The circumstances are still more remarkable in double groups : in etherine 15 volumes make exactly two ; in alcohol 9 volumes make two ; in camphene 13 volumes, and in paranaphthaline 27 volumes make exactly two ; and in these apparently complex cases, there is the same simplicity of composition as in that of water ; this is seen in fig. 5 which represents water, and in fig. 6 which represents ether.

THOMAS EXLEY.

*Bristol, September 7th, 1836.*

It may be added, that this theory was not formed in reference to chemistry, or other science, but electricity. It occurred to the author while attempting to explain electrical phenomena ; and it was not till he applied it with success to explain electrical attraction and repulsion, that he determined to publish any thing on the subject. He soon found that these principles were sufficient to solve all the phenomena he could collect relating to common electricity. Those of galvanism yielded with the same facility when relating to the excitation, the current, the quantity, the intensity, the decomposition of bodies, or the transfer of elements, &c. In magnetism he scarcely expected to overcome the difficulties of the subject ; his only hope was, that he knew his principles were correct ; and he soon ascertained, that every phenomenon in magnetism, whether relating to that of the earth, the motion of its poles, &c., or to that of magnetic bodies, communication of magnetism, &c. yielded to the influence of his two simple principles, which, also, with great facility applied equally to the phenomena of electro-magnetism.

When he published his first treatise, or "New Theory of Physics," he had scarcely turned his attention to the modern discoveries in optics, such as polarization, &c. ; and, of course, could not see the application to these subjects.

But, after studying the phenomena, he found the same principles equally available, even so much so, that they embraced all that is true, both in the theory of Newton and that of Huygens, for they include the undulatory theory when stripped of its redundancies and deficiencies. In consequence of this, the author published his "Physical Optics,"—only a few copies of these works remain in the author's hands. When they were published, he was not in possession of the demonstrations contained in this paper. He submits, in a special manner, that of the 2nd prop. to the notice of the mathematician; if this maintain its ground, the theory cannot be shaken. The author is persuaded that it is impregnable. The present application of the principles to chemistry is a farther confirmation of its truth, if such were needed; for in this short paper important phenomena are clearly explained, which the combined efforts of all the philosophers of Europe, during the last and the present century, have not been able to solve or unfold.

THOMAS EXLEY.

Bristol, September 7th, 1836.

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#### ARTICLE IV.

##### *A Theory of Accidental and Complementary Colours.*

By CHARLES TOMLINSON, ESQ.

(Concluded from p. 217.)

##### ON COLOURED SHADOWS.

17. Dr. Thomas Young, in his lectures on Natural Philosophy, vol. i., p. 456, states that "when the shadows of objects are placed in coloured light, the shadow appears of a colour opposite to that of the stronger light, even when it is in reality illuminated by a fainter light of the same colour." This he explains on the assumption that the eye cannot perfectly distinguish the intensity of a colour, either when the light is extremely faint or excessively vivid. This explanation, if such it be, I cannot adopt; because, I think it entirely inadequate to account for the phenomena of coloured shadows.

18. Independently of the theory of coloured shadows, there is an important principle established by their investigation, and admitted by Dr. Young, viz., *that every shadow*

becomes complementary in colour to the light into which it falls. I am not aware that this proposition is in any way exceptionable, for although shadows cast by the sun and by strong artificial lights are said to be black, and in some cases they certainly appear so, it is because our usual criterion is simple and imperfect, namely contrast. The unaccustomed eye is wont to view a shadow solely with reference to the surrounding light, and, as decreasing light conveys more or less to the mind impressions of obscurity or darkness, the depth of every shadow of course depends upon the greater or less absence of surrounding light. So far Dr. Young's remark applies, but it explains nothing of the principle that a shadow falling into coloured light assumes an opposite tint. This principle has been long recognized in practice, for the shadows in the pictures of the old masters are never black, but are variously coloured as circumstances or rather as Nature requires. The only black shadow that we ought I think to conceive is that cast by perfectly white light, passing through a perfectly transparent, colourless medium, and falling upon white ground; the shadow, in such a case, were it possible to obtain such perfection of observation, would probably be not black but grayish, from the admixture of a small quantity of white light with the black of the shadow. The solar rays passing through the blue æther acquire a yellowish tinge, and their shadows are generally blue of indigo, unless intercepted by the splendidly tinted clouds of morn or eve, which transmit light of their own peculiar colours, and afford shadows of opposite tints to themselves, and these colours vary in intensity and hue as the altitude of the sun varies, and are again modified by the colour of the ground upon which they fall. The effect of early morn is, as artists term it, *cold*, when the tints and shades gradually merge from gray and pass through various admixtures of yellow, green and blue, into indigo; which latter, during the day deepens into an apparent black, until evening brings its *warm* effects; the yellow merges into orange from the abundance of red rays, and the red, dark purple and indigo, with their oppositely tinted shades always modified by the ground upon which they fall, contribute to impart that richness or mellowness to the land-

scape, the successful imitation of which constitutes good painting. As the shadows of evening lengthen, they will generally be found to be indigo, if received upon white ground. Twilight, assisted by moderate artificial light, is a very favourable time for observing the coloured shadows and opposite tints of nature. By night the shadows cast by artificial lights are seldom capable of being estimated as to colour on account of their intensity, but by twilight they are distinctly seen to be either blue or indigo, and sometimes purple. By the yellow light of a lamp just after sunset, the sky, as seen through the window, is of a very rich indigo: if a sheet of white paper be so arranged as to receive the light of the lamp, and at the same time the natural light from the window, and the finger or other object held so as to throw a shadow upon the paper, two shadows will be obtained, one of bright yellow and the other of a rich indigo: this is a simple and familiar instance of coloured shadows and admits of easy explanation. The paper is illuminated by the yellow rays of the lamp and at the same time by indigo rays from the sky: the former by their intensity and abundance overcome the latter and the paper appears yellow; a shadow cast by the lamp deprives that portion of the paper occupied by such shadow of the yellow rays; the shadow is therefore illumined by the indigo rays of the sky. A converse arrangement will account for the appearance of the yellow shadow, which is cast not by the lamp but by the natural light which falls upon the ground, illuminated by artificial yellow light and natural indigo light, except where the yellow shadow appears, for there the indigo is intercepted by the opaque object.

19. The means of obtaining coloured light are various, and may be arranged into four classes; thus,

*Class 1st.* The colours of the spectrum.

„ *2nd.* Transparent coloured media.

„ *3rd.* Coloured flames.

„ *4th.* Light reflected from polished coloured surfaces.

20. *Class 1st.* On receiving a large and well defined spectrum upon a white screen, or upon the white ceiling of a room, and intercepting a portion of each of the colorific rays in succession, by means of a long narrow strip of white

card, or a similar object, the resulting shadow will be complementary to the colour of that part of the spectrum whereon the shadow falls, mingled, however, with a small portion of natural light which imparts a grayish hue to the colour of the shadow: but the result is decided and conclusive.

The most refrangible colours are best adapted to the reception of the shadow, as the illuminating power is smaller, and the space occupied by the colour larger, so that the contrast between the fundamental and complementary colours is more readily observed. In the less refrangible colours, the illuminating colour being great, and the space occupied comparatively small, the shadows appear very dark, and sometimes too deep for correct observation of their colour. If the spectrum then be too vivid, it, or portions of it, in succession, may be received upon a plane mirror, and so reflected upon the ceiling, or upon a white screen. The colours become less vivid by reflection, and the shadows consequently more appreciable.

21. *Class 2nd.* Coloured glass of every shade and depth of shade, provided light can be transmitted through it, may be employed. If a coloured disk be held before a lamp, or any strong source of light, and the coloured light fall upon white paper, a shadow formed by intercepting a portion of the light will be complementary to the colour of the disk. Coloured solutions contained in an oblong glass frame may be employed, and even vapours such as chlorine, iodine, nitrous acid, &c.

The stains of the glass, must not, however, be too light, because, as shadows are judged of by contrast, the complementary shadow will appear too dark for the observation of its colour: but this may be remedied by employing two or three thicknesses of glass. Interesting results may be obtained by transmitting light through pairs of disks of different colours,—the accidental colour of each being, of course, modified or changed by such means: thus, green glass transmits green light, a shadow falling into it is pink. The same pink shadow may be produced by passing a light through a yellow and blue disk placed the one upon the other; and so on.

One of my methods of exhibiting coloured shadows to an

audience is as follows : my lecture room is illuminated by a branch of two gas lights. A large screen is placed six or seven feet from, and parallel to the gas lights. One of the gas lights is provided with a small wire frame fitted for the reception of a piece of stained glass; the screen is thus illuminated by the coloured light, and also by the other burner—two separate shadows of the body, or any opaque object, are therefore cast, the one being complementary to the other.\*

In this class may also be included coloured silks, moreens, cottons, &c., provided they are sufficiently thin, (or the illuminating object sufficiently intense) to transmit the light either of the sun or of an argand lamp. Windows provided with red moreen curtains present favourable opportunities for observation : if the sun be shining on the window, and these curtains be drawn, a quantity of red light will illuminate the ceiling, and a shadow falling upon it will be green. So also in Mons. Meusnier's experiment, where the sun shone through a hole in a *red* curtain, the image of the luminous spot was *green*. If green, blue, yellow, &c. curtains be employed, the spots will be red, orange, indigo, &c. A disk of coloured glass with a hole in the centre is an interesting and useful article for experiments of this kind, the glass being easily pierced by means of a common awl kept well moistened with oil of turpentine.

22. *Class 3rd.* The flames employed have been the white, red, green, blue, purple and yellow fires, and signal lights; as also the less vivid, but equally efficacious, flames obtained by solutions of the various salts in alcohol. In most cases, the colours of the flames are not homogeneous, and the complementary shadows are modified more or less, by the presence of other colorific rays; but where one colour is in considerable excess, there is no difficulty in procuring the complementary shadows. I have adopted a very useful mode of testing the true colour of the shadow,

\* The advantage of this arrangement is, that the shadows are of a size sufficient for a large audience to judge of and appreciate. It is obvious, that a similar arrangement can be adopted with two lamps, or two candles. There must be *two* sources of light, one of which is to afford coloured light, and the other to illuminate the coloured shadow.

by receiving it first upon white paper, and then upon coloured paper; as, for example, boracic acid in alcohol yields a fine light green flame, the shadow is pink on white ground, violet on blue ground, orange on yellow, &c. If a sheet of white paper be held before the red light from a coke fire, and the hand be placed between the paper and the fire, the resulting shadow will be green. On a dull day, or just after sunset, a coke fire or the red part of a coal fire throws green shadows in various parts of the room. A white screen placed parallel with the window, the fire being between, is an excellent ground for the reception of the shadows. Chairs, or any opaque objects, the shadows of which are cast by the fire upon the screen, are strongly green. This, and most of the observations with coloured flames are most conveniently made immediately after sunset, or in a somewhat obscure room, or part of a room, as the abundant presence of day light prevents the shadows being appreciated. A little flat dish, containing the salt in spirit, should be placed on white ground, and the shadow of the dish will be complementary in colour to that of the flame. Or, a sheet of white paper may be held between the eye of the observer and the flame, and the hand or fingers between the flame and the white paper. If very vivid flames be required, equal parts of chlorate of potassa and white sugar may be employed mingled with one part of a salt which communicates colour to the flame, such as the muriates of barytes, strontia, soda, lime, &c. The ingredients should be well dried, and intimately mixed in a mortar. A drop or two of sulphuric acid will fire the mixture. This mode of impressing the eye is a pleasing one. I have found the spectra to be very different with different persons, and the impression on the retina lasts from two to three minutes. Flames may also be obtained from gases, such as carbonic oxide, equal parts of hydrogen and binoxide of nitrogen, cyanogen, &c. The purple flame of potassium upon water may be employed, and if the metal be thrown into a jar of carbonic acid standing over water, it will yield a fine ruby, which is almost homogeneous.

23. *Class 4th.* When a polished coloured surface is presented in an inclined position to the light, so that the coloured rays proceeding from the surface be reflected upon

white paper, a shadow formed by intercepting a portion of the coloured reflected light, will be the accidental colour of that light. Surfaces of burnished gold, polished copper, silver, steel, brass, &c., are instances, as also bright coloured varnished papers. These methods are, however, imperfect.

24. From what has been already stated of my theory, its application to coloured shadows will be readily perceived. If white ground be illuminated by coloured light, natural light being also present, a shadow introduced occupies a portion of the white ground, which would otherwise be coloured, and we see the shadow white, with the exception of the colour which illuminates the ground. Thus, if the source of coloured light be a green flame, and a sheet of paper be placed so that it be illuminated by the green flame, and at the same time by light from a second source independent of the first, an opaque object placed between the flame and the paper intercepts a portion of the former, and restores that part of the paper occupied by the shadow to its original whiteness; but this shadow which would be white is surrounded by green, which, according to the principle of homo-chromatic attraction, absorbs green from the white shadow, and red, therefore, alone remains.

25. Coloured shadows cannot be produced, unless other light be present than the coloured light which illuminates the paper. Thus, when Brewster's mono-chromatic lamp is employed in an apartment where there are no other sources of light, no accidental shadows are obtained. The pale blue or indigo flame afforded by pure carbonic oxide may be made to illuminate a sheet of white paper, but if the room be deprived of all other light, it is in vain that we attempt to produce the accidental shadow. These facts eminently support, I think, the theory I have propounded. A sheet of white paper is no longer *white* if illuminated only by *coloured* light: the paper must evidently reflect rays to the eye identical with those whence it receives light, which is, we suppose, in this case, a coloured flame. A shadow formed upon the paper, by intercepting a portion of the light of such flame, is evidently a deprivation of light and nothing more. Hence, such a shadow, if not black, is, at least, very dark, and can have no colour. If now we

admit to the paper light from a second source, not coloured, the paper will be again in a condition to reflect white light, were it not that the artificial coloured light overpowers the light from the second source. A shadow formed by intercepting a portion of the coloured light upon white ground was black, in the absence of light from the second source now becomes white from the presence of the latter; or rather, that portion of the ground occupied by the shadow, is restored to its original whiteness. Homo-chromatic attraction proceeds between the colours of the same kind in the shadow and the coloured ground which encompasses it; while the colour or colours of an unlike kind are not attracted but repelled or reflected, and constitute the accidental impression.

26. The conditions, therefore, necessary to the production of coloured shadows are,

1st. That there be two sources of light.

2nd. That the first source of light be coloured.

3rd. That the second source of light be not coloured, and not more than equal in intensity to the first.

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27. Most of the experiments in this and the preceding papers are so decided and are performed so easily and so readily, that it cannot be urged that "the diminished sensibility of the eye from fatigue," or from any other cause, produces the effects. I deny that it has any thing to do with the production of these effects, for, I believe, that the performance of any one of the experiments contained in these papers does not occupy more than a second of time. I agree with Mr. Cooper, that "the colours are produced with a facility which those who are not familiar with the subject are not prepared to expect." (*Records*, vol. ii, p. 178.) Mr. Cooper, also, in making an experiment in order to determine the shortest time in which an accidental colour could be obtained, found it to occupy less than a second. Mr. Cooper also says, "we cannot attribute the appearance of accidental colours to insensibility of the eye arising from fatigue or exhaustion. Brewster also (*Optics*, p. 309) admits that a new theory of accidental colours is requisite to explain the phenomena of coloured shadows. There are no less than nine distinct theories of accidental

colours, and if I include the theories which seek to explain coloured shadows, &c., the number will be nearly doubled. It unfortunately happens, too, that each class of facts has its own theory or theories attached to it, and the great fault of all, including M. Plateau's, is the want both of simplicity and comprehensiveness. If the theory that I propose has any merit it is that of being very simple, and at the same time it comprehends all existing facts, as far as I am aware, (except those facts which belong to and are explained by polarization,) for the more these various phenomena are studied, the stronger, I am sure, will be the conviction that they are all due to one cause, or analogous causes, and must and ought to be explained by one theory.

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28. In these two papers I have assumed that white light is composed of red, yellow and blue, one or more of which colours, by itself, or by combination, in various *definite* proportions, produce all the ordinary effects of colour.

I have endeavoured to establish the following propositions:

1st. That two colours are complementary to each other, when, from a combination by direct impression, *white* is produced; and by superposition, black.

Thus, if a disk be divided into two parts, one painted red and the other green, and the disk be rapidly rotated, the colours will combine and produce white. This is an instance of combination from direct impression.

If we view red lead through a disk of green glass the red powder will appear as black as lamp black. This is an instance of combination by superposition.

2nd. That if the fundamental colour be simple, the complementary colour is compound; and if the complementary colour is simple, then the primary or fundamental colour is compound; but in certain cases both the fundamental and complementary colours are compound, but never simple. (See table 6.)

3rd. That white light in passing through coloured transparent media is decomposed, the second surface of such media reflecting rays of the same colour as the medium itself, and the first surface reflecting the other rays, which,

combined with those of the second surface, produce white light.

4th. That an attraction and repulsion exists between colours. Colours of a like kind attract each other, and those of an unlike kind repel each other.

Thus, when a coloured object is placed on a white ground, the colour of such object attracts the rays of the same colour as itself from the contiguous white ground, and leaves a portion of the latter of the complementary tint.

5th. Hence, the theory of accidental and complementary colours, which may be included in the following proposition, viz.:

*That a coloured body has the power of decomposing white light; of attracting and retaining rays of a like kind to itself; and the other rays, which it repels, constitute the accidental or complementary impression.*

CHARLES TOMLINSON.

*Brown Street, Salisbury,  
December, 1835.*

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ARTICLE V.

*Economical Mode of forming Hyper-manganate of Potash.*

By WILLIAM GREGORY, M. D., F. R. S. E., Lecturer on Chemistry, Edinburgh.

*(To the Editor of the Records of General Science.)*

SIR,—Observing in the Records of General Science, for September, 1836, part of a memoir by Professor Mitscherlich, on the Manganic and Hyper-manganic Acids and their Salts, it has occurred to me, that your readers may like to be acquainted with the following easy and economical method of obtaining the most remarkable of these compounds, the hyper-manganate of potash.

My process is a modification of that of Wöhler, who recommends to melt chlorate of potash along with caustic potash in a platinum crucible, and to add peroxide of manganese to the fused mass.

There are several objections to this process. In the first place, the melted mass froths up violently when the last portions of oxygen escape, so that we can only employ a small quantity of materials, even in a pretty large crucible.

2ndly. I find that with less than 1 atom of chlorate to 1 of each of the other ingredients, the mass cannot be kept fused, and, consequently, the mixture is imperfect. 3rdly. From the large proportion of chlorate employed, a corresponding quantity of chloride of potassium is left, which interferes with the subsequent purification of the hypermanganate of potash; and, lastly, since 1 atom of chlorate of potash loses 6 of oxygen by heat, while 1 of binoxide of manganese requires only 1 of oxygen to convert it into manganic acid (the change which occurs in this stage of the process,) we lose  $\frac{5}{6}$ ths of the oxygen.

After many trials, I found the following process to answer remarkably well.

Take of binoxide of manganese, 132 parts (3 atoms) of fused potash 147 parts (3 atoms), chlorate of potash 124 parts (1 atom). Dissolve the potash in a very small quantity of water, and add to the solution the oxide and the salt, previously in fine powder. Mix intimately so as to form a thin paste, which dry up and pulverize finely. Introduce the powder into a platinum crucible, (which may be filled, as there is neither melting nor frothing), and expose the whole for half an hour to a very low red heat. By this the production of the green manganate of potash which had taken place to a considerable extent during the exsiccation, is completed; while any hyper-chlorate of potash which may have been formed is destroyed.

The green mass, a mixture of manganate of potash and chloride of potassium, is easily detached from the crucible. It is to be dissolved in a very large quantity of hot water, and when the solution has acquired a pure red colour, it is to be decanted from the hydrated binoxide, the formation of which accompanies the change of manganate into hypermanganate of potash. The clear solution, evaporated rapidly until crystals appear, deposits on cooling a number of small crystals nearly black. These are to be washed with a little cold water, dissolved in a small quantity of hot water, and this solution, on cooling, yields crystals of the hypermanganate of potash, chemically pure, and of ten  $\frac{3}{4}$  of an inch long. They have a fine bronze colour, and metallic lustre, and their solution in water possesses the most superb purple colour. I have always obtained, by

the above process, a quantity of crystals equal in weight to one-third of the oxide employed.

The mother liquids, on the addition of sugar, yield a large quantity of hydrated peroxide, which, with that separated by decantation, is very well adapted for a new operation.

In this process, while the chlorate is economized, and the quantity of hyper-manganate increased, that of the chloride of potassium is diminished, and only half of the oxygen is lost.

I have no doubt, that, if the green mass be dissolved in a small quantity of cold water, and the solution evaporated in vacuo, the green manganate of potash may be obtained with equal facility.

Where it is necessary to filter these solutions, as paper cannot be employed, I am in the habit of using a funnel, the throat of which is stopped with Asbestos, which answers the purpose perfectly.

I have the honour to be,

Your obedient Servant,

WILLIAM GREGORY.

10, Ainslie Place, Edinburgh,  
September, 13th, 1836.

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#### ARTICLE VI.

*Analysis of Tartar Emetic.* By Mr. THOMAS RICHARDSON.  
MR. PHILLIP'S analysis of this salt differing from Dr. Thomson's in the proportion of the water it contains, I was induced to repeat the experiments of the latter gentleman, employing the same specimen.

The analysis was conducted in the following manner :

1. 25 grs. were heated for a considerable time, on the sand bath at the temperature of about 400° Fah. and lost 1·21 grs. or 4·84 per cent.

2. What remained was dissolved in water, and a current of sulphuretted hydrogen passed through the solution till all the antimony was thrown down. The precipitate, after being well washed and dried, weighed 13·3 grains. But 11 sesquisulphuret of antimony : 8 antimony :: 13·3 : 9·67 grs. = 11·48 grs. oxide of antimony.

3. The liquid and washings from the above precipitate

were carefully evaporated to dryness, and the residual salt weighed 13·23 grs. Knowing the composition of this salt or Bitartrate of Potash, we obtain the following result :

Oxide of antimony . . . . .	45·92
Potash . . . . .	12·80
Tartaric acid . . . . .	35·25
Water . . . . .	4·84
	98·81

Resolving these weights into atoms we have for the constituents of the salt,

Oxide of antimony . . . . .	4·83 = 2·26 atoms.
Potash . . . . .	2·13 = 1·00 ,,
Tartaric acid . . . . .	4·27 = 2·00 ,,
Water . . . . .	4·30 = 2·01 ,,

The result of Dr. Thomson's analysis was,

1·997 atom Tartaric acid.
1·92 atom protoxide of antimony.
1 atom potash.
2·139 atoms water.

The mean of the two analyses as below,

Oxide of antimony . . . . .	2·12
Potash . . . . .	1·00
Tartaric acid . . . . .	1·96
Water . . . . .	2·07

leaves no doubt as to the following being the true composition of the salt :

2 atoms oxide of antimony . . . . .	19·00
1 atom potash . . . . .	6·00
2 atoms Tartaric acid . . . . .	16·50
2 atoms water . . . . .	2·25
	43·75

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#### ARTICLE VII.

*Catalogue of Plants Collected at Bombay.* By JOHN  
GRAHAM, ESQ.

(Concluded from page 198.)

309. *Moræa chinensis.*  
310. *Mentha perilloides*  
311. *Marsilea 4-folia*

312. *Morus Indica*.  
 313. *Milhavia tomentosa*. In gardens only.  
 314. *Mesembryanthemum*.? Ditto.  
 315. *Nyctanthes Arbor tristis*.  
 316. *Nicotiana Tabacum*.  
 317. *Nerium Oleander*.  
 318. „ *coronarium*.  
 319. „ *coçcineum*. Rare. } All cultivated as orna-  
 320. „ *antidysentericum*. } mental plants.  
 321. „ *tinctorium*.  
 322. *Nymphaea lotus*.  
 323. *Nelumbium speciosum*.  
 324. *Nauclea orientalis*.  
 325. *Oryza sativa*. Common rice.  
 326. *Ocimum sanctum*. Planted at temples.  
 327. *Ochna lucida*.  
 328. *Piper nigrum*. In gardens.  
 329. *Pladua virgata*.  
 330. *Plumbago rosea*.  
 331. „ *Zeylonica*.  
 332. *Physalis angulata*.  
 333. *Plumeria acuminata*.  
 334. *Periploca esculenta*. A very pretty twining plant ;  
 flowers during the rains.  
 335. *Perilla ocymoides*.  
 336. *Polyanthus tuberosa*. Cultivated in gardens ; worn  
 by native women in their hair.  
 337. *Parkinsonia aculeata*. In gardens.  
 338. *Poinciana pulcherrima*. Common in gardens. It  
 grows in abundance close to the caves of Ellora, near  
 Aurungabad, but I suppose it has all been planted.  
 339. *Portulaca oleracea*.  
 340. *Psidium pyrifera*. Grown in gardens.  
 341. *Punica Granatum*. Ditto.  
 342. *Premna integrifolia*.  
 343. *Phlomis Indica*.  
 344. *Pedaliium Murex*.  
 345. *Passiflora fetida*.  
 346. „ *laurifolia*. } In gardens.  
 347. „ *minima*. }  
 348. „ *alata-cærulea*. }

349. *Pistia Stratiotes*.  
 350. *Pentapetes phoenicea*. In gardens.  
 351. *Pterospermum acerifolium*.  
 352. *Phaseolus Mungo*.  
 353. *Polygonum glabrum*.  
 354. *Phyllanthus bacciformis*  
 355. *Pandanus odoratissimus*.  
 356. *Prenanthes sarmentosus*.  
 357. *Quisqualis Indica*. In gardens.  
 358. *Rhizophora Mangla*.  
 359. *Rosa*.? Several species in gardens.  
 360. *Ricinus communis*.  
 361. *Ruellia Zeylonica*.  
 362. *Rottleria tinctoria*.  
 363. *Saccharum officinarum*. Cultivated.  
 364. *Smilax aspera*.  
 365. *Santalum album*.  
 366. *Solanum tuberosum*.  
 367. „ *lycopersicum*.  
 368. „ *melongina*.  
 369. „ *nigrum*.  
 370. „ *jacquini*.  
 371. *Sterculia colorata*.  
 372. „ *wrens*.  
 373. „ *foetida*. Poon tree. Grows to a great height in Malabar; masts are made of it.  
 374. *Sphæranthus Indicus*.  
 375. *Sansevcera Zeylonica*.  
 376. *Sapindus emarginatus*.  
 „ *tetraphyllus*.  
 377. *Spondias Amra*.  
 378. *Sesamum Indicum*.  
 379. *Sida populifolia*.  
 380. *Smithia sensitiva*.  
 381. *Spilanthes alba*.  
 382. *Salvadora persica*.  
 383. *Stemodia ruderalis*.  
 384. *Tectona grandis*. Teak tree.  
 385. *Tamarix Indica*.  
 386. *Turnera ulmifolia*. In gardens.  
 387. *Tradescantia discolor*. Ditto.

388. *Tradescantia cristata*.  
 389. *Triumfetta annua*.  
 390. *Thunbergia grandiflora*. In gardens.  
 391. *Tamarindus Indica*.  
 392. *Tagites patula*. In gardens; worn by native women in their hair.  
 393. *Trichosanthes Anguina*.  
 394. *Trophis aspera*.  
 395. *Terminalia Catappa*.  
 396. „ *alata*.  
 397. „ *Bellirica*.  
 398. *Tabernaemontana dichotoma*.  
 399. *Utricularia stellaris*.  
 400. *Ulmus integrifolia*. Salsette.  
 401. *Unona longifolia*.  
 402. *Vitis vinifera*. In gardens.  
 403. *Vitex trifolia*.  
 404. *Vernonia arborea*.  
 405. *Vernonia anthelmintica*.  
 406. *Verbena sativa*.  
 407. „ *dichotoma*.  
 408. *Viscum compressum*.  
 409. *Vangueria spinosa*.  
 410. „ *edulis*.  
 411. *Vitmannia elliptica*.  
 412. *Yucca gloriosa*.  
 413. *Zingiber officinale*.  
 414. *Ziziphus Jujuba*.  
 415. *Zinnia elegans*. In gardens only.  
 416. *Zea Mays*. Indian corn; extensively cultivated.  
 417. *Zapania nodiflora*.

## ARTICLE VIII.

## SCIENTIFIC INTELLIGENCE, &amp;c.

I.—*British Association for the Advancement of Science*.\*

## SECTION A.—MATHEMATICS AND PHYSICS.

*President*, Rev. W. Whewell.—*Vice Presidents*, Sir D. Brewster, Sir W. R. Hamilton.—*Secretaries*, Professor Forbes, W. S. Harris,

\* For these notices we are indebted to the Bristol Mirror, the Athenaeum, and other sources.

Esq., F. W. Jerrard, Esq.—*Committee*, C. Babbage, Esq., F.R.S., F. Baily, Esq., Professor James Challis, Mr. Chatfield, Professor Mc Cullagh, Robert W. Fox, Esq., William Friend, Esq., G. Jerrard, Esq., Professor Lloyd, J. W. Lubbock, Esq., Rev. Dr. Lloyd, Provost of Trinity College, Professor Moll, Rev. G. Peacock, Professor Rigaud, Professor Ritchie, John Robison, Esq., Professor Stevelly, H. F. Talbot, Esq., Professor Wheatstone.

*Monday, 22nd August.*—1. *Report on a Rock Salt Lens.*—*Sir D. Brewster* stated, that he had no regular report. Through the kindness of Dr. Trail he had obtained from Cheshire some large masses of rock salt, singularly transparent and homogeneous, and of great purity, likely to afford a concave lens of considerable magnitude.

2. *Mr. Lubbock's Report of Recent Discussions of Tide Observations.*—From the discussion of the Liverpool tides, by Mr. Dessiou, he finds the diurnal inequality at Liverpool very considerable. The errors of prediction, at Liverpool and London, being classified, the result obtained by Mr. Dessiou confirmed the influence of atmospheric pressure on the tides. The law of the intervals, when the discussion is instituted with reference to the transit immediately preceding the time of high water, whether at London, Liverpool, or Brest, depends partly on the phenomena as deducible from Bernouilli's equilibrium theory, and partly upon the law of the intervals between the moon's successive transits. The general conclusion of Mr. Lubbock, from an adequate discussion of tide observations by Mr. Jones and Mr. Russell, is, that the equilibrium theory of Bernouilli satisfies the phenomena nearly, if not quite, within the limits of the errors of the observations, and that it leaves little, if any thing, to be accounted for otherwise.

*Mr. Whewell* observed, that it appeared from Mr. Bunt's calculations, that though the observed laws of the tide at Bristol might be made to agree with Bernouilli's theory of equilibrium tides, by referring them to a certain anterior transit, so far as the changes due to parallax are concerned; yet, that this anterior period is not the same for parallax as for declination; so that there is no one anterior period which gives theoretical tides agreeing with the observed tides; and, hence, the Bristol tides do not altogether, at present, appear to confirm the result obtained by Lubbock from the London tides.

3. *Mr. Whewell* reported respecting the Committee appointed to fix the relative level of the land and sea, with a view to determine its permanence. He observed, that the Committee had not taken any practical steps for the purpose for which they were appointed, having found difficulties which required consideration; but it was stated, that it was intended to prepare to re-appoint a Committee for this purpose, with instructions grounded upon the views at which the members of the Committee have arrived, namely: 1. To strike level lines for considerable distances along the land; as, for example, from Bristol to Ilfracombe, and from Bristol to Lyme Regis, with great accuracy: the permanence of these two lines (independently of reference to the sea) would determine the permanence of the relative points. 2. To refer the extremities of these lines to the sea at each extremity; the tides at the extremities being of any

different amount, the observations would decide whether a level line agrees with low water, mean water, or high water ; and, thus, what is the true level of the sea.

4. *Mr. Lubbock* then read a paper on the importance of forming new empirical tables for finding the moon's place. Although astronomical tables are sufficiently perfect for the general purposes of navigation, yet astronomers are not satisfied to rest here ; but desire to reach, by calculation and theory, a higher degree of accuracy, such as that obtained by the best instruments in fixed observatories. The most remarkable works on the theory of the Moon are, on account of their extent, those of M. M. Damoiseau and Plana. Those of M. Damoiseau's calculations are, however, in such a shape that it is almost impossible to verify them. M. Plana's work constitutes a new era, from the circumstance that the results are developed according to the powers of the eccentricities, inclinations, &c., as also of the quantity in denoting the ratio of the sun's mean motion to that of the moon ; otherwise the calculations are similar to those of M. Damoiseau and there exists, finally, a great difficulty, from the circumstance that the expressions for the co-efficients do not converge. Mr. Lubbock, from these and other considerations, suggests the importance of deducing the numerical values empirically from the best observations, and so construct new lunar tables, which may serve to check the results obtained by theory. This resolves itself into a question of expense merely, since there are plenty of persons to be found adequate to the task of computing the proposed new tables.

5. *Sir W. Hamilton* was then called upon to give an account of the result of Mr. G. B. Jerrard's process for resolving equations of the fifth and sixth degrees. Professor H. reported that he conceived himself to have proved that, in that particular process it had failed ; but it was only a particular case of a far more general method invented by Mr. Jerrard, with admirable mathematical skill, which is adequate to effect a very curious and unexpected transformation, or rather, class of transformations, on the general equations of the  $M^{\text{th}}$  degree, though it fails when that degree is below a certain minor limit ; and that, for this and other reasons, the researches of Mr. Jerrard are highly worthy the attention of all who interest themselves in the progress of algebra. It had not been found necessary to employ the grant voted in Dublin for an experimental discussion of the question.

6. *Mr. Phillips* offered a brief statement of the means taken by the Committee of the Association for the purpose of procuring regular and uniform experiments on subterranean temperature. The errors incidental to observations made in the air or water of mines had induced the Committee to recommend observers to attend simply to the temperature of the rocks themselves ; with this view 36 thermometers had been duly compared, and the errors of them ascertained ; many of these had been placed in secret situations, at the lead hills, by Professor Forbes ; at Newcastle, by Mr. Briddle ; at Wearmouth, by Mr. Anderson ; near Manchester and at Northampton, by Mr. Hodgkinson. Within a few days Professor Phillips

has found means of placing thermometers in a colliery at Bedminster, near Bristol. The results confirm the alleged increase of temperature beneath the surface. In one instance, the instrument stood at 78 degrees constantly, whilst the mean temperature of the air above was 47 degrees.

7. *Mr. Craig* read a paper on Polarization, with a view to show that the phenomena are referable to the division and consequently to the weakening of the impulse of light; and the inability, therefore, to pass through other regular structures without exhibiting phenomena which arise out of the peculiarities of such structures.

*Tuesday, 23rd August*—1. *Mr. Russell on the Phenomena of Waves.*—Mr. R. states that, in the course of his experiments on this interesting subject, he finds many different classes of waves obeying different laws. He has observed principally four classes; 1st, the simple ripple, which is not propagated beyond the point of generation; 2nd, oscillatory waves, such as are produced by a stone dropped into water; these are propagated with a velocity dependent on the magnitude of the displaced fluid; 3rd, waves having a broken top, called surges; 4th, the single, solitary wave, which is propagated with a nearly uniform velocity. The two last classes are those which had been principally investigated by Mr. Russell: 1st, it was observed that when any addition is made to a quiescent fluid, an elevation is propagated along its surface with a velocity equal to that which would be required by falling through half the depth of the fluid; 2nd, that the height of such wave is to be added to the depth, in order that the law may express accurately the velocity; 3rd, that the length of the wave is closely connected with the depth of the fluid; 4th, that it varies with the height; 5th, that when the height of the wave is nearly equal to the depth of the quiescent fluid, the wave breaks; 6th, by a canal of variable depth the deviation of surges or waves is explained, and also the formation of a tide bore; 7th, the phenomena of waves observed in a canal which gradually diminishes in breadth, are analogous to the phenomenon of the extremely high tides observed in narrow rivers and channels.

2. *Mr. Powell on Refractive Indices.*—The determination of the refractive indices for definite rays of the solar spectrum, marked by the dark lines, from the direct observation of their deviations produced by prisms of different substances, first proposed and executed by Fraunhofer, for ten media solid and fluid, was carried on by M. Rudberg for ten more cases. The necessity of an extended series of such determinations was pointed out by Sir J. Herschell and Sir D. Brewster, and was further urged by a special recommendation of the British Association. Mr. Powell, by a simple and most ingenious apparatus, has ascertained the refractive indices belonging to each of the standard primary rays for various media, which may be considered as a most valuable contribution to this branch of science.

3. *Sir David Brewster on the Polarizing Structure of the Crystalline Lens, of the eyes of Animals after Death.*—These inquiries, which form the subject of this paper, were made by com-

paring the changes which take place in the polarizing structure of the crystalline lenses of animals in old age, with those after death, the lenses being placed in distilled water, as being the only fluid which did not affect their transparency. From these investigations Sir D. Brewster has been led to conclude that there is in the crystalline lens a capability of being developed by the absorption of the aqueous humour; that a perfect structure is not produced until the animal frame is completely formed, and that, when it begins to decay, the lens changes its density and focal length, and sometimes degenerates into that state called hard and soft cataract. Sir D. Brewster is led to entertain a hope that these researches may furnish a means of preventing or curing this alarming disease.

4. *The Rev. Mr. M'Gauley* having been called on by the President to read a paper in continuation of the one which he gave last year, respecting the theory of the *Application of Electro-Magnetism to Mechanical Purposes*, began by stating that he had met with many practical difficulties, as might be expected, in preparing for the Section a small model of a machine, intended to act with effective power; one of the most serious of these was, that the crank and fly-wheel could not be made to move along with the primary moving pendulum. He then proceeded to give his views of the theory of electro-magnetic influence, and the best modes of constructing the several parts of the apparatus, so as to produce the greatest effect, and illustrated the whole by copious extracts from a most laborious course of experiments.

*Wednesday, 24th August.*—1. The business was commenced by *Mr. Harris*, "On some Phenomena of Electrical Repulsion." The author endeavoured to show in this paper that, from the disturbing force of electrical induction, the indications of electricity, operating by repulsion, are often anomalous and irregular, and do not, under all circumstances, indicate the quantity of electricity with which the repelling bodies, either one or both of them, are charged; he determined the nature of the cases in which the disturbing influences of induction may be supposed to arise. The author further gave the results of some inquiries on the nature of the tangent disc, and was led to believe that it may be greatly influenced by position on the charged body; intensity of the charge, thickness, or other extension, and the like, without any reference to electrical distribution on the body touched. The author conceived that the present theories did not account for the phenomena of electricity satisfactorily, and that we may eventually find electrical action reducible to a system of undulations set up in the finely attenuated medium between the surface of bodies.

2. *Professor Challis' Supplementary Report on the Mathematical Theory of Fluids.*—This report gives an account of the application of mathematics to problems in the equilibrium and motion of fluids, which had not been touched upon in the author's previous reports. These were principally the mathematical theory of elastic fluids, as bearing on the determination of the heights of mountains, and the barometer, and the amount of astronomical

refraction, and the theory on the determination of the velocity of sound. The bases on which these theories rest were stated, and a comparison made of the theoretical results with experiments.

3. *Professor Stevelly on the Interpretation of the Doubtful Sign in Certain Algebraical Formulæ.*—Mr. Stevelly stated that he had some years since been led to see the importance of a correct interpretation of the doubtful sign, in certain formulæ, in algebraic geometry, by observing that in the transformation of co-ordinates, it was requisite sometimes to use the positive sign for a perpendicular upon a plane, and sometimes the negative sign, in a manner which to him appeared to admit of no infallible rule to guide the choice. This induced him to consider the origin or meaning of the doubtful sign, and he found it to be the value of a perpendicular upon a line, given in position by its equation from a point given by its co-ordinates, the perpendicular in one position of the given line being assumed as a position; if you cause the line to revolve one half round in the plane of the axes of co-ordinates, when it arrives at its new position the same equation will again belong to it, but the law of continuity will now compel you to use the negative sign for the same perpendicular from the same point.

4. *Mr. M'Cullagh on the Laws of Double Refraction of Quartz.*—The object of the author was to show how the various phenomena presented by quartz in its action upon polarized light, which are altogether different from those of any other known crystal, and which had never been explained by any theory, may all be grouped together by means of a very simple mathematical hypothesis. Besides explaining all the laws already known, this hypothesis leads to a new and very remarkable one, which has been for some time a desideratum in optical science; and this new law enables us to connect together two classes of phenomena between which there was no connexion whatever previously, though experiments upon both had been made by different observers, M. Biot and Mr. Airy. The law is of such a nature that the experiment of Mr. Airy can be computed solely from the data furnished by those of M. Biot, and a very close agreement is to be found between the results of calculation and experiment.

5. *Mr. Addams* made a communication on the interference of sound. From the lateness of the hour, and having left home quite unprepared, he should merely explain the nature of a subject which, at a future meeting, he should be happy to go into at a greater length. He then proceeded to make some pleasing experiments with a tuning fork and a small glass tube, one end of which was closed. When the fork was held over the latter, the air propagated into it produced a sound which increased or diminished according to the distance between them. With two tubes, one placed horizontally the other perpendicularly, a curious phenomenon was observed: when the tuning fork was put in vibration in a certain position between the two unclosed ends of the tubes, no effect was observed, but when this position was changed, or the mouth of one of the tubes closed, a very audible sound was produced. With a tube of 14 inches long, open at either end, tones were only obtained by stopping

a small hole in the centre; but on inserting into this a glass tube of three inches in length, the effect was reversed, the sound being only heard when the glass was unclosed; upon increasing this small tube to seven inches, being half the length of the larger one, no sound was produced. Mr. Addams said he would not attempt to explain the cause of this phenomena, but leave it to Professor Wheatstone and other abler hands.

*Thursday, 25th August.*—1. *Mr. Talbot's Researches on the Integral Calculus.*—Mr. Talbot had succeeded in assigning an algebraical value to the sum of three or more integrals of functions, whose denominators are not merely quadratic radicals of entire functions of the fifth and higher degree, such as constitute the ultra elliptic integrals furnished by Abel's celebrated theorem, but also of others whose denominators are cubic, and other radicals of similar functions which that theorem does not contemplate; thus effecting a very great extension of one of the most difficult and interesting departments of the integral calculus.

2. *Dr. Apjohn on the Specific Heat of Gases.*—All gases have not, under equal volumes, the same heat, as is conceived by Haycraft, Marcet, De la Rive, and others. Neither is this law true of the simple gases, as supposed by Dulong. The specific heat of hydrogen, under an equal volume, is nearly one and a half that of atmospheric air. The author's numbers come nearer those of De la Roche and Berard than any other. No simple relation appearing to exist, according to the author, between the specific heats of gases and their specific gravities or atomic weights.

3. *Professor Hamilton on the Calculus of Principal Relations.*—By this method, the author proposes to reduce all questions in analyses to one fundamental equation or formula. Its principle depends on the fact, that he had discovered the following relation to subsist between all differential functions, no matter how numerous—

$$\frac{\partial d s}{\partial d x} = \frac{\partial s}{\partial x}.$$

4. *The Rev. W. Scoresby on Two Magnetical Instruments.*—The first instrument depends on the influence of an extremely susceptible bar of soft iron, on a magnetic needle; so susceptible that by merely passing it roughly through the hand, a deviation of many degrees of the needle may be obtained. By means of an appropriate contrivance the bar is set at any given angle to a common compass, and by this the dip may be obtained to within a very small error. The other consists of a needle of about 16 inches long; it is constructed of several thin laminæ of steel, united at their extremities, and separated in the centre by a block of wood, supporting the agate, upon which it rests; the weight of this needle, which is not great, is relieved from the point supporting the agate by suspension silk. There are at the extremities silver verniers. It is intended to apply this instrument to the measurement of magnetic powers generally, and to the distances between inaccessible points, as in the case of the thickness of rocks and walls.

5. *Professor Forbes on the Terrestrial Magnetic Intensity at various heights.*—Professor Forbes, in this communication, has gone

far to determine an important but disputed question in physics, viz., the change in magnetic intensity at different altitudes above the earth's surface. He stated briefly the results of 45 series of observations with Causteen's intensity needles at 13 stations in the Alps and Pyrenees, from six to 1000 feet above the level of the sea, and compared with the intensities observed in the intermediate valleys. The general result at which he arrives is, that there is no general decisive vindication of diminished intensity with height, at least within the limits of error of the instrument, and certainly, if it exists, the diminution must be exceedingly much smaller than M. Kupffer has proposed.

6. A paper, by *Sir D. Brewster*, on the action of crystallized surfaces upon light was read, and gave rise to some discussion on the part of *Sir W. Hamilton* and *Mr. M'Cullagh*; but the statements of the paper do not admit of abridgement.

7. *Dr. C. Williams* gave an account of an improved ear-trumpet, by which sounds are rendered audible at three times the usual distance. The Section then adjourned.

*Evening Meeting.*—8. *Mr. G. W. Hall on the Connexion of the Weather with the Tides.*—*Mr. H.* observed, that the barometer undulates at the changes of the moon, but more commonly sinks than rises; the weather is then generally unsettled, with high winds; as the weather settles, it not unfrequently remains in an indeterminate state. These variations apply to all the changes of the moon.

9. *Mr. Ettrick on an Instrument for observing Terrestrial Magnetism.*—This instrument consists simply of a common Coulomb's balance, substituting for the needle a magnetic bar, to which is screwed a finely divided graduated circle. The degrees of deviation are read off by means of a telescope with cross wires placed perpendicularly to the plane of the circle.

The author described and exhibited to the Section a new kind of cushion; which, being constructed of distinct pieces, and acted on by springs, could apply itself to a cylindrical glass of any irregular surface. He also described an improved rubber for the plate machine, by which the author supposes an increased power is obtained; it is not very different from the form of the rubber as usually employed.

The same gentleman described a *New Instrument for trying the effect of Electrical Discharges in Rarefied Air, or in different kinds of Gases.*—He finds the bar usually observed on sending a shock through a card quite uninfluenced by the state of the medium in which it is placed; it also remains the same in gases of different kinds.

10. *Mr. Addams on the Vibration of Bells.*—The author, in this paper, endeavours to explain a peculiar beat frequently heard in the sound of a clock or tower bell, which he considers to arise from unequal thickness of the metal; this he illustrated by some striking experiments on a glass bell.

11. *Mr. Rootsey on the higher order of Grecian Music, and on Mnemonic Logarithms.*—The first of these papers relates to some kinds of musical chords, rejected by some persons as being im-

practicable, but which the author thinks may be used with considerable effect.

The second paper relates to a system of logarithms, which may be used with great advantage in calculations.

The Section the adjourned.

*Friday, August 26th.*—1. Mr. Whewell gave an account of his anemometer, which he described last year at Dublin, in an unfinished state.—See *Records*, vol. ii. 218.

2. Mr. Stevelly, on *Mathematical Rules for constructing compensating Pendulums*. The author having found the rule given by Kater erroneous, he endeavoured to lay down the true formulæ.

3. *Mr. Phillips* stated the results of his experiments, "On the direction of the Isoclinal Magnetic lines in Yorkshire," to be, that the lines of equal dip were not straight, but had considerable flexures distinctly related to the elevation of the ground, the bendings taking place rapidly to the south, on two elevated ridges by which Yorkshire is intersected.

4. *Sir David Brewster*, "On a very simple contrivance for tracing lines in the Solar Spectrum, which were invisible by other means." This contrivance consisted merely in introducing a cylindrical refractor between the eye and the eye-glass of the telescope. The effect of this refractor was to give a linear form to the most irregular image.

5. *Dr. Hare*, on "Electrical Attractions and Repulsions, and upon the Electric Spark." The object of this paper was to support Franklin's theory, and to refer repulsion to the action of surrounding bodies.

6. *Dr. Carpenter*, on the method of teaching the blind to read.

7. *Mr. Russell*, on "Some of the Elements of the resistances of fluids." The laws of the resistances reduce themselves to, 1st, The law of the emersion of the floating body from the fluid, which is related to the velocity alone; 2nd, The relation of the resistance of the wave; 3rd, The relation of resistance to a certain form of the body.

8. *Mr. Hodgkinson* gave an account of his experiments on the comparative strength of iron made with the hot and with the cold blast. In most cases the hot blast iron seems to be somewhat but not greatly weaker. Its specific gravity is also rather less, except at the Devon works. The fracture of the cold blast was white, that of the hot blast gray. No. 1 and 2 had less tenacity when prepared by the hot blast. The opposite, however, was the case with the Devon iron, No. 3, which by the hot blast was less hard but greatly stronger.

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#### SECTION G.—MECHANICAL SCIENCE.

*President*, Davies Gilbert, Esq.—*Vice Presidents*, M. I. Brunel, Esq., John Robison, Esq.—*Secretaries*, T. G. Bunt, Esq., G. T. Clark, Esq., William West, Esq.—*Committee*, Captain Chapman, G. Cubitt, Esq., J. S. Enys, Esq., William Hawkes, Esq., E. Hodgkinson, Esq., Dr. Lardner, Professor Moseley, M. Le Play,

Sir John Rennie, George Rennie, Esq., John Taylor, Esq., Rev. W. Taylor.

*Monday, 22nd August.*—*Professor Moseley, "On certain points connected with the theory of Locomotive Carriages."*

The author commenced by stating, that there were many gentlemen present acquainted with the practical working of steam-engines, but the relations between the theory and practice were not perfectly understood. The piston of a locomotive engine was pressed on either side by two forces; one resulting from the friction on the road, and the other from the passive friction of the engine itself. If it was lifted from the ground, a person endeavouring to move the wheels would find a resistance equal to 150 lbs.; the cause of the resistance was this, that the traction upon the engine induced additional friction of the machinery, and that probably was one-fifth of the whole amount of friction. If the carriage moved without a train, there would be a passive resistance; if a train were attached to it, there would be induced a considerable friction of machinery. There were, in fact, three causes of resistance; the friction of the carriage, the passive resistance, the additional friction by the train; the first and last varying according to the weight of the train. On the other side there was the expansive force of the steam. The quantity of work done was greater as the velocity was less. He would chiefly apply himself to inclined planes. Great power was required in drawing a train up an inclined plane, but when the train came down the inclination, no additional power was gained, because the steam evaporated through the safety-valve. On this account, in addition to the loss of time, inclined planes on railways were highly injurious, and should be avoided.

Dr. Lardner would make a few observations in consequence of his having given evidence on this subject before Parliament. In all inclined planes, which were more steep than the angle of repose, there would be an unfavourable loss of power. The portion of mechanical force used in ascertaining the inclined plane was not repaid in the descent. If you could take advantage of the accumulation of power in the descent, there might be a perfect mechanical confirmation, but that was not the case in practice, because you were obliged to check the velocity in the descent. It had been stated that in his evidence he had said that inclined planes were not of importance, as the friction in the ascent was given back in the descent. That was an error; he had not so said. Inclined planes were injurious. All the experiments led to the conclusion that every effort should be made to attain as level a line as possible. Every departure from a level, though it saved a quantity of capital in the construction of the road, entailed an everlasting expense. After a number of experiments he had made, the results were these; that in the ordinary state of the roads, the pressure necessary on a level was 7lbs. to a ton, but he had found an extraordinary difference depending on the state of the rails; a difference amounting, in some instances, to such an extent that the friction was reduced to 4lbs.; this occurred when it rained, and the rails were wet, but as soon as the rails became dry the friction again increased to 7lbs. He, there-

fore, should consider that it would be a good thing to place watering-pots before the wheels of the carriages, which would give an additional power in the proportion of nearly 50 per cent.

2. The next paper was on the application of our knowledge of the phenomena of waves to the improvement of the navigation of shallow rivers and canals.

*Mr. Russell*, of Edinburgh, regretted he had not had time to put his observations on this subject into writing. To gentlemen connected with railways he would say, that where canals did exist there could be no man who did not wish that the traffic upon them should be conducted in the most favourable manner. The result of various experiments he had made confirmed the law of Sir Isaac Newton, that the resistance was in proportion to the square of the velocity. The difference of resistance between a horse drawing a vessel trotting and cantering was 108 to 136.

He had made a table which was the result of 2400 experiments. In going at a velocity of

4	miles an hour the resistance was . . .	33 lbs.
6	. . . . .	91
7½	. . . . .	265
8½	. . . . .	215
9	. . . . .	235
11	. . . . .	246
12	. . . . .	352
15	. . . . .	444

But at the velocity of 20 miles an hour the vessel apparently sailed along the surface of the water, and the resistance was very trifling. They would observe that at 8½ miles an hour the resistance was not so great as at 7 or 9. When a vessel was propelled at a certain velocity and she stopped, it had the effect of giving an impetus to the water, and produced a wave varying in its form according to the mass of the water, and he had followed such a wave no less a distance than a mile and half; the velocity of the wave was uniform and independent of the velocity of the vessel; for if a vessel was going 4 miles an hour the wave would go at the rate of 8 miles, and he had seen a large wave overtake a small wave and pass it; waves never exceed in height the depth of the quiescent water. If the velocity of a vessel did not exceed eight miles an hour, it did not divide the water but pushed it forward in the shape of a wave, but beyond that velocity the water was divided. It was possible to increase the velocity and get upon the wave and then the resistance was nothing. Where a canal had a depth of

3 feet	there might be a velocity of . . .	6 miles an hour.
5	. . . . .	8
9	. . . . .	11

So that the greater the depth of water the greater velocity might be attained. The resistance was less above 6 miles an hour, but 4 and 6 miles an hour the velocity was unfavourable, beyond 11 miles an hour you had high velocity and comparatively little resistance. Where it was intended that the velocity should be great he recom-

mended rectangular canals. To make canals wide with sloping banks was an evil.

*Mr. Whewell* considered these observations to be of the highest value and of the greatest importance. It must be recollected that although the wave travelled, the current was not increased. He was very anxious to have the velocity of waves at sea discovered.

*Professor Mosely* considered this might be adopted by taking the velocity of a wave made by a steam vessel.

*Mr. Russell* proposed that the wave should be measured in this way. Let two vessels go out, and one of them be anchored; have a line attached from one to the other when the vessels were each on the top of a wave, let the rope be drawn tight, and that would give the width of the wave; then when the vessels were both in the bottom, a sight taken from the mast would give the height of the wave, and by these means you might almost make a map of the bottom of the sea. The farther vessels went the less would be the resistance, and the Atlantic might thus easily be traversed by steam vessels.

The Chairman said the experiments were most important, and the gratitude of the country was due to the young man who had made them, *Mr. Russell*.

*Tuesday, 23rd August.*—1. *Mr. Hawkins* read the following paper on an *Improvement upon Neper's Rods, for facilitating the multiplication of high numbers, with little liability of error; the invention of J. N. Copham, Esq., of Bristol.*—The invention consists in cutting each of Neper's rods into cubes, and in stringing the cubes together by means of pins passing through two perforations in each cube, made at right angles from each other, parallel to the figured side. By this arrangement the cubes may be readily placed in such positions, in respect to each other, that the product may be obtained by addition only, without the necessity of transcribing the figures from the rods previous to the addition; thus avoiding a great liability to error, and effecting a great saving of time in the calculation. The pins are in two sets, with heads of two different shapes. On the heads of one set of pins are marked 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, respectively, the same pin having the same number on each side of the head, but the number on one side of the head is inverted in respect to the position of the number on the other side. The heads of the other set of pins are also numbered 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, but the pin having 0 on one side of the head has 9 on the other side, that having 1 on one side has 8 on the other, &c. The figures in this set also are inverted in respect to those of the opposite side of the head. The cubes are kept strung on those pins which have the same figures on each side of the head, ten cubes on each pin representing one of Neper's rods. On the pin marked 0, all the cubes are marked 0 on both sides. On the pin marked 1, the cubes are marked 0, 1, 2, &c., on one side, and 9, 8, &c., on the other side, the two sides of each cube, on being added together, make 9: on the pin marked 2, the cubes are marked 0, 2, 4, &c., on one side, and 18, 16, &c., on the other side: the numbers on the two sides of each cube, on being added together, make 18; and thus the numbers on

the cubes of each pin are all consecutive multiples of the number on the head of the pin, and the two numbers on each cube, on being added together, make the number on the head multiplied by 9; the numbers ascending on one side and descending on the other.

2. *Dr. Daubeny* explained the properties of an instrument he had contrived for obtaining sea water at great depths.

3. *Mr. Braham* explained an improvement he had made in the mariner's compass. It was found, that in consequence of the vibratory motion in steam vessels, the compass got out of order; he, therefore, proposed to put a fluid in the box of the compass, so that the card might float upon the fluid. It was obvious, that a card on a fluid would be liable to injury and decay; he had, therefore, caused the points of the compass to be painted on porcelain, which he had affixed to a flat piece of cork, and thus it was kept floating upon the fluid, and the motion of the steam vessel had no effect upon it.

*Evening Meeting.*—4. *Mr. Whewell* delivered a brief lecture on Tides, and stated, that at the last annual meeting of the Association, a sum of £250 had been voted for the purpose of making experiments and observations relative to the subject, which sum had been expended, but the report of the results had not yet been published. He also produced a machine which had been used in the Avon, with a view of ascertaining the variation in the tides, and the result had been, that one of the theories on this branch of science had been proved to be practically correct.

5. *Steam Communication with India.*—*Dr. Lardner* then proceeded to deliver a lecture on this question. The subject, which it was wished should be brought under the consideration of the Section, was one of very considerable importance, but of peculiar importance to this particular city; taking it in its whole extent, it was the solution of a problem, how far the present powers of steam were capable of being extended in their application to navigation. The moment this question was proved, two grand commercial problems would suggest themselves, namely, the connexion by steam of Great Britain with our colonial possessions, and the connexion by steam of Great Britain with the United States of America. These two questions embraced a great variety of topics of much interest; he, therefore, proposed, with the approbation of the general council, to present at present all the more popular and more intelligible parts of the subject, reserving for the morning meeting the more technical details. The subject to which he wished more particularly to address himself this evening, was the application of steam to a communication between Great Britain and India. There were several routes by which a communication could be effected; the only route which was in a continued line of navigation was by the ordinary voyage by the Cape. That had been already tried by steam, but if it had not, they were sufficiently conversant with the properties of the present steam engine to know that it was incapable of being worked profitably. That voyage was attempted by the Cape by the *Enterprize*, and it was performed in 113 days, but only 64 days were worked by steam; it afforded evidence, that to establish a line of steam communication by that route was out of the question.

The other routes were partly by land and partly by water. One from Great Britain, across Germany by the Danube, the Black Sea, across Turkey to the river Euphrates, descend to the Persian Gulph, and then pass on to India. But this route was out of the question, from the difficulties to be encountered. The real practical courses then which presented themselves were only two, and these would come to a certain point. They proposed starting from Falmouth, to proceed up the Mediterranean; indeed, we did that already to Malta, therefore, it was not necessary to discuss the practicability of that route. From Malta they would adopt either of two courses—one would be to proceed to Alexandria, a distance of 800 miles, and quite within the present limits of steam power, as 1600 miles had already been performed; they then proceeded to Syria, and descended the Euphrates. So far as Alexandria was concerned there was no practical difficulty; they then proceeded to the Red Sea by land, or partially by the Nile, to Cairo. If they would refer to the plan of the Isthmus of Suez, the road to Cairo followed the banks of the Nile; the only difficulty they had to encounter here was the Isthmus; they would have to proceed to Suez by a sandy desert; but about the centre of it there were springs of fresh water, and the distance did not exceed 70 miles, and was now performed in less than 24 hours. It was, however, suggested, that a canal might be cut across, and the idea had been thrown out that a railroad might be constructed. Now, there were other modes of crossing the Isthmus, which were deserving of notice. The general character of the Isthmus, between the Mediterranean and the Red Sea, was very peculiar; when we proceed due north, we pass over three miles of land very little elevated above the surface of the Red Sea; we then come into a narrow valley bounded on each side by hills, and in the centre of those valleys we found the distinct trace of a canal, which was known to have existed in former times, and with the history of which they were all acquainted; this canal now, in many places, is in as perfect a state as many of the old canals in this country; but one of the remarkable peculiarities was this, that a great portion of its surface was not only below the level of the Red Sea, but still more remarkable, was below the level of the Mediterranean—now the valley, which from the point is three miles towards the Mediterranean, was below both. That this valley was at some time or other filled with water, connecting the two seas, was rendered probable by the fact, that it was lower than the level of both the two, and that the water which remained was in fact salt, and was called the bitter lake.—That portion of water was, in fact, a part of the ancient canal; coming to a point midway between Suez and the Mediterranean we find two other lines of hills. The canal was conducted into the hill near Cairo; one-third of that canal now existed, and only required to be cleaned out; and the other portion could be restored. This canal was begun by Sesostris, and was the channel by which European commerce was conveyed to the East. But this stupendous work could be used only two months out of ten, for the ancients were unacquainted with our contrivance of locks. Nevertheless, during those two months the trade of

Europe passed in this canal to Suez. During the other ten months, the commerce passed up the Nile, and then to an ancient port, where they embarked on the Red Sea. Now, it was proposed to open that canal, and to look it as far as Suez, if no physical obstacle arose. The Red Sea was the next point for consideration, and much had been said about the difficulties which existed in the transit through this sea, owing to the beds of coral; but they were confined to the coasts, and were well known to the local pilots, and created no more difficulties than the rocks which existed on our own coasts. The steam navigation of the Red Sea was practicable and convenient, and there were several safe harbours. Proceeding downwards, they came in a run of about 300 miles, to Corsair, from whence they would go 300 or 400 miles, from thence to Jedda, on the opposite side of the coast, which was the port of the city of Mecca.\*

There was another town, Mocha, where there was a safe road-stead. On the coast of Arabia there were two or three harbours, all of which were convenient; so far there was no practicable difficulty. He should state, however, that the voyage from North to South of the Red Sea was more easy than from South to North; for ten months the North wind blows so violently, that no steam packet could face it. They now came to the real difficulty in the case; when they proceeded from the ports on the Arabian coast, they had a long run to get to the nearest port in India, namely, Bombay, that being 1200 miles: now, when they had to encounter a run of this kind, which approached very near the extreme limit of our present power of steam navigation, it became a question of the last degree of importance, whether or not they should have average weather: now, the seas between Arabia and Africa and India were subject to periodical vicissitudes, which were unknown in our climate. There was a periodical wind which blew as regularly as the sun rose and set, and blew with an intensity with which we were little accustomed, they were known by the name of the north-east and south-west monsoons; the south-west blew from June to September. They would readily perceive there could exist no steady atmospheric current of that kind, without passing some counter current to produce an atmospheric equilibrium; accordingly, they found the counter current flowing in the opposite direction between November and March; but, although they blew about equal time, they were very different in intensity, and in their effects on the water. The north-east monsoon was a wind against which a steam vessel could go without any difficulty; consequently, during the months from June to September, the navigation was impracticable from Bombay, but it was practicable during all the year from Europe to Bombay; the swell for 800 miles from Bombay was such, that if they attempted to use sufficient power to propel the vessel, it would drive her into the sea.

The other route was from Malta to any part of Syria, and was without difficulty from these ports to the banks of the Euphrates; but then there were some other difficulties; the passage was not across a sandy desert, but it was infested by savage tribes, who were

\* See these various distances, Records, i. 474.

not professed robbers, but would not stop much to consider the propriety or impropriety of robbing you. The town, which had been suggested as the point of departure, was Birr, 1200 miles from the mouth of the river; now, this river was one which presented many circumstances extremely questionable for the application of steam navigation. The average speed of the current of the river was no greater than three miles an hour, at certain points, however, it rose to seven miles an hour. The magnitude of this river they might form a notion of, when he told them, that at Birr, the breadth was something like the Thames at Lambeth, and down as low as Babylon, it flowed like the Thames at Deptford. The depth of the river was quite sufficient for safe and speedy navigation; but this river is subject to a low season, during which, there were some difficulties—it existed only in part of the river. Passing Babylon and Bagdad, you come to a village called Elkain, a distance of about 170 miles, and in these 170 were included all the physical difficulties. There were 15 or 16 shallows and rapids, which, in the low season, were difficult; but it did so happen, that the low season of the Euphrates was the very season during which the north-east monsoon blew; the impracticable season of the one was during the practicable season of the other. It was proposed to navigate by iron vessels, and coal might be obtained from Wales, at a cost of about £2 a ton.

In ancient times the communication between England and India was by the Euphrates, and step by step the very route they were now thinking of resuming, and it was a remarkable circumstance, for the progress of civilization seemed to sport with our endeavours; before the discovery of the Cape, our merchants found their way to India by the Euphrates, and a Portuguese was immortalized for the discovery; but another discovery was made by means of the steam engine, and that sent us down the Euphrates again in the old way to India. The voyage by either route, from Falmouth to Bombay, might be done in seven weeks. The Section then adjourned.

*Wednesday, 24th August.*—The Chair was taken by Davies Gilbert, Esq., at eleven o'clock.

1. The Chairman read a paper on Naval Architecture, sent by Mr. Henwood, of Portsmouth Dockyard.

2. *Mr. Price* exhibited a model of a new construction of paddle wheels; he had them placed on his vessel, and could now do 108 miles in eight hours and a half. The paddle rose vertically and the water ran off, and it was also a saving of one-third in fuel and time. These paddle wheels were adopted by the Ordnance.

*Mr. Russell* would state that in Scotland they had had great experience in steam vessels, and he would state some circumstances which were within his knowledge, and he would address himself particularly to the encrustation on the boiler, produced by the salt water; he had found out, when on board a steam vessel, a simple and beautiful expedient for remedying this, and it had been kept a perfect secret. He would take a boiler of a cylindrical shape; that which was most dense in the water would of course fall to the bottom, and therefore, as the cold water came in at the top the salt would descend to the bottom below the furnace, and then came the secret,

there was a pipe with a stop cock, and the engineer filled the boiler a little too full, he then opened the stop cock and got off the salt. The boiler was worked for nine months, and a man was then sent into it for the purpose of clearing it out, and he found he had nothing to do for there was no encrustation. With regard to the engine, he was not one of those who expected any very great radical improvement in the construction of the steam engine; Watt, in his opinion, had left them but very little to do. In Scotland they had adopted the plan of the Cornish engine. An engine was worked on the high pressure system, and it worked expansively, and with this engine at the top of high water, with a cargo of 150 passengers, in its ordinary rate he had gone  $14\frac{3}{4}$  miles an hour; the great thing to be attended to was the precise place of fixing the engine; he believed that with the ordinary boilers well made, and every thing being of the best kind, every effect they could reasonably expect would be obtained. With regard to paddle wheels, he considered those produced by Mr. Price of great value, where the engine was not properly proportioned, or where the vessel was not a good one; but he was convinced, from a long train of circumstances, that in a well-built vessel, with properly proportioned engines, the common paddle wheel was not only the simplest, the cheapest, the most secure, but was the best in theory as well as in practice.

*Mr. Price* maintained that the patent paddle wheel was very far superior. He had laid out £1000 in putting them to his vessel, and he had found that he could beat all the other vessels of the same size.

*Thursday, 25th August.*—1. *Mr. Chatfield* read a very long essay on *Naval Architecture*.

2. *Mr. Enys* gave a long account of the working duty on the Cornish Steam Engines.

3. *Steam Communication with Distant Parts.*—*Dr. Lardner* in making his remarks on this subject said, he would beg of every one, and more especially of those who had a direct interest in the inquiry, to dismiss from their minds all previously-formed judgments about it, and more especially upon this question to be guarded against the conclusion of mere theory; for if there was one point in practice of a commercial nature which more than another required to be founded on experience, it was this one of extending steam navigation to voyages of extraordinary length. He was aware, since the question had arisen in this city, it had been stated that his own opinion was adverse to it; that impression was totally wrong; but he did feel that as steps had been taken to try this experiment, great caution should be used in the adoption of the means of carrying it into effect; almost all depended on a first attempt, for a failure would much retard the ultimate consummation of their wishes. With regard to the power of steam engines, practical men considered that for short trips the best proportion was to give the vessel the power of one horse for every two tons; that as the length of the trips increased they must have a smaller proportion of power, this should be three tons for every horse power, and for the longest trips to which steam could be applied, about one horse to four tons.

# Meteorological Journal, -- For August.

Kept at the Manse of the Parish of Abbey St. Bathans, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea.  
By the Rev. JOHN WALLACE.

DATE.	THERMOMETER.						HYGROMETER (Lieske's.)						BAROMETER. At 35° of Fahrenheit.						Rain in Inches Weekly.	Direction of Wind at X. A.M.	REMARKS.
	IN. A.M.	X. M.	III. P.M.	P. M.	X. P.M.	W. M.	IN. A.M.	III. P.M.	P. M.	X. P.M.	W. M.	IN. A.M.	X. M.	III. P.M.	X. P.M.	W. M.					
Mo. 1	58	53	54	50	17	23	24	7	29-319	29-301	29-207	29-171	S.W. by W.		Brisk wind, showery, evening cloudy but fair.						
Tu. 2	53	54	55	48	17	17	31	0	29-436	29-437	29-463	29-496	N.E. by E.		Calm, A.M. cumuli floating on a blue sky, P.M. cloudy, evening overcast.						
We. 3	53	54	57	51	0	0	0	0	29-393	29-393	29-382	29-421	S. E.		Calm, A.M. incessant rain, P.M. frequent fog, evening wind veering N. E.						
Th. 4	52	51	51	45	2	0	6	0	29-496	29-515	29-537	29-561	E.		Calm, A.M. foggy, P.M. partially clear, evening copious deposition, sky clear.						
Fr. 5	53	53	57	51	18	13	20	1	29-566	29-560	29-537	29-575	S. E.		Calm, A.M. fleecy cumulus masses of cloud on blue sky, P.M. overcast.						
Sa. 6	60	61	60	55	26	28	24	3	29-598	29-607	29-628	29-700	N. W.		Calm, cloudy, and the aspect of the sky occasionally thunderous.						
Su. 7	60	61	63	53	22	25	30	8	29-683	29-704	29-635	29-610	E.		Calm, cloudy, A.M. aspect of the sky thunderous, P.M. cirrocumuli prevalent.						
Mo. 8	54	53	60	49	22	25	30	8	29-610	29-624	29-640	29-663	W. N. W.		Calm, fleecy clouds on a blue sky, evening cloudless, deposition.						
Tu. 9	58	60	65	52	21	23	23	2	29-670	29-674	29-664	29-681	W. N. W.		Calm, a few fleecy clouds on a blue sky, evening cloudless.						
We. 10	59	63	63	48	16	18	14	0	29-710	29-713	29-704	29-754	W.		Very calm, A.M. clouds of a thunderous appearance, P.M. nearly cloudless.						
Th. 11	62	65	68	54	15	19	34	2	29-797	29-794	29-769	29-789	S. S. E.		Very calm, cirri with cumuli on a blue sky, evening clear.						
New M Fr. 12	67	68	72	56	38	31	48	2	29-768	29-770	29-716	29-751	N.W. by W.		Very gentle breeze, cirri and cumuli prevalent, evening calm and cloudless.						
Sa. 13	64	66	70	52	28	30	17	0	29-673	29-656	29-589	29-501	N.W. by W.		Very gentle breeze, cumuli prevalent, evening cloudless.						
Su. 14	64	66	70	59	15	18	30	2	29-392	29-369	29-293	29-348	S. S. E.		Calm, heavy masses of cumulus clouds, evening sky thinly veiled with cirrocu.						
Mo. 15	57	59	58	53	8	9	10	2	29-502	29-516	29-546	29-615	S.E. by E.		Gentle breeze, A.M. fog, P.M. sky veiled with hazy clouds, evening foggy.						
Tu. 16	60	61	63	52	9	16	17	7	29-554	29-537	29-433	29-374	W. S. W.		Gentle breeze, cloudy, sometimes overcast and lowering.						
We. 17	57	57	58	51	25	27	28	14	29-377	29-377	29-404	29-435	W.		Gentle breeze, cirri and cirrostrati on a blue sky, evening cloudy.						
Th. 18	58	60	60	51	1	6	9	11	29-211	29-186	29-122	29-270	S.W. by W.		Brisk wind, cloudy, with frequent showers, evening clear, wind strong.						
Fr. 19	54	55	58	49	13	20	30	3	29-397	29-391	29-398	29-416	W.		Brisk wind, cloudy, (cirri and cumuli), evening calm and cloudless.						
Sa. 20	56	57	60	48	13	15	20	14	29-142	29-122	29-066	29-228	S. W.		Gentle breeze, sky overspread with soft clouds, evening calm.						
Su. 21	51	53	54	52	17	21	18	8	29-233	29-214	29-174	29-097	S. W.		Brisk wind, cloudy and troubled sky, P.M. occasional light showers, evg. calm.						
Mo. 22	50	50	45	45	5	7	0	2	29-185	29-158	29-989	29-986	W. N. W.		Gentle wind, rain until 5 P.M., evening calm and clear, copious deposition.						
Tu. 23	46	47	51	45	18	23	27	0	29-135	29-128	29-237	29-433	N. W.		Calm, A.M. clear, cumulus clouds on a blue sky, evening heavy showers.						
We. 24	49	49	52	42	22	22	20	0	29-632	29-634	29-642	29-633	N.		Calm, fleecy clouds on a blue sky, 6 P.M. cirrostratus polarized from N. to S.						
Th. 25	50	53	54	51	11	19	19	0	29-458	29-444	29-324	29-267	S. W.		Brisk wind, cloudy and occasionally overcast, evening lowering.						
Full M. Fr. 26	52	54	54	46	28	32	33	9	29-213	29-225	29-235	29-341	W.		Brisk wind, cumuli on a blue sky, evening sky veiled with cirrostratus.						
Sa. 27	49	50	57	44	0	7	35	8	29-248	29-289	29-310	29-364	W. by S.		Gentle wind, cumuli on a blue sky, evening nearly cloudless.						
Su. 28	53	53	54	43	13	11	29	10	29-433	29-424	29-265	29-368	W.		Brisk wind, A.M. cloudy, P.M. floating cumuli, evening cloudless.						
Mo. 29	51	53	54	43	24	29	26	10	29-433	29-454	29-475	29-548	W. by N.		Brisk wind, cumuli on a blue sky, evening calm and clear.						
Tu. 30	48	46	48	50	5	5	0	0	29-554	29-545	29-425	29-357	S. E.		Calm, after 10 A.M. rain or drizzle, evening cloudy.						
We. 31	58	61	63	54	0	6	15	0	29-230	29-230	29-235	29-251	S. W.		Brisk wind, cloudy, drizzle in the evening, calm and mild.						

Means Therm. 53°·2 } Mean temperature of spring water 40°·2  
 for the Hygr. 11° } Mean point of deposition 48°·9  
 Month } Bar. 29·447 in. } Moisture in a cubic inch of air = ·002353 grs.

Means. 53·5 56·7 58·5 49·8 15 18 21 4 29-443 29-442 29-423 29-452 2-526

# RECORDS

OF

## GENERAL SCIENCE.

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### ARTICLE I.

*Notice of Carburet of Potassium, and of a New Gaseous Bi-carburet of Hydrogen.* By EDMUND DAVY, ESQ.,  
Professor of Chemistry to the Royal Dublin Society.

(Communicated to the British Association, 26th August, 1836.)

IN January last, the author made different experiments to obtain the metal of potash on a large scale, by exposing to a high temperature, in an iron bottle, a mixture of previously ignited tartar and charcoal powder, in proportions of the latter varying from about  $\frac{1}{10}$ th to  $\frac{1}{15}$ th of the whole mass.

In one experiment, a substance was obtained of a dark gray colour, rather soft to the knife, though adhering with tenacity to the iron, and inclining to a granular structure. This substance, when put into water, decomposes it with great facility, carbonaceous matter is disengaged, and gas copiously evolved, with occasional inflammations on the surface, as is commonly the case with potassium under similar circumstances. The gas, when examined, was found to consist of hydrogen, and a new bi-carburet of hydrogen, (noticed in a subsequent part of this communication), in nearly equal volumes. The author regards the substance in question as a mixture of potassium and carburet of potassium; the former, by its action on water furnishing the hydrogen, the latter, the new gas. In collecting gas from the substance, by the action of water over mercury, a novel and interesting case of combustion was observed. A little of the substance being placed in a tube

filled with mercury, on letting up a few drops of water, gas was copiously disengaged, and as the mercury descended along the tube, small portions of the substance became ignited, exhibiting the appearance of bright sparks of fire in continued succession.

In another experiment with the iron bottle, the author procured no potassium, but a small quantity of a substance partly in powder, and partly in small lumps, of a dense black colour. This substance the author regards as carburet of potassium. It exhibits no appearance of crystallization to the naked eye; but when viewed with a glass of high magnifying power, the author thinks he has observed congeries of exceedingly minute four-sided prisms, truncated at their solid angles. When the carburet is exposed to the air, it soon undergoes changes, oxygen and water appear to be absorbed, and caustic potash and carbon remain.

When the carburet is put into water, both substances are decomposed, one portion of the carbon unites with the hydrogen of the water to form the new bi-carburet of hydrogen, which is the only gaseous product, the remainder being disengaged, whilst the oxygen of the water and the potassium form potash.

Alcohol and turpentine act feebly on the carburet, acids strongly.

The carburet undergoes partial decomposition at a dull red heat in close vessels, potassium slowly rises from it, whilst the carbon remains of a deep and bright black colour.

The author regards the pure carburet as a binary compound of one proportion of carbon and one of potassium.

#### *New Bi-carburet of Hydrogen.*

This gas was obtained by the action of carburet of potassium on water. It is highly inflammable, and when kindled in contact with air, burns with a bright flame, apparently denser and of greater splendour than even olefiant gas. If the supply of air is limited, the combustion of the gas is accompanied with a copious deposition of carbon. When the new gas is brought in contact with chlorine gas instant explosion takes place, accompanied by a large red flame, and the deposition of much carbon, and these effects readily

take place in the dark, and are, of course, quite independent of the action of the sun's rays or of light. The new gas may be kept over mercury for an indefinite time without undergoing any apparent change; but it is slowly absorbed by water. Recently boiled distilled water, when agitated in contact with the new gas, absorbs about its own volume of it; but, on heating the aqueous solution, the gas is evolved apparently unaltered. The gas is absorbed to a certain extent by and blackens sulphuric acid.

The new gas detonates powerfully with oxygen, especially when the latter forms three-fourths or more of the mixture, and the only products appear to be water and carbonic acid gas. It requires for its complete combustion  $2\frac{1}{2}$  volumes of oxygen gas, two volumes of which are converted into carbonic acid gas, and the remaining half volume into water. From the author's analysis by different methods, the new gas appears to be composed of one volume of hydrogen and two volumes of the vapour of carbon condensed into one volume. Its density is therefore less than that of olefant gas by the weight of a volume of hydrogen equal to that of its own bulk. It is, in fact, a bi-carburet of hydrogen composed of two proportions of carbon and one of hydrogen, and may be represented by the formula  $C^2 + H$ , or  $2C + H$ ; and its constitution seems to differ from that of any other known gas.

From the brilliancy with which the new gas burns in contact with the atmosphere, the author thinks it is admirably adapted for producing artificial light, if it can be procured at a cheap rate.

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## ARTICLE II.

*On the Atomic Weight of Nickel and its Oxides.* By THOMAS THOMSON, M. D., F. R. S. L. & E., Regius Professor of Chemistry in the University of Glasgow.

IN a paper entitled "Observations on the Atomic Weights of Bodies," published in the third volume of the *Records of General Science*, I adopted (see page 255) 3.625 as the atomic weight of nickel, because that number agreed better with the specific heat of that metal, stated by Dulong and Petit, at 0.1035, supposing the law that the atomic weight

multiplied by the specific heat amounts to the constant quantity 0.375 to be correct. My atomic weight for nickel deduced from a very simple experiment, which I still consider as susceptible of greater accuracy than any analysis whatever, was 3.25. Now,  $3.25 \times 0.1035 = 0.337$ ; while  $3.625 \times 0.1035 = 0.375$ . At the same time I stated, that I had never been able, by the most careful experiments, to obtain any number for nickel ever approaching to 3.625, which would make the atom of nickel higher than that of iron. I made a hasty experiment in the month of January last, which I have stated in a note.—*Records of General Science*, iii. 255. But it was unsatisfactory; and my numerous occupations, at that busy season of the year, in the college of Glasgow, put it out of my power, at that time, to prosecute the experiment. I, therefore, left the investigation till summer should arrive, and give me leisure to resume the subject. I have just finished the experiments which I had projected (July 10th, 1836), and propose in this paper to state the results which I obtained.

In my *First Principles of Chemistry*, vol. i. p. 358, I have stated the experiments from which I deduced 3.25 to be the atomic weight of nickel. I found, that when a solution of 17.125 grains of pure crystals of sulphate of nickel is mixed with a solution of 13.25 grains of chloride of barium, a double decomposition takes place, and after the sulphate of barytes has fallen to the bottom, the supernatant liquid, tested by solutions of sulphate of soda and chloride of barium, remains transparent, and, consequently, contains no appreciable quantity either of sulphuric acid or barytes.

This experiment I still find to be perfectly correct. I employed in repeating it sulphate of nickel, which had been purified by five successive crystallizations. The crystals were small but well formed, and they were rendered as dry as possible without losing any of their water of crystallization by pressure between folds of bloating paper and exposure to the air. The chloride of barium was prepared on purpose for the experiment. It was perfectly pure, and rendered anhydrous by exposure to a red heat in a platinum crucible.

The conclusion which I drew from this experiment was, that 17.125 grains of crystallized sulphate of nickel con-

tains exactly five grains of sulphuric acid. Dr. Turner has objected to this conclusion, that I employed 13.25 grains of chloride of barium instead of 13 grains, the quantity containing the exact proportion of barium that is necessary to saturate 5 of sulphuric acid. It would seem at first sight, that my experiment indicated the presence of 5.096 of sulphuric acid instead of 5. But I find that in practice we must always employ 13.25 of chloride of barium to throw down 5 of sulphuric acid. If we employ less, the whole sulphuric acid is not thrown down. In consequence of this curious fact, I found it impossible to determine the atomic weight of barytes by mixing sulphate of potash and chloride of barium. The result always gave 9.75 for the atomic weight of barytes. I succeeded in obtaining 9.5 only when I substituted carbonate of barytes for sulphate, and employed re-agents, which were dissipated by a red heat. Many an experiment on this subject has been made by my practical pupils, and some of the most accurate among them have been so much struck with it as to be inclined to adopt 9.75 for the atomic weight of barytes. But I satisfied myself many years ago (as I have elsewhere stated) that 9.5 is the real number; but, that owing to some curious combinations which take place, we never can precipitate the whole sulphuric acid from a solution without employing a slight excess of barytes.

The above experiments (which any person may easily repeat) leave no doubt on my mind, that 17.125 grains of pure crystallized sulphate of nickel contain exactly 5 grains of sulphuric acid.

The determination of the water of crystallization of this salt is a very difficult thing. For if we heat the salt too high, we drive off not merely the water, but also a portion of the acid. The heat should be applied very slowly and equably, and it should be raised higher than 600°, but not so high as a red heat. If the salt after this exposure be still soluble in water, it is a proof that none of the acid has been driven off. In a very cautious experiment I drove off 7.8 grains from 17.125 of the crystals, and the salt was still soluble in water. In another, I drove off 8 grains; but the salt did not dissolve completely in water. From these trials I concluded, that the water of crystallization lies

between 7·8 and 8 grains. Now, 7 atoms of water weigh 7·875, which is within one per cent. of 7·8, the quantity actually got. I concluded from these facts, that sulphate of nickel contains 7 atoms water; nor do I think any doubts can be entertained on the subject. It is clear, then, that 17·125 grains of crystals of sulphate of nickel contain

Sulphuric acid, . . .	5·
Water, . . . . .	7·875

What is wanting to complete the sum, must be the weight of an atom of protoxide of nickel. Now, this quantity is 4·25. Therefore, 4·25 is the weight of an atom of protoxide of nickel, and, consequently, 3·25 is the atomic weight of metallic nickel.

Such are the results which I still continue to obtain, when the experiment is made in the way above stated, however pure the sulphate of nickel and chloride of barium which are employed. But, when we subject crystallized sulphate of nickel to analysis, the results which we obtain are different.

The result, however, of this analysis is quite incompatible with the atomic weight of nickel as stated by Berzelius, which he makes 3·69675. I do not know on what data this number is founded; for I am not aware of any experiments which he has made on the subject. He quotes Rothoff's experiments, the result of which only, so far as I know, has been published by Berzelius himself. According to Rothoff's experiment, protoxide of nickel has an ash-gray colour, and is composed of 100 nickel and 26·909 oxygen. This is equivalent to

Nickel, . . .	3·716
Oxygen, . . .	1·

So that it does not appear upon what authority Berzelius has made the atomic weight of nickel 3·69675.

We have an analysis of sulphate of nickel by Mr. Richard Phillips, well known for the accuracy of his experiments. According to him, it is composed of

Oxide of nickel, . . .	26·30 or 4·669
Sulphuric acid, . . .	28·16 or 5·
Water, . . . . .	45·54 or 8·08

---

100·00\*

\* *Annals of Philosophy* (2nd Series), vi. 440.

According to this analysis protoxide of nickel has an atomic weight of 4.669. This is lower than the number assigned by Berzelius, and still lower than that which results from the experiments of Rothoff's. But it is so much higher than my number, 4.25, that I was desirous of discovering, if possible, the cause of the difference between us.

I dissolved 50 grains of crystals of sulphate of nickel, purified by 5 successive crystallizations in water, and threw down the sulphuric acid by chloride of barium, purified on purpose. The sulphate of barytes, after thorough edulcoration and ignition, weighed 42.41 grains = 14.62 grains of sulphuric acid, or 29.24 per cent.

Fifty grains of the same salt, being dissolved in water, were mixed with a solution of caustic soda. The oxide of nickel, after thorough edulcoration and ignition, weighed 13.79 grains, or 27.58 per cent. According to this analysis sulphate of nickel is composed of

Oxide of nickel, . . .	27.58 or 4.716
Sulphuric acid, . . .	29.24 or 5.
Water, . . . . .	43.18 or 7.38
	100.00

This analysis was repeated with almost the same result.

According to this analysis the atomic weight of nickel is 4.716, the very same as stated by Rothoff to be the result of his experiments.

But a little consideration must satisfy us, that the oxide of nickel obtained by this analysis is not the oxide which exists in sulphate of nickel. For the water in the salt (if the oxide weighs 4.716) is only 43.18 grains. But I actually separated from it (as has been already stated) 45.54 grains of water, which is more than could have existed if the atom of protoxide of nickel were 4.716, or even 4.69675 as Berzelius makes it.

For the component parts of this salt would be

Oxide of nickel, . . .	27.58
Sulphuric acid, . . .	29.24
Water, . . . . .	45.54
	102.36

giving a surplus of 2.36 per cent., which must be owing to

some change in the oxide of nickel. For there can be no mistake either respecting the sulphuric acid or water.

This circumstance induced me to turn my attention to the oxide of nickel, which was obtained by mixing sulphate of nickel with caustic soda, edulcorating the precipitate, and exposing it to a red heat. The oxide of nickel obtained in this way is a beautiful black matter concentered into small lumps, having considerable lustre, and easily reduced to powder. It is tasteless and insoluble in water. But it dissolves slowly in sulphuric and nitric acids. During the solution an effervescence takes place, and oxygen gas is given out in abundance. This remarkable evolution of gas, which uniformly took place, led to the conclusion, that the oxide was not the protoxide, but the peroxide of nickel; and that it had been peroxidized during the process of ignition.

To determine this point, I put 10 grains of it into a small ball blown in a green glass tube. This tube was attached to another, containing fragments of fused chloride of calcium, connected with a small Woulfe's bottle, in which hydrogen gas was evolved by the solution of zinc in dilute sulphuric acid. After all the common air had been expelled, and the tubes filled with hydrogen gas, a spirit lamp was placed under the oxide of nickel, and kept under it till it was gradually raised to a red heat. During the whole process a current of hydrogen gas was passing through the tube, and it continued to pass till the experiment was finished and the apparatus cold. It is well known, that under these circumstances, the oxide of nickel is rapidly reduced to the metallic state. The object of the experiment was to determine the loss of weight which the oxide of nickel would sustain when reduced to the metallic state. The experiment was made thrice successively. The following table shows the results :

	Oxygen.
1. 10 grains oxide when reduced, lost	3·33 grains.
2. 10 . . . . .	2·95
3. 10 . . . . .	3·57
	<hr/>
Mean . . . . .	3·28

The oxide of nickel obtained then is a compound of,

Nickel . . .	6·72 or 3·25
Oxygen . . .	3·28 or 1·58
	10·00

It is obviously the peroxide of nickel obtained by Rothoff, and which he showed to be a compound of 1 atom metal and  $1\frac{1}{2}$  atom oxygen.\* If we were to consider the result of this analysis of sulphate of nickel and peroxide of nickel as perfectly accurate, the atomic weight of nickel would be only 3·085. But, I consider my original number, 3·25, founded on the determination of the sulphuric acid in a given weight of the sulphate by double decomposition as susceptible of far greater precision, and, therefore, more to be depended on. Meanwhile, the preceding experiments leave no doubt about the cause of the different atomic weights given to nickel by Berzelius and myself. He has adopted the numbers derived from the experiments of Tupputi and of Rothoff, and these chemists have mistaken the peroxide of nickel for the protoxide.

It may be worth while to correct my analysis of sulphate of nickel, by changing the peroxide of nickel obtained into protoxide. Reducing the peroxide, on the supposition that

\* As a further corroboration of this constitution I may mention another experiment. 10 grains of the same black oxide were mixed with 17·63 grains of a sulphuric acid, composed of

1 atom acid, . . . . .	5·
3 atoms water, . . . . .	3·375
	8·375

and, therefore, equivalent to 10·525 grains anhydrous acid. The solution took place slowly and with effervescence, but was complete. It was placed under the exhausted receiver of an air pump over sulphuric acid. It crystallized to the very last drop. It was left under the receiver till the crystals had effloresced, all except a few large ones in the centre, which continued green and translucent in the middle, though the surface had effloresced. The salt weighed 27·5 grains. It was composed of

Acid . . . . .	10·525 or 5·
Oxide of nickel . . . . .	8·947 or 4·25
Water . . . . .	8·028 or 3·813

Had the large crystals not retained too much water, it is obvious that there would have remained exactly three atoms water; so that four had escaped. The excess amounted to about two-fifths of an atom.

This does not prove the oxide to be protoxide, but it shows that the atomic weights of the oxides of nickel are 4·25 and 4·75.

an atom of protoxide of nickel weighs 4·25, and an atom of peroxide 4·75, we have,

Protoxide of nickel . . . . .	24·67 or 4·218
Sulphuric acid . . . . .	29·24 or 5·
Water . . . . .	46·09 or 7·881
	<hr style="width: 10%; margin: 0 auto;"/>
	100·00

The water of crystallization, in this case, is very near seven atoms, exceeding that number by only 0·006 or only  $\frac{1}{187}$ th part of an atom. I think the reason of this small anomaly, making the atom of protoxide of nickel 4·218, instead of 4·25, is probably, that a small portion of the oxide of nickel examined, may have existed in the state of protoxide. For I find that by long exposure to an intense red heat, the oxygen in the black oxide of nickel is sensibly diminished. I once succeeded in getting an oxide by intense heat of an ash gray colour, with a very slight tint of green, which was reduced by means of hydrogen gas in the way above described. 5·05 grains of it sustained a loss of 1·19 grains. Hence, it was composed of,

Nickel . . . . .	3·86 or 3·25
Oxygen . . . . .	1·19 or 1·001
	<hr style="width: 10%; margin: 0 auto;"/>
	5·05

so that it was very nearly in the state of protoxide. But, a strong and long continued heat is requisite to bring it to this state.

From the experiments stated in this paper, which were made with every possible attention to accuracy, no doubt remains on my mind that the atomic weight of nickel is, as I long ago stated, 3·25, and that Rothoff's number 3·716 is the consequence of his having taken the peroxide of nickel for the protoxide. I must, therefore, retract the statement made in the *Records*, (vol iii. p. 255,) and admit that the error lies not in the atomic weight of nickel, but in the number given by Dulong and Petit for the specific heat of that metal. If the specific heat of nickel be 0·115, instead of 0·1035, then the law of Dulong and Petit would apply with accuracy to that metal, supposing its atomic weight to be 3·25, as I believe it to be.

## ARTICLE III.

*On Manganic and Hypermanganic Acids, on Hyperchloric Acid and the Salts of these Acids.* By E. MITSCHERLICH.

(Concluded from page 183.)

IF hypermanganate of potash be dissolved in a solution of potash, and the solution evaporated under the air pump over sulphuric acid, the red crystals of hypermanganate of potash again make their appearance, only a very small quantity being decomposed.

A very dilute solution of hypermanganate of potash is converted by caustic potash, in the cold gradually, but when heated more quickly into manganate of potash.\* The solution must, however, be so dilute, that the liquid is sufficient to absorb the oxygen which is extricated. If the decomposition happens slowly, the quantity of the green increases gradually, the red at the same time diminishing till at last the liquid becomes quite green, and, during the change, a succession of colours is observed, produced by the mixture of the green and red in different proportions. From these alterations of colour this solution has been called *chameleon mineral*. If an acid be added to the green solution it becomes again red, binoxide of manganese at the same time being formed, and precipitated in the form of a brown powder.

One grain hypermanganate of potash, treated with nitric acid, and heated till the acid was completely decomposed, gave out 105.9 C. C. dry oxygen gas, or 0.1518 gr. oxygen. The filtered hydrated binoxide when ignited was converted into 0.4785 gr. of red oxide, which consists of 0.348 gr. manganese, and 0.1305 gr. oxygen. 0.348 gr. of manganese acquire 0.196 gr. oxygen to be converted into binoxide. Now,  $0.196 : 0.1518 :: 4 : 3.1$ . Hence, hypermanganic acid contains 7 atoms oxygen and 2 manganese. According to another experiment, 0.5 gr. hypermanganate of potash yielded 52.5 C. C. oxygen gas. In a third, 2 grs. of the salt gave 0.985 gr. of red oxide, which corresponds with 1.42 gr. of hypermanganic acid, and 1.295 gr. nitrate

\* According to Hopff (*Central. blatt, September, 1836*), carbonate of manganese answers better for the preparation of manganate of potash than the binoxide.—  
EDIT.

of potash containing 0.6077 gr. potash. Hence, 100 parts of hypermanganate of potash contain

	Oxygen.
Hypermanganic acid, . . . 71	35.2
Potash, . . . . . 30.135	5.1

The oxygen of the potash is, therefore, to that of the acid in this experiment as 1 : 69, whence, it follows that the true proportion is as 1 : 7. The composition of the hypermanganate of potash calculated after these data, is

Hypermanganic acid, . . . 70.53
Potash, . . . . . 28.47

Several experiments, which were made before I determined the best method, agreed very nearly with this result.

The hypermanganate of potash is little soluble in water, 1 part requiring, at 59° F., 16 parts of water to dissolve it. All the other salts of this acid are much more soluble, except the hypermanganate of silver, 1 part of which dissolves in 109 of water. I have not observed any insoluble salt which it forms. Hypermanganic acid has so great an affinity for potash, that it will, when combined with it, unite with no other base by double decomposition. For example, a solution of hypermanganate of potash may be mixed with a solution of chloride of barium, and the mixture evaporated. The hypermanganate of potash will crystallize along with the chloride of barium without any interchange of the acids taking place. Hence, the salt which hypermanganic acid forms with oxide of silver is the only one convenient for combining it with other bases.

If a warm solution of nitrate of silver be added to a warm solution of hypermanganate of potash, the hypermanganate of silver separates as the solution cools, in large, beautiful, easily measurable crystals. These crystals may be dissolved in water and re-crystallized, but the solution must not be boiled, or it will be decomposed, which does not happen if it be cautiously evaporated.

By means of the hypermanganate of silver the other salts of this acid may be formed, by adding to the crystals of this salt as much of the solution of any chloride as is necessary to decompose them. The crystals must previously be reduced to a fine powder, and they must be stirred for a long time with the solution of the chloride. The chloride

of silver must be washed by decantation, and if any of it get mixed with the solution, it must be allowed to sink to the bottom, for we cannot, as has already been mentioned, filter any of these compounds; we may, in this manner, obtain compounds of hypermanganic acid, which is a very powerful acid, with all the bases, except oxide of lead, protoxide of manganese, and protoxide of iron; but these bases deprive it of its oxygen, and become more highly oxidized.

Most of the salts of hypermanganic acid are very soluble in water, and deliquescent. Such, for example, are the hypermanganates of soda, lime, strontian, magnesia, zinc, copper, and many others. The hypermanganates of ammonia, potash, lithia, and barytes are the only salts which can be obtained in good and measurable crystals; which I shall immediately notice.

If the hypermanganate of barytes be dissolved in water, and a sufficient quantity of sulphuric acid be added to precipitate the barytes, we obtain free hypermanganic acid in solution in water. The solution has a deep red colour similar to that of the salts.

It being summer when these experiments were made, I could not succeed in concentrating the hypermanganic acid. It is decomposed, though very slowly, even at the common temperature of the air, and with great rapidity at a temperature from  $86^{\circ}$  to  $104^{\circ}$ , binoxide of manganese being precipitated, and oxygen making its escape. It is, as follows from what has already been said, not volatile.

This acid excels even the deutoxide of hydrogen in the facility with which it gives off oxygen. The different vegetable and animal pigments are immediately bleached by it. The salts have also the same effect, although in a less degree.

Hypermanganate of ammonia, is not liable to decomposition, but may be dissolved and evaporated to dryness. If, however, an excess of ammonia be added to any salt of hypermanganic acid, azote is immediately extricated, the ammonia and the acid being decomposed. I tried to determine the composition of the acid from the quantity of azote extricated, but I could not succeed, because there is also produced by the decomposition a compound of oxygen and azote.

What has previously been considered as manganic acid, was either hypermanganate of potash or of barytes.

#### 4. *Analysis of Hyperchloric Acid, and Hyperchlorate of Potash.*

As the salts of hyperchloric acid promised very important results respecting the connexion between the crystalline form and the composition of substances, I had prepared them long ago in considerable quantity. Hyperchlorate of potash is easily formed, by placing a vessel with sulphuric acid in the open air, and throwing into it, in small quantities at a time, fused chlorate of potash in fine powder, at the same time heating the sulphuric acid slightly. If one part of chlorate of potash be added to one part of sulphuric acid, both are completely decomposed, sulphate of potash, perchloric acid, and chlorous acid being formed. The chlorous acid is extricated either undecomposed or as chlorine and oxygen, and there is no danger to him who performs the experiment, if he take care not to breathe the gas which is extricated. The hyperchlorate of potash is not very soluble in water, while the bi-sulphate of potash is very soluble; hence, these salts may easily be separated by crystallization.

In determining the crystalline shape of the hypermanganate of potash, which, at the commencement of my experiments, I considered as an acid manganate of potash, I had convinced myself that it was the same as that of the hyperchlorate of potash. From this I suspected at first that hyperchloric acid contained only six atoms of oxygen. At any rate, it was evident that the experiments of Stadion, in which he found this acid to contain seven atoms of oxygen, would require to be repeated, as this proportion had never been observed in any other compound. This induced me, even before I had analysed the hypermanganate of potash, to investigate the hyperchlorate of potash.

The hyperchlorate of potash may be freed entirely from mechanical water by heat, particularly if it has previously been pounded. Only a low red heat is required to decompose it, but towards the end of the ignition it must be raised slightly. The chloride of potassium is at this temperature very volatile, and is partially carried away in vapour along with the oxygen, and carried with it out when it cools. To prevent this as much as possible, the operation must be conducted very slowly, and a long barometer tube, with a

bent tube attached to it, employed instead of a retort, to allow the chloride of potassium to be deposited.

0.600 gr. hyperchlorate of potash, yielded in one experiment 192.1 C.C. oxygen gas, or by weight 0.275 gr. The same quantity in another experiment gave 191.9 C.C. or 0.2749 gr. oxygen. Now,

$$(600 - 275) : 275. :: 100 : 84.73$$

If the hyperchloric acid contain 6 proportions of oxygen, then for every 100 parts of chloride of potassium there would be 75.04 of oxygen; but if it contain 7, then each 100 parts of chloride of potassium would be combined with 85.76 parts of oxygen. The same proportion was obtained by weighing the residue after the ignition of the perchlorate of potash. And, although this experiment from the escape of a small quantity of the chloride of potassium, can never be so accurate as the former, yet it agrees with it very well. 2.7155 gr. ignited lost 1.2515 in weight, so that for 100 parts of chloride of potassium there were 85.5 of oxygen.

5. *Crystalline form of some Hypermanganates and Hyperchlorates.*

It follows from these experiments, that Stadion's result is correct. The experiments on hypermanganic acid show that it also contains seven proportions of oxygen. The same difficulty which exists in the preparation of the hypermanganates, interferes also with that of the hyperchlorates. The hyperchlorate of potash is the most difficult to decompose of all the salts of that acid, so that the others can only be formed by means of fluosilicic acid. I precipitated the hyperchlorate of potash with the fluosilicates of ammonia, copper, lead, and several others, or else I decomposed the salt by the fluosilicic acid, and then combined the acid with a base.

All the hyperchlorates, except those of potash and ammonia, are very easily soluble in water. Most of them are volatile, as for instance, the hyperchlorates of soda, barytes, lime, copper, lead, and some more. The crystalline form of the salt of silver, which is also very soluble, is distinguishable, but not sufficiently so to measure. It is soluble in ammonia and forms with it a crystallizable compound. The only salts whose crystalline shape I have been able to measure accurately, are the hyperchlorates of

ammonia and potash, which are isomorphous with the corresponding hypermanganates. The primary form is a right rhombic prism. The crystal of the hypermanganate of silver is an acute rhombic prism. The form of the hypermanganate of barytes is completely similar to the anhydrous sulphate or seleniate of soda.

The same correspondence which exists between the crystalline shape of the hyperchlorate and hypermanganate of potash and ammonia, and that of the sulphates of barytes, strontian, and lead, is to be found also between the crystal of the hypermanganate of barytes and that of the sulphate of soda or silver. It would seem that the law of this circumstance, of which I have already given several examples at different times, lies very deep, and that its discovery will, perhaps, enable us likewise to calculate the crystalline form of compound substances from that of their elementary constituents.

The isomorphism of the compounds of hypermanganic and hyperchloric acids is an important fact with respect to the connexion between the crystalline form and chemical composition of substances, because it enables us to compare the greater part of the metals with several simple gaseous substances. Manganese in its lowest degree of oxidation being isomorphous with lime, oxide of copper, protoxide of iron, &c. The sesquioxide of manganese being isomorphous with the peroxide of iron, the oxide of chromium, and alumina; manganic acid being isomorphous with chromic acid, sulphuric acid, and selenic acid; and hypermanganic acid being isomorphous with hyperchloric acid, we may compare the above mentioned metals, sulphur and selenium, with oxygen, iodine, bromine, and chlorine.

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#### ARTICLE IV.

*New Demonstration of the Law of Mariotte, with Corrections of a former paper.* By THOMAS EXLEY, A. M.

(*To Dr. R. D. Thomson.*)

DEAR SIR,—I find there is a fault in one of the steps of the demonstration of the 2nd prop. relating to the composition of the forces, which I did not perceive till it was particularly pointed out to me by my friend, Mr. Horner, of Bath: I, therefore, wish to substitute another proof; I

had one which showed, that the prop. is true in general, if it is so in any particular case, and that case could easily be derived from experiment. However, in re-considering the subject, I discovered the true demonstration, first proving, from the theory, Mariotte's law, or the 4th prop. in the paper, and then the 2nd as a corollary; therefore, instead of the 2nd and 4th propositions please to insert as the 2nd the following, which, with the 1st cor., includes them both: the rest of the paper will then remain good, making only one or two very obvious changes in the references.

Before introducing the new proof, it may be observed, that in fig. 1, p. 273, the inner concentric spherical vessel V P W is supposed such as to confine the tenacious atoms, but to admit a perfectly free communication to those of the ethereal class. Now, it will easily be seen, that a certain number of tenacious atoms in the inner vessel will form a concentric spherical mass; and, when the number is such, that this sphere has a less radius than that of the vessel, there will be no compression of the tenacious atoms by the re-action of the surface V P W, the density of the tenacious atoms, at that surface, being in this case nothing. But when a greater quantity is introduced, there will arise a compression from the re-action of the surface, preventing the extension of the sphere of tenacious atoms.

*Prop. 2.* (prop. 4 of the paper.) If the pressure at T on the exterior vessel, fig. 1, be given, and a body of tenacious atoms in the inner vessel be compressed by a force, as at the piston P, but such, that the tenacious atoms may be kept apart by intervening ethereal matter; then, the compressing force at P will vary as the density of the body of tenacious atoms.

For, suppose the increment of density produced by an increment of the compressing force to be divided into a number of equal parts, and the increment of pressure into the same number of parts, such, that taken in order from the beginning, each shall produce one of the equal parts of the increment of density: let the number of these parts be increased, and, consequently, their magnitude diminished without limit. Then, taking  $u$  for the compressing force, and  $x$  for the density, the ultimate, or nascent ratio of the increments will be  $\frac{d u}{d x}$  in which  $d x$  is constant, since all

the parts of the increment of  $x$  are equal. Again, because the pressure at T is given, and there is a perfectly free communication of the ethereal atoms between the two vessels, and the tenacious atoms in the inner vessel are separated by intervening ethereal matter, the initial resistance to any increment of density, that is, the re-action of the ethereal atoms between those of the tenacious class is always the same, being in equilibrium with the given pressure at T, therefore  $d u$  is constant; hence, it follows,

that  $\frac{d u}{d x} = a$ , where  $a$  is a constant; therefore,  $u = a x$ ,

which needs no correction, because when the compressing force is nothing, the density at the surface V P W is nothing, as shown above: therefore,  $u$  is proportional to  $x$ , or the compressing force to the density is a constant ratio, which was to be shown.

*Cor. 1.* (prop. 2 of the paper.) The resistance between two tenacious atoms, which opposes an additional compressing force on the inner vessel, is inversely as the distance between the two atoms.

By this proposition the compressing force varies as the density, and the density varies inversely as the cube of the distance between adjacent atoms; therefore, the compressing force varies as the same cube; but it also varies as the number of atoms on a given surface and the force of each; now, the number of atoms on a given surface is inversely as the square of the distance; therefore, the force of each is inversely as the distance.

*Scholium 1.* It might, perhaps, at first seem strange to some, that the limit of the ratio of the compressing force and the density should always be the same, and yet, that the actual resistance to a definite additional compressing force should vary: but the difficulty will vanish by a little attention to the 1st prop., which shows that the tenacious atoms are invested with atmospherules of ethereal matter increasing in density to the surface of repulsion; hence, as these approach nearer together up to a certain term, there will be a denser and greater portion of ethereal matter to be acted on, and pressed against the spheres of repulsion, and forced out from between the tenacious atoms, than when these tenacious atoms are at a greater distance; hence,

the reason for a variation is manifest, and its law is seen from the above corollary, which is the proof of the 2nd prop. in the paper.

*Cor. 2.* During an increase of compression, ethereal atoms will be continually given out from the inner vessel, and during a diminution of pressure they will be absorbed.

*Cor. 3.* After a certain limit of pressure, two adjacent tenacious atoms will enter at once into union.

For, although the atmospherules of the tenacious atoms are more dense as we approach the surfaces of repulsion, yet, when these surfaces, after contact, intersect each other, the atmospherules will begin to be repelled from the line immediately between the centres; hence, a limit to an increase of assistance from intervening ethereal atoms will be attained, and union of the tenacious atoms will occur on a small additional pressure, for this effect will be promoted by the pressure, and the greatly increasing attraction of the atoms.

*Cor. 4.* Hence, when a gas liquifies by compression, the main body of the gas will become liquid at once.

For, in common circumstances, since the pressure is nearly the same throughout the vessel, the point of equilibrium will be attained very nearly at the same time in every part of the vessel.

*Cor. 5.* A little before liquifaction the ratio of the compression to the density will diminish.

For, when the equilibrium is just attained, the operation of the law ceases, since the ethereal atoms begin to be removed from the line joining the centres, and the resistance diminishes.

*Cor. 6.* The resistance to compression will be much greater in liquids than in gaseous bodies, and will follow a different law.

For, the repulsion between tenacious is vastly greater than between ethereal atoms; and the resistance in this case does not depend on the pressure at T.

*Cor. 7.* When a gas becomes a liquid, abundance of ethereal matter will be evolved; and the converse.

*Cor. 8.* The compressing force varies inversely as the volume, because the density varies in that ratio.

*Scholium 2.* That the density of air varies as the com-

pressing force, within certain limits, has been long established: it was usually called the Boyleian law, but now the law of Mariotte. Sir I. Newton, in the *Principia*, b. ii. p. 22, Sch., says, "But, as to our own air, this is certain from experiment, that its density is either accurately or very nearly as the compressing force." Modern philosophers have removed some sources of error, and verified the truth of the proposition through a very extensive scale of compression.

Now, it must be considered as a strong evidence of the defects of every received theory, that none afforded a solution of this simple phenomenon, not even when applied by the powerful mind of Newton, or Laplace, or the host of great names which have graced the annals of science since the times of Boyle and Newton. Hence, this proof of the proposition must be considered as sufficient of itself to stamp a character of truth on the theory from which it is obtained.

The corollaries are added as easy deductions, and because most of them are experimental results, not previously demonstrated from theory, the pressure at T may be considered as representing temperature; for it is very probable that ethereal matter at rest is insensible caloric and absorbed light; and the same in motion is sensible caloric, or light according to its velocity: see my Treatise on "Physical Optics." The terrestrial atmosphere, according to this theory, consists of tenacious atoms separated by ethereal matter, which extends, perhaps, several hundreds of miles above the highest stratum of tenacious atoms; and this ethereal matter will produce a pressure similar to that of T in common circumstances; while the same augmented or diminished by local causes answers to the same pressure exerted in a greater or less degree.

According to this rational view, cors. 2, 4, 5, 6 and 7, are theoretical proofs of well known experimental facts, which, I presume, cannot be explained on any other general principles. With my best respects,

I remain, dear sir, yours truly,

THOMAS EXLEY.

Bristol, Oct. 7th, 1836.

ARTICLE V.

On some Astronomical Methods of Observation. By WILLIAM GALBRAITH, A.M., Teacher of Mathematics, Edinburgh.

(Concluded from page 194.)

THE following pages contain the methods of registering the indications of the level. The first is that generally adopted by the French mathematicians, such as M. Puissant, and the other the method practised by the writer of this paper, which he thinks more simple, and is that employed in the preceding example, the first instance so far as he is aware of the obliquity of the ecliptic, being determined at Edinburgh, and that to a very great degree of accuracy by observation. In these means no errors or discrepancies amount to more than one-third of those in Rumkers observation of the latitude of the observatory of Paramatta, under Sir Thomas Brisbane, where it is stated, in *Phil. Mag.*, vol. i. new series, p. 302, that differences to the amount of 15" are to be found between observations on  $\beta$  Argus and those on the solstices and Zodiacal stars. See also the Brisbane Catalogue of Stars just published.

Method of M. Puissant, *Traite de Geodesie*, T. I. p. 165.

MARCHE DU NIVEAU FIXE.					
Object.	—	diff.	Observateur.	+	diff.
Observateur	+	a	Object.	—	b
	52	} — 3		52	} — 3
	49			55	
	51	} — 1		53	} — 1.5
	50			54.5	
	51	} — 1		54	} — 1
	50			55	
	51	} — 2		54	} — 2
	49			56	
	50	} — 1		55	} — 1
	49			56	

Somme des a = - 8                      Somme des b = - 8.5

Inclination du axe de rotation =  $\frac{a + b}{2 n} = - 0.825$

Factem constant, . . . . . 11.51

Product ou correction du nivean = - 9".50

## Method of W. Galbraith.

FIXED LEVEL.	
<i>e</i>	<i>o</i>
49	52
52	55
50	51
53	54.5
50	51
54	55
49	51
54	56
49	50
55	56

$$e = 515 \qquad 531.5$$

$$o = 531.5$$

$$e - o = -16.5$$

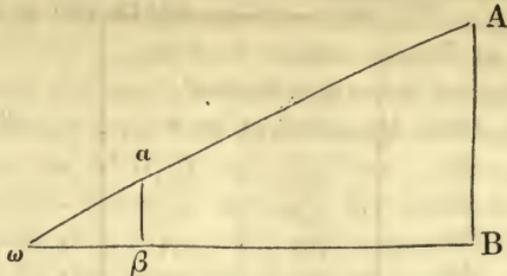
$$e'' = \frac{(e - o) a''}{2n} = \frac{-16.5 \times 11''.51}{20} = -9''.496 =$$

the correction of the level with its proper sign.

This last method is very easy when the divisions of the level read from a central zero, but cannot be so well practised by a scale having two zeros.

The preceding is the commencement of a series of observations on the obliquity of the ecliptic, which it is intended to be continued for some time, to ascertain the accuracy of a circle of very moderate dimensions. By the facility of repeating the observations, and the general stability of the instrument from the nature of its construction, much more accurate results may be obtained than is generally supposed. It is well known that the latitudes of different points in the trigonometrical survey were determined by a zenith sector of eight *feet* radius, while those of the French arc of the meridian were observed with one of Borda's repeating circles of about eight *inches* radius, and it is at least doubtful, which of the two is, on the whole, the more correct, the accuracy arising from the facility of repeating the observations in the latter case being nearly equivalent to that derived from the greater dimensions of the instruments in the former.

Determination of the Time of the EQUINOX by OBSERVATION.



Let  $\omega A$  be the ecliptic,  $\omega B$  the equator, and the angle  $A \omega B$  the obliquity of the ecliptic,  $\omega \beta$  the motion of the sun in *one day* from the equinoctial point  $\omega$ , and  $\alpha \beta$  the corresponding change of declination, which at the present time is  $23' 40''$  at the vernal equinox, and  $23' 24''$  at the autumnal, which it has been and will continue to be for some time, on account of the slow motion of the perigee and the action of the planets being nearly insensible, or at least very small.

By similar triangles,

$$\begin{aligned} \alpha \beta : \beta \omega &:: A B : B \omega, \text{ or} \\ 23' 40'' : 1^d &:: A B : B \omega \text{ in March.} \\ 23' 24'' : 1^d &:: A B : B \omega \text{ in September.} \end{aligned}$$

the time from the equinox in days and fractions, depending upon the observed declination  $A B$ .

The first analogy gives  $1^m \cdot 014085$ , and the second  $1^m \cdot 02564$  for each second of arc in the declination  $A B$ .

For days and decimal we would have, I., const. log.  $6 \cdot 847712$ , II.,  $6 \cdot 852633$ . It would be a little more accurate to take the change of declination from observations, by good instruments or from accurate astronomical tables, on account of the slight change in the daily variation, which we have assumed to be constant for a few days before and after the equinox.

Ex. September 25th, 1835, in latitude  $55^{\circ} 56' 56 \cdot 4$  and longitude  $12' 44''$  W, the observed zenith distance by Kater's

inch was . . . . .	56° 37' 5"·4 S
Lat. of 54 South Bridge . . . . .	55 56 56·4 N
Sun's declination South . . . . .	40 9·0 S

II. Constant logarithm . . . . .	6·852633
Dec. 40' 9" = 2409" log. . . . .	3.381837
Time after eq. $1^d \cdot 7158$ log. . . . .	0·234470

Day of Obs. 25

Equinox  $23^{\circ}28'42'' =$  Sept.  $23^d 6^h 49^m$  at Edinburgh,  
or  $23^d 7^h 2^m$  at Greenwich.

In this manner were determined,

1.	Sept. 18th, by observation	. . . . .	23 <sup>d</sup> .2785
2.	„ 23	. . . . .	.2692
3.	„ 24	. . . . .	.2806
4.	„ 25	. . . . .	.2842
5.	„ 26	. . . . .	.2699

Mean . . . . . 23.2765

h. m. s.

On Sept. 23rd, at . . . . . 6 38 10

Longitude in time . . . . . + 12 44 W

Time at Greenwich . . . . . 6 50 54

By Nautical Almanac . . . . . 6 57 6

Difference . . . . . — 6 12

which might arise from an error of 5" or 6" in the aggregate of the latitude and observed declinations, and on account of the unfavourable state of the weather in Sept. last.

WILLIAM GALBRAITH.

54, South Bridge, June 1, 1836.

ARTICLE VI.

*On Capillary Attraction, and on the disposition there is in Fluids to assume a Globular Form; with introductory observations upon some galvanic combinations connected with the explanation of these subjects.* By PAUL COOPER, Esq.

I HAVE lately published a sketch of a system of natural philosophy, which appears to me to connect the operations of nature upon general principles; and wishing to obtain the opinions of philosophers as to the correctness of my views, before I publish the work from which the abstract is taken, I beg to submit the following paper to your readers, although the subjects, I trust, are sufficiently important to claim their attention, partly for the purpose of illustrating the principles upon which it is founded.

It is assumed in this theory, that bodies are formed of

matter, consisting of globular atoms of different sizes, having an attraction for each other in the direct proportion of their bulk, or quantity of matter, and inversely as the square of their distance; and of light, consisting of globular atoms, constantly separated by a repulsive force, regulated by the same law of distance, uniform in size, and much smaller than the atoms of matter; for which they have an attraction, also regulated, both with regard to distance and dimensions, by the laws already mentioned.

The light under the influence of these laws must surround the atoms of matter, forming what we have called an atmosphere about each atom, and this atmosphere will be held in its position by various degrees of force, which will draw the atoms of light nearer to each other as they approach the atom of matter, and thus give it greater intensity. The atmosphere, it is supposed, will be divided into strata of different intensities, forming concentric spheres; every atom of light at equal distances from the centre, being held in its position by equal forces.

The point of saturation, under ordinary circumstances, will be when the repulsive force of the light is sufficient to counteract the attraction of the central atom of matter; when, with the exception of the attraction of the atoms of matter for each other, forming the force of gravitation, the atoms will be neutral; the two opposing forces of attraction and repulsion being equal at all distances.

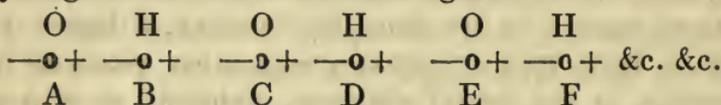
If, under these circumstances, an unlimited number of atoms of equal size are brought together, although every atom of light will act upon every other atom of light, agreeably to the law of distance, precisely as if it were unattached, it is evident that the atmospheres of the atoms of matter will continue uniform, because the forces which surround each of these atoms are every where equal. But, if atoms of different sizes, and, consequently, with different forces at the points of contact, are introduced to each other, the superior force of the light of the positive atom will repel the light of inferior force upon the negative atom, and both classes will acquire positive and negative surfaces on the opposite sides of the several atoms.

If, for example, we bring an equal number of atoms of oxygen and hydrogen together, the oxygen being positive

when compared with hydrogen, the superior force of the former will repel the inferior force of the latter, so as to render the surface of the hydrogen in contact with the oxygen still more negative; while, on the other hand, the light which forms the atmospheres of the oxygen will flow towards the hydrogen, and thus render the surface of the oxygen in contact with the hydrogen still more positive.

In this state there will be an attraction between the oxygen and the hydrogen; because the reciprocal attraction of the light of contiguous atoms is only neutralized by the repulsive force of the same light when the atmospheres of the atoms are uniform; for the attraction being as the sum of the forces upon the contiguous surfaces, every atom of light attracting every atom of matter, remains unchanged by an unequal distribution between them; whereas the repulsion being as the forces multiplied into each other, every atom of light repelling every other atom of light, is lessened by inequality, although the force taken from one surface be added to the other.

In consequence of this attraction, the oxygen will have an affinity for the hydrogen, and the atoms will alternately unite with each other; the positive surface of the atom of oxygen with the negative surface of the atom of hydrogen, and the positive surface of the atom of hydrogen with the negative surface of a second atom of oxygen, which will leave its positive surface ready to unite with another atom of hydrogen;\* as in the annexed figure.



Bodies which consist of single proportionals, whether solid or fluid, are formed upon the same general principles. The dissimilar atoms are united by the positive surface of the positive atom, and the negative surface of the negative

\* If we suppose the atoms of oxygen and hydrogen to be united by cohesion, this will form two particles of water. These two particles of water will be united to each other by the positive surface of the hydrogen of one particle, and the negative surface of the oxygen of the other particle; a very inferior force to that which unites the atoms to form the particles; and as a second atom of oxygen is held by an atom of hydrogen, to form a particle of oxygenated water, by the same force, as may be seen from the above description, it accounts for the ease with which the combination thus formed suffers decomposition.

atom to form particles; and the particles thus formed are united by the positive surface of the negative atom, and the negative surface of the positive atom to form lines of particles; which are again united, in the same order, in two directions perpendicular to each other and to the first line, to give to its length, breadth and depth. So that each atom must have three positive and three negative surfaces; and as the electrical forces by which they are generated are precisely equal, the atom of oxygen being in contact with an atom of hydrogen, and the atom of hydrogen in contact with an atom of oxygen at three points equidistant from each other, the polar forces produced by their action must also be equal; and, in the absence of other polar forces or disturbing causes, at right angles to each other.\*

There are some circumstances attending arrangements of this description, in which polar forces are generated by the action of contiguous surfaces in different electrical states, that deserve our attention. Whatever may be the nature of the bodies A B C D, &c., whether they consist of atoms of oxygen and hydrogen, or plates of zinc and copper, or of any other bodies, provided the alternations are different in electrical force, they will form a galvanic series with properties which, in many respects, are common to all.

It is evident, that however we extend the series A B C, &c., there will be a positive surface at one end, and a negative surface at the other unconnected. These surfaces, if in contact with the air, or with any other inferior conductor of derangement, will resist the electrical force of the bodies O H, O H, &c., and this resistance, re-acting upon every part of the series, will prevent the full derangement of the different surfaces which their inductive influence upon each other disposes them to acquire. If, then, we connect the negative surface of A with the positive surface of F, by means of an equally good conductor, with the bodies A B C, &c., the resistance being removed, these bodies will assume a state of derangement in equilibrium with the forces; to complete which, the light will flow,

\* This arrangement forms the three directions perpendicular to each other, which Fresnel calls axes of elasticity; and, as in the formation of water, by which it is here illustrated, the force is the same in all three directions, the light transmitted by this fluid is free from double refraction.

simultaneously, from the negative surface of A, through the conductor, to the positive surface of F, from the negative surface of B to the positive surface of A, from the negative surface of C to the positive surface of B, from the negative surface of D to the positive surface of C, from the negative surface of E to the positive surface of D, and from the negative surface of F to the positive surface of E; so that the actual quantity of light between these bodies will be constantly the same, but differently distributed upon the contiguous surfaces.

This is the foundation of galvanism; if we suppose the series A B C, &c., to be formed of alternate plates of zinc and copper, the zinc supplying the place of the atoms of oxygen, and the copper those of the atoms of hydrogen, upon connecting the positive surface of F with the negative surface of A, the light will flow through the conductor from A to F to complete the increased derangement of these surfaces, arising from the removal of resistance and their inductive influence upon each other. But the light which flows through the conductor, so far from circulating through the whole of the series, is only what is required to produce an equilibrium between the exterior surfaces of the *first section* of atoms upon the *surfaces* of the end plates; every other section of atoms in the interior of these plates, and throughout the series, being brought into the same state by an *equal* transfer between contiguous surfaces, in the manner we have already described. Upon breaking the communication between A and F, the plates return to their previous state, and are thus prepared to repeat the operation when the conductor is re-placed.

The great disparity in the lifting and sustaining power of electro-magnets arises from the reduction of the derangement of the magnet, when the circuit is interrupted by the removal of the armature, upon the principles here stated.

In consequence of the great increase of force derived from the completion of the polar circuit, bodies in a state of derangement are constantly disposed to produce the necessary communication between their opposite surfaces, through the medium of intermediate bodies, although they may be very inferior conductors. The magnetic curves formed in the air, and rendered visible by means of iron

filings, are of this description. These curves in the air continually endeavour to bring the two ends of the compass-needle together, to form a magnetic circuit by a more direct communication; and it is the predominant force of these curves, when their equilibrium is destroyed by the interposition of a magnet, that gives motion to the needle in obedience to its distant force.

These curves, or rather curves formed upon this principle, which can only give motion to the solid needle as a whole, acting in the same manner, produce the actual union of the unconnected surfaces of a line of fluid particles, such as are described in the figure, by bending the line so that the two ends may meet.

All bodies having polar forces are disposed to unite their terminating surfaces in this manner, and would do so if they had freedom of motion. But there is a force opposed to it, even in fluids; and it arises from the position of the poles, which being opposite to each other upon the several atoms, disposes the particles to form right lines; when, therefore, the line is bent so that its two ends may be united, this force brings it into a circle by the equality of its resistance. It is, in fact, in the state of an elastic rod, the two ends of which are brought together; and the forces that give to both a circular form are similar.

The union of circles of particles formed in this manner, which intersect each other upon the principle we have already described, produces globules, or drops; and it is the same arrangement, which is liable to unlimited increase from concentric layers of particles, that gives the rounded form to fluids generally, when they are not attracted by the containing vessel, and which is more particularly observable in mercury, and other metals when reduced to a fluid state by fusion; the latter case differing from the former, only in the flattened form of the larger quantity by the force of gravitation, from which globules are exempted by the greater force required to bend the lines which form their surfaces; upon the same principle that a rod of great length in proportion to its elastic force will bend by its own weight, when formed into a circle and placed in a vertical position; whereas a small part of the same rod,

brought into the same form, will, in addition to its weight, resist a considerable pressure.\*

The disposition which is found in fluids to assume a globular form, cannot be derived from any central force; for if there were any such force in one particle, from their exact similarity, there would be the same force in every particle; and these equal forms, disposing each of them to be the centre of its own system, would destroy each other.

It is only, however, in the absence of bodies with surfaces favourable to a union with the positive and negative terminating surfaces of the lines of fluid particles, that this circular arrangement is made; we have already observed that there is a force constantly opposed to it, and the moment such surfaces are presented, this force urges the fluid particles to resume their rectilineal position to form a union with them. In this state, the inductive influence of the positive and negative terminating surfaces of the fluid particles, produces a considerable degree of attraction between the fluid and the surface of the solid to which it is by these means united, and the lines of particles, which, for instance, stretch from one side of the containing vessel to the opposite side, adhere to these surfaces with considerable force.

In consequence of this adhesion, when the two surfaces to which the lines are attached are very near to each other, as in capillary tubes, the weight of these lines of particles is in a great measure removed from the surface of the fluid and transferred to the sides of the capillary aperture, like ropes stretched between two opposite walls; the included column, in consequence, presses with less weight upon the liquid surface than similar surrounding columns, and to

\* It is probable, that water in a gaseous form is divided into particles, consisting of an atom of oxygen and an atom of hydrogen; that, when it forms a visible vapour, whether under the appearance of clouds, or in steam, or dew, these particles are united into globules, varying in size under different circumstances, until they become too large for their external atmospheres of light to support them; and they descend in rain, or when frozen in this globular state, in the form of hail. Snow, on the other hand, and all those efflorescent incrustations of ice which are formed by the progressive crystallization of water floating in the atmosphere, are probably produced by the union of particles in right lines, the terminating surfaces of which being unconnected, are constantly prepared to unite with any other particles that approach within the sphere of their attraction.

produce an equilibrium, more of the fluid is forced into the aperture; this is suspended upon the same principle, until, by a succession of these supplies, the height of the column above the level of the surrounding fluid, gives it an additional weight, sufficient to counteract that which is transferred to the sides of the aperture.

When, instead of a capillary tube, the fluid occupies a vessel of considerable dimensions, the same adhesion to the sides takes place; but from the length of the lines they fall in the centre by the force of gravitation, and only exhibit the effect of lateral suspension near their terminations; where it is rendered evident, by causing the fluid which is thus connected with the sides of the vessel, to rise above the general level.

If the capillary aperture be formed by two parallel plates, placed at a little distance from each other, the experiment will include both cases; the lines suspended by the plates will be crossed by lines running in a transverse direction to the sides of the vessel which contains the fluid, and as the weight of the latter must be supported by the former, the rise will be only half what it would be in a tube with a square or circular aperture equal to the distance between the plates.

Light bodies floating upon any liquid which is disposed to adhere to them in this manner, are drawn together by the lines of particles which are formed between them, upon the same principle that the opposite poles of two magnets are drawn together: this is effected, not by their direct attraction, which is extremely small, but through the medium of intermediate bodies brought into a magnetic state by their inductive influence, the particles of which, when in a gaseous or fluid state, are displaced by their greater attraction for the magnets than for each other.

The principles here developed are of great importance in the economy of nature; but as the operations to which we more particularly allude, include the decomposition of fluids by electrical currents, a subject not now before us, their consideration will be deferred to some other opportunity.

It will be observed, that in this theory oxygen is considered positive, and hydrogen negative, and of course, all other bodies which are separated at the zinc surface are

included in the former class, and all which, are separated at the copper surface in the latter class.

If we refer to the figure, and suppose the atoms of oxygen to be plates of zinc, and the atoms of hydrogen plates of copper, and that the former are united to the latter in the usual manner, so as to form a galvanic series of three alternations of the metals; it is evident, that the zinc will present a negative, and the copper a positive surface in the cells between the plates B and C, and between D and E; and also, that when a conducting communication is made between A and F, the light will flow from the negative surface of the zinc plate A, to the positive surface of the copper plate F. It therefore appears, that although zinc is positive and copper negative, when water is decomposed by the galvanic action of these metals, its oxygen is evolved at a negative, and its hydrogen at a positive surface.

This arrangement derives considerable support, from its agreement with the refractive force of the different bodies which are thus placed in the positive and negative classes; the former being known to repel, and the latter to attract the light, when it falls obliquely upon their several surfaces.

It may be supposed that the subjects of this paper are not sufficiently prominent to become distinct objects of investigation; but it is by attention to minute particulars that we arrive at general conclusions; and to prove the soundness of such conclusions, we are frequently called upon to retrace our steps, by producing particular cases to which they are applicable; the magnitude of the subjects, under these circumstances, is not measured by our usual views of their importance, so much as by the ease and simplicity with which the theory applies to their explanation, and the opportunities they offer for its general illustration.

P. COOPER.

*Weston Super Mare, Oct. 6, 1836.*

*To the Editor of the Records of General Science.*

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#### ARTICLE VII.

*On Internal Prismatic Reflexion.* By Mr. GEORGE DODD.

*(To the Editor of the Records of General Science.)*

SIR,—The ingenious and interesting papers of your correspondent, Mr. Cooper, on the nature of white light, contain

many views and suggestions which merit careful consideration, inasmuch as they clash in many points with opinions which have received the stamp of age, and the support of great names.

This opposition to received opinions, so far from being viewed with distrust, ought to obtain for the writer a larger share of attention, as, in science, it is almost always necessary, before we can make real additions to our knowledge, to eradicate some previous notions, either ill formed or incomplete.

The principles endeavoured to be established by Mr. Cooper, appear to be these:—

1. *That White Light consists of three colours, Red, Green, and Violet.*

2. *That there are but three indices of refraction in any given substance, viz., one for each colour.*

3. *That the two preceding principles are sufficient to explain the phenomena of diffraction.*

4. *That, supposing the 2nd proposition to be correct, the dark intervals which would necessarily result in the prismatic spectrum, may probably be filled up by multiplied reflexion within the body of the prism.*

It is not my intention, at present, to offer any remarks on the general nature of the experiments contained in Mr. Cooper's papers, nor upon the evidence which he adduces in support of the three first propositions; I will merely confine my attention to the 4th, with a view to draw Mr. Cooper's notice to a few circumstances, which seem to militate against the correctness of his deduction.

The extent to which multiplied reflexion can be carried in the body of a prism, without exhausting the luminosity of the ray, appears never to have been made the subject of experiment; but I will endeavour to show, that be that extent great or small, it can never produce the effect surmised by Mr. Cooper.

In order the more clearly to shew this, I will at once assume, for the sake of argument, that the two first propositions are true: that white light consists of red, green, and violet, and that each colour has but one index of refraction: I will then detail reasons for believing that the

multiplo-reflected rays could not fill up the dark spaces which would result in the prismatic spectrum.

In most Optical works, the relative proportions between the different coloured spaces of the spectrum are given for two or more substances: but neither the refracting angle of the prism, nor the point of the side at which the ray enters, are stated as modifying the *proportions* between the colours: the spectrum may be longer or shorter according to the angle of the prism and the obliquity of the ray's passage, but the *relative proportions* are not altered: now, the points at which the internal multiplied reflexions take place, depend almost entirely on these three conditions:

1. The refracting angle of the prism.
2. The parallelism or obliquity of the central ray's path, with respect to the base.
3. The point of immersion, whether midway between the apex and the base, nearer to the base than to the apex, or nearer to the apex than to the base.

Let us suppose the prism to be of flint glass: the spectrum is then of a certain nature, nearly, if not quite, equal in its proportions, however the prism be held: that is, any mode of using it which would lengthen or shorten one coloured portion, would do so equally to the others: but, supposing there to be three colours and three indices of refraction, the rays, after multiplied reflexion within the prism, would emerge at every imaginable position with respect to the primary rays: the violet emersions, for instance, might be all on their proper side of the green in one experiment, while in another, they would be largely mingled with the red on the opposite side, *and this with the same prism*: the difference being due to a variation of the point at which the light enters, or a variation of the angle at which the primary ray passes through.

Mr. Cooper has shewn in a diagram that the effect which he attributes to internal reflexion *may* be produced, or at least, that all the emersions of any one colour may be parallel; but, I believe, he will find that that can only take place when these three conditions are observed:

1. The prism to be equilateral.
2. The point of immersion to be midway between the apex and base.

3. The central ray to pass through parallel to the base.

Now, if any one of these conditions were altered in the slightest degree, the *relative* points of emersion would be changed, and the appearance of the spectrum altered.

In order to shew this more clearly, I will trace the progress of a homogeneous ray (for the sake of simplicity) through an equilateral prism, under six different circumstances, three depending on the point of immersion, and three on the *direction* of its passage with respect to the base.

As we do not know to what extent these reflexions may go, I have traced them to twenty in each experiment; now, out of these twenty about seven will occur at the posterior face, and, as there is an emersion at every reflexion, there are at the posterior face seven emersions, which form the subject of the following table; the object of which is to shew the difference in the direction and the intensity of the ray occasioned by the six variations of the conditions before stated. By *intensity* I mean priority in transmission or emersion, for the numbers in the table denote the number of reflexions which have preceded the respective emersions, and a ray must obviously retain more luminosity after two or three, than after twelve or sixteen reflexions.

1. *Impinge equidistant from apex and base.*

Inclined in its passage $5^\circ$ towards base.	Inclined $5^\circ$ from base.
direct ray, & $6 = 5^\circ$ } inclin. $16 = 15^\circ$ } toward } base.	3, 13, 19 = $5^\circ$ } inclin. } toward } base.
3 & 9 = $5^\circ$ } inclin. $13 = 15^\circ$ } from } base.	direct ray, 6, } = $5^\circ$ } inclin. 10, 16 }       } from } base.

2. *Impinge nearer to apex than to base by  $\frac{1}{10}$ th.*

Inclined $5^\circ$ towards base.	Inclined $5^\circ$ from base.
direct ray, 6, 16 = $5^\circ$ } inclin. } toward } base.	3, 12, 18 = $5^\circ$ } inclin. } toward } base.
3, 9, 13, 19 = $5^\circ$ } inclin. } from } base	direct ray, 6, } = $5^\circ$ } inclin. 9, 15 }       } from } base.

3. *Impinge nearer to base than to apex by  $\frac{1}{6}$ th.*

Inclined $5^\circ$ towards base.		Inclined $5^\circ$ from base.
3, 9, 13, 19 = $5^\circ$	} inclin. } toward } base.	direct ray, 6, } 10, 16 } = $5^\circ$
direct ray, 6, 10 = $5^\circ$	} inclin. } from } base.	3, 13, 19 = $5^\circ$
		} inclin. } toward } base.
		} inclin. } from } base.

Thus, it appears, that if a homogeneous ray, red for instance, were traced through a prism, the positions of the different emersions with respect to each other would be most materially altered by any variation in the point of its entry, or the direction of its passage.

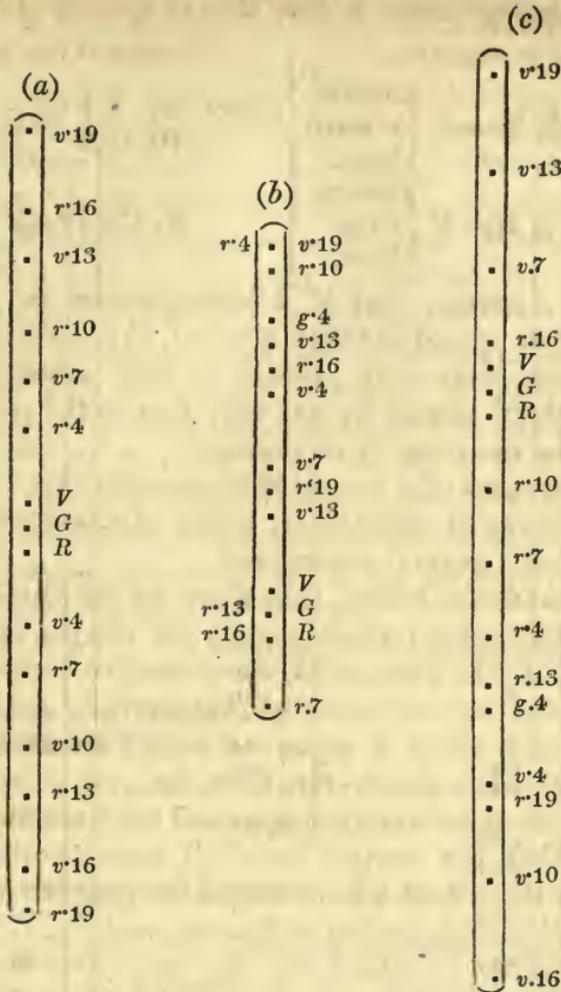
Having treated of a simple homogeneous ray, I will now proceed to a ray of *white* light, under circumstances more consonant with general experiment.

Let us assume, as before, that there are but three colours, and three refractive indices:—then let the prism be equilateral and of flint glass, with the refractive indices of the mean, and the two extreme rays, respectively 1.620, 1.600, 1.580 (which is about a mean between Fraunhofer's table and those of other observers): then, let a ray of white light impinge midway between the apex and the base, and at such an angle, that the central ray shall pass parallel to the base: with these data I have traced the progress of all the three rays through twenty reflexions, and fig. 1. (a) represents the points at which all the emersions would take place at the posterior face; in fact, it would be the form of the spectrum immediately on leaving the prism.

Let the white ray now impinge at a point nearer to the apex than to the base by  $\frac{1}{2}$ th, but let the central ray still pass parallel to the base: it will now be found that the spectrum immediately on leaving the prism would be shorter than before, and not only so, but the relative disposition of the colours very different, as in fig. 1 (b).

Let the case be now reversed, and the ray impinge nearer to the *base* in the same proportion, the parallelism being still preserved: the same process of tracing the ray through twenty reflexions would give us a spectrum as in fig. 1 (c).

Fig. 1.\*

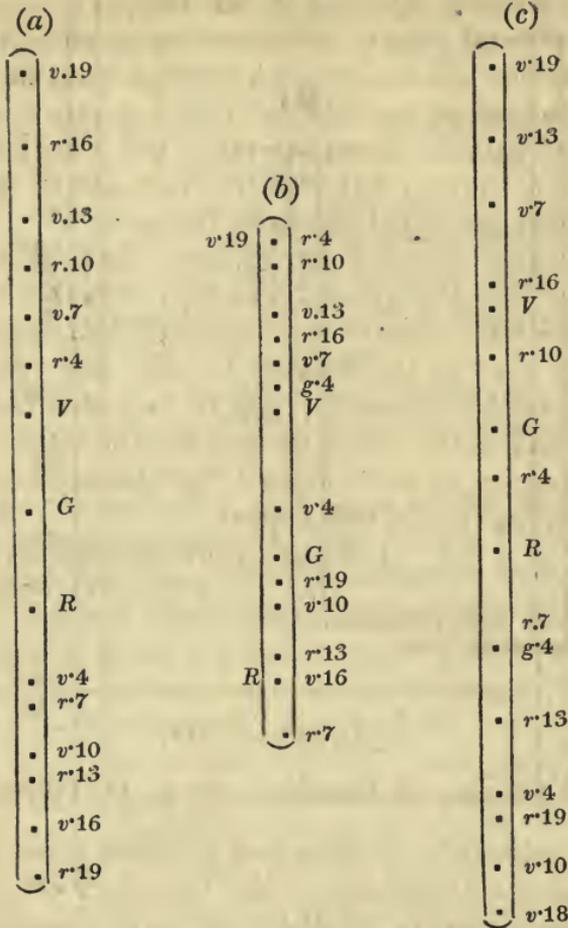


I next traced all those three spectra to a distance from the prism equal to six times the breadth of its face, and fig. 2 will give an idea of the great difference which this short distance would make in the nature of the spectra. Fig. 1 (a) and fig. 2 (a) are the same spectrum at two different distances, one close to the prism, and the other a few inches from it; so likewise are fig. 1 (b) and fig. 2 (b), as are also fig. 1 (c) and fig. 2 (c): fig. 2 is reduced to save

\* Fig. 1 represents the comparative length and chromatic distribution of the spectra immediately on leaving the prism; v, r, g, denote the colours violet, red, green; (the capitals V, R, G, being the primary or direct rays); and the numbers denote the number of reflexions which have preceded each individual emersion: in some few points two emersions coincide.

room, the principal object being to shew the relative construction of *a*, *b*, *c*.

Fig. 2.



Now, the question is, sir, are these discrepancies ever observed in practice? Do the mere facts of changing the point of incidence, changing the direction of passage, or varying the distance at which the spectrum is observed, make such great changes in the chromatic distribution of the spectrum? I think not. It is generally thought desirable that the central ray shall pass parallel to the base; but I am not aware that in any of Newton's, Fraunhofer's, or Brewster's experiments the mere position of the point of incidence, with respect to the apex and the base, was any matter of moment: as I am aware that parallelism to the base is generally sought for in practice, I have con-

ceded that in these six figures, but still find abundant changes in the spectra by the other means which I have detailed.

I have another objection to Mr. Cooper's supposition: if in the course of twenty reflexions seven emerge from the posterior face, the others must emerge from the other two faces of the prism, and thus form faint spectra from the horizontal and anterior faces, as well as the principal spectrum from the posterior; but this, neither reading nor experiment induce me to believe to be the case.

I will again observe, sir, in conclusion, that I do not now enter into the general question, whether the indices of refraction be three or innumerable? but merely attempt to shew, that if the former be the case, the requisite filling up of the spectrum *cannot* be produced by multiplied reflexion within the prism, because whether those reflexions be five, twenty, or one hundred, the discrepancies which I have described would still appear.

I am, sir, your obedient servant,

G. DODD.

52, Myddelton Street, Clerkenwell,  
August 31st, 1836.

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#### ARTICLE VIII.

*On some Silicates of Alumina.* By R. D. THOMSON, M.D.

To the compounds of silica and alumina great interest is attached, in consequence of the frequency of their occurrence, the diversity of their external characters, even when closely allied by chemical composition, and their utility in manufactures.

Notwithstanding the care which Berthier has bestowed on the subject, the arrangement of this class of minerals labours under great disadvantages, and it is questionable, whether our present knowledge is adequate to enable us to introduce that precision which has been applied to other bodies possessing analogy of chemical composition.

I was led to the investigation of the composition of these simple compounds, from having lately met with three species of minerals, which appeared to possess distinctive external characters; and could not be referred by these features with certainty to any described species. They were all

found in rocks connected with a red sand-stone, which is deposited in horizontal strata along the banks of the Tweed, in the neighbourhood of Melrose, and appears to be referable to the old red sand-stone series, or transition formation, being occasionally interrupted by dykes of greenstone and clay-stone porphyry. In the latter of these trap rocks, two of the minerals alluded to are found in considerable quantity, while the third, seems to occupy a place in the sand-stone itself.

I shall describe their characters and composition, and compare them with the simple compounds of silica and alumina which have been examined. The first of these minerals, I have termed *Tuésite*,\* from Tuésis the river Tweed. It occurs in veins in porphyry, or indurated sand-stone slate, which is intimately connected with the old red sand-stone. Its colour is milk white, opaque; lustre dull; sectile. Hardness 2.5. Specific gravity from 2.434 to 2.558.

Before the blow-pipe *per se*, it becomes blue and brittle, fusing with carbonate of soda into an opaque bead, and with borax and salt of phosphorus into a transparent glass. It forms an excellent slate pencil. A portion of the mineral was finely pulverized and fused with carbonate of soda. The silica being separated in the usual manner, the alumina was precipitated by caustic ammonia in the form of beautiful white flocks, which after determining its weight, was dissolved in sulphuric acid, with the addition of potash. Regular crystals of potash—sulphate of alumina were the result of the gradual evaporation of the solution. The liquid remaining after separating the alumina was precipitated by oxalate of ammonia. The product was a small quantity of lime. The residual liquor was evaporated to dryness, the dry salts heated to redness, dissolved in pure water and boiled with carbonate of soda; a precipitate of magnesia ensued. This precipitate being weighed, was dissolved in dilute sulphuric acid. The whole of it dissolved, with the exception of a minute portion of silica, scarcely appreciable, and which produced an amber coloured bead when fused by the blowpipe with carbonate of soda.† The water in one trial amounted to 13.5 per cent, in another to 13.2.

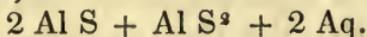
\* See description, Thomson's Mineralogy, vol. i. 214.

† This is a peculiar state of silica well known to chemists as occurring in the latter stage of analyses, which has frequently been mistaken for Titanic acid.

The constituents are

Silica, . . . . .	44.300	4 atoms
Alumina, . . . . .	40.400	3 ,,
Lime, . . . . .	0.755	
Magnesia, . . . . .	0.500	
Water, . . . . .	13.500	2 ,,
	<hr/>	
	99.455	

The formula, to represent its composition, which I am disposed to adopt, is



According to Berthier's view it would be,



If, however, the lime and magnesia were taken into account, and supposing them to re-place alumina, we should have, according to the formula of Professor Thomson,



Corresponding with this composition, we find several analyses; especially one by Boussingault, of a Halloysite, as he terms it, from Guatequa in New Granada, found in carbonaceous schist with anthracite, of a soft consistence with a cheesy fracture, becoming transparent in water; and two of Kaolin by M. Berthier.

	Halloysite from		Kaolin.	
	Guatequa.*	St. Yriex.†	Schneeberg.‡	
Silica, . . . . .	45	46.8	43.6	= 4 atoms.
Alumina, . . . . .	40.2	37.3	37.7	3 ,,
Potash, . . . . .		2.5		
Peroxide of iron,			1.5	
Water, . . . . .	14.8	13.	12.6	2 ,,
	<hr/>	<hr/>	<hr/>	
	100.0	99.6	95.4	

It is quite obvious that these correspond with the formula which we have already given. The halloysites of Berthier differ essentially in their external character from tuesite. The specimen from Anglar, near Liege, possesses a density of 1.8 to 2.00, or, according to Ingelspach Lariviere, from 1.82 to 2.09. § Fracture compact, waxy, conchoidal, pure white or blueish, scratched by the nail, taking a polish under the finger. It is found in veins of hydrate of iron

\* Ann. de Chim., liii. 439.

† Traite des Essais par la Voie Seche par M. Berthier, i. 58.

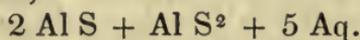
‡ Ib.

§ Ann. des Mines, v. 310.

mixed with galena, carbonate of lead and calamine, which traverse a transition limestone, in masses varying from the size of the fist to that of a cubic metre. Two specimens afforded,\*

	Anglar.	Hall.	
Silica, . . .	39	39	4 atoms.
Alumina, . . .	34	35	3 ,,
Water, . . .	26	25·5	5 ,,
	—	—	
	99	99·5	

Their formula is,



If then the lime and magnesia be considered foreign to the composition of tuesite, we shall have halloysite forming with it a sub-species of the same mineral, but possessing decidedly distinct characters, apparently occasioned by the addition of 3 atoms of water. And it seems necessary that the term halloysite should be restricted to those compounds, consisting of silica and alumina, in the proportion of 4 atoms to 3, with 5 atoms of water, while tuesite contains the same proportions of solids but possesses only 2 atoms of water.

2. *Bisilicate of Alumina or Fuller's Earth.*—This mineral is found in round masses in the bed of a stream, associated with clay-stone porphyry, near Maxton. Sp. Grav. 2·394.

With nitre, soda and salt of phosphorus, fuses before the blowpipe into an opaque mass. With borax, fuses into a transparent bead, pale amber coloured when hot, colourless when cold. Colour yellowish white; fracture earthy, soft, soiling the fingers; scratched by the nail, tuesite and sulphate of lime; adheres to the tongue like halloysite; contains crystals of decomposing felspar interspersed through the mass. Its constituents I ascertained to be,

Silica, . . .	57·105	4 atoms.
Alumina, . . .	31·850	2 ,,
Magnesia, . . .	2·615	
Water, . . .	7·280	1 ,,
	—	
	98·850	

and its formula  $2 \text{ Al S}^2 + \text{ Aq.}$

\* *Traite des Essais*, i. 58.

Berthier has included a mineral possessing exactly the same composition, with less water, under halloysite, and another under kaolin, as is exhibited in the following table:\*

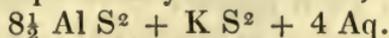
	Halloysite. Fahlun.	Kaolin. Normandy.	
Silica, . . . . .	46·8	50	2 atoms.
Alumina, . . . . .	26·7	28	1 ,,
Peroxide of iron, . . . . .	5·0	5·5	
Magnesia, . . . . .	0·4	·7	
Lime, . . . . .	3·0	5·5	
Water, . . . . .	13·5	9·5	1 ,,
Potash, . . . . .		2·2	
	<hr/>	<hr/>	
	95·4	101·4	

Berthier considers these two specimens as affording instances of felspar in different states of decomposition, and, although he states no circumstances which tend to establish his supposition, yet it is possible his conjecture may be correct. I could observe no fact, however, which could give the slightest countenance to the idea, that Tuésite or Fuller's earth, but more especially the former, are in any way connected with felspar.

3. In the sand-stone, which has been already described, a soft whitish substance occurs. It is smooth, yielding to the finger, containing greenish streaks, and answers to the description of lithomarge or rock marrow. The specific gravity is 2·457. Its constituents are, according to my analysis,

Silica, . . . . .	56·850	19·0 atoms
Alumina, . . . . .	25·000	7·5
Potash, . . . . .	6·178	1·
Lime, . . . . .	3·492	·5
Magnesia, . . . . .	2·640	·5
Water, . . . . .	5·840	4·
	<hr/>	
	99·900	

Its composition, including the lime and magnesia with the alumina, is expressed by the formula,



Berthier analyzed two Kaolins which approach litho-

\* Traite des Essais, i. 60.

marge in composition, magnesia taking the place of the potash.

Kaolin from St. Tropez,  $3 \text{ Al S}^2 + \text{M S}^2 + 2 \text{ Aq}$ .

Kaolin from Mende,  $4 \text{ Al S}^2 + \text{M S}^2$ .

For the sake of greater precision, I shall give a view of the composition of the various Halloysites and Kaolins analyzed by Berthier and Boussingault, including along with them the minerals analyzed by myself, and expressing their constitution by formulæ.

I. 1. Tuésite,  $2 \text{ Al S} + \text{Al S}^2 + 2 \text{ Aq}$ .

Under this species are comprehended Halloysite from Guatequa analyzed by Boussingault, and two Kaolins from St. Yriex and Schneeberg, examined by Berthier, and Clay of Angleur.

2. Halloysite,  $2 \text{ Al S} + \text{Al S}^2 + 5 \text{ Aq}$ , including the Halloysites of Anglar and Hall, and, perhaps, Nontron.

II. Bi-silicate of Alumina or Fuller's earth,

$2 \text{ Al S}^2 + \text{Aq}$ , and  $\text{Al S}^2 + \text{Aq}$ ,

expressing the composition of Halloysite from Fahlun, Kaolin from Normandy, and Fuller's earth from the Tweed. Kaolin from Meissen possesses less water =  $3 \text{ Al S}^2 + 2 \text{ Aq}$ .

III. Linzinite is the name which John originally gave to the Halloysite of Hall. It may, with propriety, be applied to the ter-silicates of alumina, whose composition is denoted by

$\text{Al S}^3 + 3\frac{1}{4} \text{ Aq}$ .

Berthier analyzed two Halloysites from St. Sever and Confoleus possessing this constitution, besides clays from Hoeganas, Forges, Montereau, Cymolite, Cologne, with less water.

IV. Lithomarge,  $8\frac{1}{2} \text{ Al S}^2 + \text{K S}^2 + 4 \text{ Aq}$ .

Under this species, we may, perhaps, include the Kaolins from St. Tropez and Mende whose expressions have been already exhibited.

In presenting the preceding arrangement, it is only my intention to assist in simplifying the study of these interesting compounds; for it appears more beneficial to classify minerals, not according to theoretical views, but according to their actual nature. M. Berthier, whose analytical accuracy none will dispute, has thrown all the simple

silicates of alumina into one class. But, it may be asked, are none of these compounds as much true species as other minerals with which we are acquainted, or, if they are different states of decomposing felspar, are they not in this respect analogous with many species, which have been presumed with considerable probability, to be the products of decomposing rocks?

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ARTICLE IX.

*On Resins.* By HENRY ROSE.\*

THE remarkable phenomena of isomerism are observed in much greater abundance in organic than in inorganic bodies. The laws, however, which regulate isomeric modifications among inorganic substances, can, from the small number of atoms of which they are composed, be more easily determined, than is possible with respect to organic substances, which contain a greater number of atoms; though these are probably more constant than analysis has hitherto determined them to be. It is also possible that this great number of elementary atoms may occasion many discoveries on the laws of isomerism. At all events it appears to me that the investigation of isomeric organic substances, and their relations to other bodies, belong principally to organic chemistry.

In this point of view the experiments of Blanchet and Sell,† on volatile oils, are incontestibly very valuable. They found that a great number of those volatile oils, which contain no oxygen, have the same composition, and the experiments of other chemists, made at the same or a later period, have increased the number. According to Blanchet and Sell the following volatile oils are isomeric: 1st and 2nd, the two oils which compose oil of turpentine, and which they have named *Dadyl* and *Pencyl*: 3rd and 4th, the two oils called by them *Citronyl* and *Citryl*, which form oil of lemons. To these may be added oils of turpentine and lemons themselves, unless, as is very probable, we consider them as mixtures. From the experiments of Blanchet, it appears that we must add to the above the

\* Translated from Poggendorff's *Annalen*, xxxiii. 33.

† *Pogg. Ann.*, xxix. 133.

following, 5th, the oil of the balsam of copaiva; 6th and 7th, the two oils which compose the oil of juniper, extracted from unripe fruit;\* according to Ettling† 8th, oil of cloves, and 9th oil of valerian have also the same composition, when they are separated from the acids with which they are combined.

The most of these are certainly not mixtures of several substances, for they have been united with muriatic acid so as to form crystalline compounds, some of which have been often examined. The compounds which they form with oxygen have been much less frequently examined than they deserve, since they belong in part to the most common kinds of resins.

Blanchet and Sell have examined colophonium, and found that it might be considered as an oxide of oil of turpentine. But when the experiments of Unverdorben and of Ries determined colophonium to be composed of two resins, one of which could be obtained in crystals, they considered that substance as a mixture of isomeric resins.

Besides that which exists in colophonium, several other crystalline resins have been detected, and their investigation is peculiarly interesting, as they are pure compounds, and are not formed of a mixture of several different substances.

Now, as resins appear usually to be formed by the oxidation of volatile oils, it appeared to me to be important to ascertain the relation in composition which exists between the resins obtained from isomeric oils. This investigation can be carried farther than that of the oils themselves, because the most of the resins formed from oils, as Unverdorben has long ago shown, behave as acids, and are capable of forming saline compounds with inorganic bases. I have examined a few crystalline resins of both these kinds, and though they might undoubtedly have been increased, I have here communicated my results.

#### RESINS WHICH HAVE THE PROPERTIES OF ACIDS.

##### *Resin from Balsam of Copaiva.*

No resin can be obtained in such beautiful crystals as this. Schweitzer first gave a method of obtaining it in the

\* Ann. der Pharmacie, vii. 154. Pogg. Ann., xxxi. 526.

† Ann. der Pharmacie, ix. 68. Pogg. Ann., xxxi. 526.

crystalline state, and he at first believed that it was a combination of copaiva with ammonia,\* but he afterwards found that the ammonia was only formed during the process, and that crystallization from alcohol alone was required to obtain the resin in a state of purity.† The crystals are combinations of two oblique 4-sided vertical prisms.

Parallel to one of the faces, there are traces of cleavage, which, however, are difficult to detect from the extreme softness of the crystals. These are pure white, the smaller transparent, the larger translucent, or only so, at the edges; they are all very soft.

This crystalline resin is more soluble in strong boiling alcohol than in cold. The spirituous solution reddens litmus paper. This resin combines with inorganic bases, and the compounds which it forms have all the properties of salts.

0.509 grammes of crystallized resin were put into Leibig's apparatus with oxide of copper. They yielded 0.464 grms. of water and 1.459 of carbonic acid. Hence, their composition is the following:

Carbon, . . . . .	79.26
Hydrogen, . . . . .	10.15
Oxygen, . . . . .	10.59

This composition agrees with that which Blanchet and Sell have found for common resin, which, according to them contains 79.65 carbon, 10.08 hydrogen, and 10.27 oxygen. Colophonium is an oxide of oil of turpentine. Now, since the composition of this last agrees, according to Blanchet, with that of volatile oil of copaiva, we must also consider resin of copaiva as an oxide of this oil. We see also, that isomeric bodies, when oxidized, yield isomeric oxides.

The composition of copaiva resin as well as that of colophonium may be expressed by the formula,  $10C + 16H + O$ ;‡ calculating from this, its constituents would stand thus, carbon 79.275, hydrogen 10.355, oxygen 10.37.

If we mix a solution of copaiva resin in alcohol with ammonia, there appears, as is usual with all solutions of the acid resins, no precipitate. If the resin be thrown

\* Pogg. Ann., xvii. 487.

† *Ib.*, xxi. 172.

‡ Rose considers the atom of hydrogen one-sixteenth of that of oxygen.—EDIT.

down from the solution by means of water, the precipitate is easily dissolved by the addition of ammonia. A solution of potash in alcohol produces no change in the alcoholic solution of the resin. In like manner, the alcoholic solution of the resin may be mixed in any proportion, with a concentrated aqueous solution of potash without any precipitation; but, if water be added to the solution, then the combination of the resin with the potash separates from the excess of potash.

An alcoholic solution of resin is not precipitated by solution of nitrate of silver in alcohol. But if some ammonia be added, a combination of the resin and oxide of silver falls down, which is easily soluble in an excess of ammonia. The solution of the resin behaves itself towards a solution of oxide of silver exactly as an acid, which forms with it a difficult soluble or insoluble compound, but soluble in free acid, and in free ammonia. The precipitate is crystalline, and retains its shape after drying. It is not absolutely insoluble in alcohol, though it is very slightly soluble in that liquid. By the action of light it is blackened like the other salts of silver. At a moderate heat, it melts like a resin; at a higher, it is decomposed, and leaves after the combustion of the cinder metallic silver.

In three experiments, I obtained from quantities, which were prepared at different times, by combustion, the following quantities of silver.

I.	0.308 grammes of the compound gave	0.081 grms. silver
II.	0.321	0.082
III.	0.376	0.096

Hence, the composition per cent. was as follows:

	I.	II.	III.
Oxide of silver, . . . . .	28.25	27.41	27.40
Resin, . . . . .	71.75	72.59	72.60
	100.00	100.00	100.00

If the resin combines, without decomposition, with the oxide of silver, then it contains in this compound four times as much oxygen as the oxide of silver, for, in the above table, the mean proportion of oxygen contained by the oxide of silver is 1.9, and by the resin 7.5. A compound of the resin and oxide of silver, in which the oxygen



In this compound, also, there are four times as much oxygen in the acid as in the base. A compound of copaiva resin and oxide of lead, in which the proportions of oxygen are as 4 : 1, would be composed of 26·56 oxide of lead, and 73·44 resin.

The quantity of resin, in the portions of the compound which were examined, is rather less than it ought to be from this calculation, because a small quantity of carbonate of lead easily falls down from the alcoholic solution of the acetate, unless atmospheric air be entirely excluded.

The alcoholic solution of copaiva resin gives no precipitate with a solution of chloride of calcium in alcohol. But, if the solution be diluted with water, a white precipitate separates, which cannot consist of pure resin, because it is insoluble in alcohol. The lime which this precipitate contains can be separated by long continued washing. But a more permanent compound of this resin and lime can be obtained by adding ammonia to a mixture of the alcoholic solution of copaiva resin with an excess of the alcoholic solution of chloride of calcium, and allowing to precipitate, which appears to fall down in a corked vessel, so as to prevent its being mixed with carbonate of lime. It must afterwards be filtered in such a manner as to prevent the access of air, and washed with water as long as it does not act upon lime.

0·4195 grms. of this precipitate were cautiously ignited; the residue, when treated in the usual way, with a solution of carbonate of ammonia, weighed 0·062 grammes, and was carbonate of lime.

Hence, the compound of the resin with lime contains per cent.

Lime, . . . .	8·32
Resin, . . . .	91·68
	100·00

This compound agrees with the former analogy, the oxygen in the base is one fourth of that in the resin. Such a compound of copaiva resin and lime would contain per cent. 8·45 lime, and 91·55 copaiva resin.

Now, since the three described and examined combinations of copaiva resin with oxide of silver, oxide of lead,

and lime are analogous in composition, although they were prepared in quite different ways, this resin would appear to form only one set of saliform compounds with bases, and in this set the oxygen in the resin is four times as great as that in the base. It appears to follow from this, that the atomic weight of the resin is four times as great as it is made by the formula given above. The right formula, for the composition of the resin, is, therefore, not  $10\text{ C} + 16\text{ H} + \text{O}$ , but  $40\text{ C} + 64\text{ H} + 4\text{ O}$ . It appears to me to be convenient, and, probably, also correct, to express the last formula as follows,  $4(10\text{ C} + 16\text{ H}) + 4\text{ O}$ .

(To be continued.)

#### ARTICLE X.

#### *The Art of Dyeing.*

(Continued from page 208.)

This mode of mordanting is very suitable for the production of a saturated logwood blue, which is more permanent than logwood violet; as logwood colours containing copper are far more permanent than those containing simple alum mordants.

#### BROWN AND BLUE FROM ALUM MORDANTS, AND COPPER MORDANTS, WITH WILLOW BARK AND LOGWOOD.

1. *Brown from Alum and Copper Mordants and Willow Bark.*—The process of mordanting cotton for willow bark brown has been already given. The colour is darker in proportion to the length of time that the calico remains in the copper mordant. The bark of the basket,\* or also the crack† willow should be first boiled in a little water; then the proper quantity is added, and the dyeing performed without allowing the solution to boil completely.

By boiling with soap-suds the dye loses its powdery appearance, and becomes still darker.

#### PROPERTIES OF WILLOW-BROWN.

Light and air, as well as washing with soap, produce no considerable change upon this colour.

\* *Salix viminalis*.—Ehrr.

† *Salix fragilis*.—Ehrr.

*Solution of potash* forms a yellowish spot. If the whole piece is immersed in a weak solution of potash the shade becomes light.

*Lime water* has no action.

*Ammonia* renders the colour brown.

*Vinegar* produces spots of a light brown colour.

*Lime juice* forms light brown spots which ammonia does not completely remove.

*Both tin mordants* discharge a light brown.

*Solution of chloride of lime*, when printed on it, has no action. It produces a scarcely perceptible brown shade.

*Remark.*—In the use of this dye, a gummy matter is formed in the solution, by which it will be weakened. It is necessary, therefore, to employ an excess of willow bark. With birch bark a similar colour is obtained possessing analogous properties.

## 2. BLUE FROM ALUM AND COPPER MORDANT WITH LOGWOOD.

The method of mordanting for logwood blue has been already given. The blue is darker in proportion to the length of time that the mordanted calico remains in the copper mordant. An excess in dyeing is proper for giving a clear colour. A greater proportion of bran is also useful. The following proportions may be used:—12 lbs. mordanted calico, 3 lbs. logwood, and 30 lbs. bran.

The dyeing solution, which, after the dyeing, still contains much colouring matter, may be again employed for the production of the same colour, while yet logwood and bran are added.

The dyeing should be operated slowly, and the temperature of the solution should not exceed 167°. If it is raised to a boiling heat a violet hue is produced. When 32 lbs. of cloth are boiled with 1 lb. soap in 2000 lbs. water the dye takes up soap and becomes darker, and the solution of soap becomes only slightly reddish. If the proportion of soap is increased, it acts upon the dye, and the calico becomes paler as with 4 lbs. soap to 32 lbs. cloth.

### PROPERTIES OF LOGWOOD BLUE.

This colour is, as formerly stated, pretty permanent to light and air in consequence of its containing copper.

When boiled with soap-suds (1 soap and 200 water) for a quarter of an hour, the violet blue colour of the dye is converted into a greenish blue. This withstands for a long time soap-suds.

*Solution of potash* makes green spots, which after being treated with vinegar remain white.

*Lime water* has no action.

*Ammonia*, diluted with water, forms a purer blue.

*Lime juice* produces orange-yellow spots, which vinegar only so far removes as to make the places approach nearer to a light blue.

*Tin mordant*, No. 1, discharges a purple.

*Tin mordant*, No. 2, acts in the same way, only the colour is less clear.

*Solution of chloride of lime* (1 to 40 water) has no action.

#### EMPLOYMENT OF COPPER MORDANT AFTER DYEING.

As the ammoniuret of copper combines with the mordanted calico, in the same way it combines with the dyed cloth, and in this respect affords an unlimited field to the dyer for varying his shades. No shade is produced which is not altered by immersion in the copper mordant, No. 2.

Copper mordants having alumina grounds, act quite differently upon logwood colours. They change the logwood violet blue to a disagreeable colour; while the blue after some time acquires a gray hue.

Compounds also of alumina, with substances containing tan, as the brown from birch, alder, maple, oak and willow barks, undergo various changes by the use of copper mordant.

Madder-red imparts to the copper mordant a purple colour, which is equal to that produced by the addition of logwood; being, however, much more permanent.

Berry yellow is not much altered. It remains yellow but loses all lustre. Quercitron yellow assumes a greenish hue.

Tan yellow produced with acetate of alumina mordant, as well as yellow wood yellow becomes brownish yellow. On the contrary, fernambuc red acquires a scarlet colour, which is more durable than the pure red.

To what has been already stated, it may be added, that the colours cleared or shaded by ammoniuret of copper,

are not destitute of lustre, and earthy as is frequently the case when it is employed as a mordant before dyeing. They retain their lustre much more; but no pulverulent oxide of copper collects on the surface.

Therefore, the copper mordant, No. 2, requires no mordant to be added to it.

The copper mordant exhibits here also remarkable property. It makes the colours light and prove permanent to soap. The logwood blue formed with copper mordant, No. 2, remains after 60 hours exposure to the sun at Berlin, in June, completely blue, while the violet blue at the same time changes to a dirty brown red.

The mode of using the copper mordant is here the same as above described, except that in order to render it permanent the cloth should be well moistened before it is placed in the mordant, otherwise spots will be formed.

#### ACTION OF SALTS OF TIN UPON CALICO IMPREGNATED WITH ALUM MORDANT.

If calico impregnated with acetate of alumina mordant, No. 1, and well rinsed, be passed through a solution of 1 part salt of tin in 200 water, allowing it to remain as short a time as possible, and washing it afterwards in water, we obtain, after dyeing 9 parts cloth with 12 madder and 36 bran, the usual madder-red of such a lustre as cannot be obtained in any other way. But the dye only succeeds on a small scale. It is an important point that the calico shall remain for as short a period as possible in contact with the salt of tin. If it remains longer, the acid of the salt of tin dissolves the alumina, and the madder-red will be much deteriorated. This cannot be avoided on a large scale. Diluting the solution of tin with water does not remedy this defect, as it is accompanied with the precipitation of oxide of tin, and consequently with an acidification of the solution. Alkaline solutions of tin do not succeed better. A solution of salt of tin in caustic lye is not at all practicable, as it dissolves the alumina, and takes it away so completely from the calico that on dyeing with madder a madder-red is not produced, but only an indistinct pink. The solution of salt of tin in ammonia does not act upon the alumina of the calico, but it has no advantageous action

upon the madder-red, as some oxide of tin precipitates from the milky solution upon the calico fibre and injures the lustre of the red.

ACTION OF THE ALUM MORDANT UPON THE IRON MORDANT.

The action of calico impregnated with acetate of alumina is not less remarkable than that of the copper mordant. Let the mordanted calico which has been hung up several days and well washed, be placed in a solution of 10 lbs. iron alum in 1200 lbs. water, and remain there for a quarter of an hour, it acquires a dark rust yellow colour, while a piece of unmordanted calico, treated in the same way, becomes only slightly yellow. As the sulphuric acid of the iron alum is in a state to dissolve the alumina of the calico, it is rather remarkable that this does not occur in the present instance. The alumina rather forms with the oxide of iron, a double combination on the calico. This more especially takes place when a solution of iron alum several months old is employed, as by the precipitation of much oxide of iron it has become acid. Since the iron alum solution is colourless, it is interesting when two pieces of calico are placed in the solution, to observe the one impregnated in the alumina becoming gradually dark rust yellow, while the other (an unmordanted piece of calico) remains quite white.

Numerous experiments have already proved the affinity existing between the earths and oxides, but it is impossible to shew it more clearly by experiment than as in the present instance by means of calico.

This affinity of alumina for oxide of iron explains the utility of previously boiling the cotton with alum, when for the purpose of dyeing it with prussiate of potash it is impregnated with iron salts.

It is convenient for the dyer to employ alum mordants mixed in different proportions with iron mordants.

ACTION OF IRON MORDANT UPON COPPER MORDANT.

A piece of cotton impregnated with iron mordant, No. 2, and well rinsed, alters by being placed in copper mordant, No. 2, only a little while; it loses the deep yellow lustre of the rust colour and appears paler yellow.

*(To be continued.)*

## ARTICLE XI.

## ANALYSES OF BOOKS.

*The Botanist; containing accurately coloured Figures of tender and hardy Ornamental Plants, with Descriptions.* Conducted by B. MAUND, F. L. S., assisted by Professor HENSLOW, 4to., to be continued Monthly, No. I.

As it is the province of this journal to take cognizance of every thing tending to promote science, we hold such a work as the above-named to come legitimately within the sphere of our notice. An occasion for commencing a new publication of this kind presented itself at the present time, as a blank in the illustrative series of botanical works, had been created by the cessation of the Messrs. Loddiges' Botanical Cabinet—a blank which seems likely to be ably filled up by this its successor, for we perceive, that these gentlemen, with their characteristic liberality, and zeal to forward science, have thrown open their splendid collection to the conductors of "The Botanist:" so that the beauties of that sanctuary, by being reflected in the faithful mirror of these plates, will give pleasure to thousands to whom the originals are unapproachable. Many other proprietors, both of public and private collections, and cultivators of plants appear to have given extensive facilities for the selection of subjects, to the artists engaged in it. The skill and ability of these seem to render vain and nugatory the lamentation of the poet; that nothing can give back the hour of splendour in the grass, of glory in the flower; for the pencil of the artist can give permanence to the former, and endurance to the colours of the most delicate and transient of the offspring of Flora. Nor is this all, the productions of every region of the earth are here brought together, and made to ornament our drawing-rooms and libraries, and in a state too, in which, unlike the originals, they require little space, and no care.

Such a manual, cannot but be acceptable to many already engaged in the study or cultivation of plants; while, for those who have yet to begin these interesting pursuits, let them take this as their companion, and as soon as they enter upon the domain of the goddess of flowers, they will see numerous vistas open up, and extend before them

"In long perspective of delight."

If the succeeding numbers of this work equal the first, in the selection and treatment of the subjects, and interesting nature of the information in the descriptive part, we cannot doubt its utility or success.

## ARTICLE XII.

## SCIENTIFIC INTELLIGENCE.

I.—*British Association for the Advancement of Science.*

## SECTION C.—GEOLOGY AND GEOGRAPHY.

*President*, Rev. Dr. Buckland.—*Vice-Presidents*, R. Griffith, Esq., G. B. Greenough, Esq., (*For Geography*) R. J. Murchison,

Esq.—*Secretaries*, W. Sanders, Esq., S. Stutchbury, Esq., T. J. Torrie, Esq., (*For Geography*) F. Harrison, Rankin, Esq.—*Committee*, H. T. De la Beche, Esq., M. Van Breda, — Carne, Esq., Penzance, Edward Charlesworth, Esq., Major Clerke, Lord Cole, Rev. William Conybeare, Rev. William Hopkins, Robert Hutton, Esq., Boscowen Ibbotson, Esq., Rev. T. T. Lewis, James Macadam, Esq., Sir George Mackenzie, M. Van der Melen, Lord Northampton, Professor Parigot, Professor Phillips, Professor Sedgwick, William Smith, Esq., John Taylor, Esq., Dr. William West, Samuel Worsley, Esq., Rev. James Yates.

*Monday, 22nd August.*—1. The first paper at this Section was read by Edward Charlesworth, Esq., entitled “A Notice of the Vertibrated Animals in the Crag of Norfolk and Suffolk.”

The author’s principal object in bringing forward this subject, was to establish the fact, of the remains of mammiferous animals being associated with the mollusca of the tertiary beds above the London clay, in the Eastern Counties of England. These remains are confined to a part of the Crag formation, which appears to extend from Cromer in Norfolk, to within a few miles of Albro’, in Suffolk. The bones of fish, and a large proportion of the testacea, that are met with in this stratum, differ widely from those in the coralline beds, and from that part of the clay deposit which skirts the southern coast of Essex and Suffolk. Among the mammalia, which the author stated really to belong to the Crag, is the *Mastodon angustidens*, of which several teeth have recently been obtained in Norfolk, from localities adjoining the parish of Withingham, the spot in which Dr. W. Smith states the specimen to have been found, which is figured in his “Strata identified.”

The author next noticed the discovery of the mineralized remains of birds, which he had obtained from several Craggs in the same district.

Among the fish, the most remarkable is the *Charcarias Megalodon*, the teeth of which are found in Suffolk, equalling in size the specimens brought from the tertiary formations of Malta.

No traces of the existence of Reptilia have yet been detected in the clay, which would rather support the opinion, that the climate during the Crag epoch was analogous to that of the Polar regions.

It appeared to him, that the whole town of Cromer stood on a chalk pebble. He hesitated to name the circumstance to any geologist, but some time afterwards he submitted it to Dr. Buckland, who confirmed his view of it. He should be glad to know if any gentleman had paid attention to the subject; and should any one visit Norfolk, it would be desirable to ascertain if there was any connexion between the town of Cromer and the stratum, or whether it stands on a detached mass.

Mr. Murchison, said he had come to a different conclusion, as to the great pebble on which the town was supposed to stand. In Lyell’s book there are two little sections of that district, by which it is clearly made out, that the chalk rises up from the stratum in immense masses. In conclusion, he begged to compliment Mr. Charlesworth on his interesting paper.

2. *Cefn Caves*.—Mr. Bowman described the Cefn Caves in Denbighshire. They are situated in limestone. Their roofs are covered with stalactites, and their floors with animal matter, principally the clytra of beetles, bones, and teeth.

*Tuesday, 23rd August*.—*On the Classification of the old Slate Rock of Devonshire*, with an explanation of the true position of the Culm Deposits of the central portion of that County, by Professor Sedgwick, and R. J. Murchison, Esq., V. P. R. S., &c.

Mr. Murchison began by observing, that he was about to submit a mere outline of a more detailed memoir on the physical structure of Devonshire, which, in conjunction with Mr. Sedgwick, he proposed to lay before the Geological Society of London. One object they had in view was, to remedy the defects in existing geological maps, as to colouring sub-divisions of formations; and another, to ascertain by actual sections, the true position of successive deposits, and their natural sub-divisions, so as to bring them into comparison with other corresponding deposits, and to determine their true place in the succession of British formations. By help of a section, the following succession of deposits in the ascending order was determined:—

1. A system of slaty rocks, containing a vast abundance of organic remains, generally in the form of casts; these rocks sometimes pass into a fine glossy clay slate, with a true transverse cleavage—sometimes into a hard quartzose flag-stone, not unusually of a reddish tinge, sometimes into reddish sand-stone, subordinate to which are bands of incoherent shale. In North Devon, they are very seldom so calcareous as to be burnt for lime, but in South Devon, rocks of the same age appear to be much more calcareous. This series is finely exposed in the Valley of Rocks, and the Valley of the Lyn, but its base is no where visible in this line of section.

2. A series of rocks, characterized by great masses of a hard thick-bedded red sand-stone and red flag-stone, subordinate to which are bands of red, purple, and variegated shales; the red colour occasionally disappears, and the formation puts on the ordinary appearance of a coarse silicious graywacke, subordinate to which are some bands of slate, but too imperfect to be used for roofing. This system contains very fine organic remains; it is several thousand feet in thickness, occupying the whole coast from the west end of the Valley of Rocks, in Combartin, being thrown back by a dip into the cliffs between Porlock Bay and Linton; it re-appears in North Hill and the Quantock Hills.

3. The calcareous slates of Combartin and Ilfracombe, of very great aggregate thickness, abounding in organic remains, and containing in a part of its range, at least, nine distinct ribs of limestone, burnt for use. This lime-stone is prolonged into Somersetshire, and is apparently the equivalent of the lime-stone on the flank of the Quantock Hills.

4. A formation of lead-coloured roofing slate of great thickness, and occupying a well-defined zone in North Devon, its upper bed alternating with, and gradually passing into a great deposit of green, gray and purple, or red sand-stone, and silicious flag-stone. These

silicious masses alternate with slates, and are in some places surmounted by great masses of red unctuous shale, which, when in a more solid form, generally exhibit a cleavage oblique to the stratification.

5. The Silurian System, resting conformably on the preceding, of great thickness, on the western coast of North Devon, occupying a zone several miles wide, and containing many subordinate beds and masses of lime-stone. In its range towards the eastern part of the county, it gradually thins off, but its characters are well preserved, and throughout it contains an incredible number of characteristic organic remains.

6. The carbonaceous system of Devonshire. This system is very greatly expanded, stretching in a direction east and west across the county, occupying the whole coast from the neighbourhood of Barnstaple to St. Gennis, in Cornwall, and on its southern boundary ranging so close to Dartmoor, that its lower beds have been tilted up and mineralized by the action of the granite. This great formation, is, therefore, deposited in a trough, the northern border of which rests partly in a conformable position upon the Silurian system, and partly upon older rocks, partly of the division, No. 4. Its southern border rests on the slate rocks of Cornwall and Launceston, and on the north flank of Dartmoor. From one side to the other, it exhibits an extraordinary succession of violent contortions, but its true place in the ascending section, admits of no doubt whatever. In some places, it is overlaid by patches of green sand, and in one part of the coast, west of Bideford, it is overlaid by the conglomerates of the new red sand-stone, which are seen for half a mile resting unconformably on its edge. The lowest portion of this vast deposit is generally thin-bedded, sometimes composed of sand-stone and slate, with impressions of plants—sometimes of indurated compact slate, both in an earthy and crystalline state. These beds are surmounted by alternations of shale and dark-coloured lime-stone, with a few fossils. Subordinate to these beds, there are on the west side of the county many thin veins and flakes of culm and anthracite.

On the eastern side of the county the coal is wanting, and the calcareous beds are much more expanded. On the south side of the great trough, the calcareous bands and dark shales are well exhibited, but near Oakhampton are, as above stated, mineralized by the action of the granite. The higher beds of this deposit are well exhibited on the coast west of Bideford, and consist of innumerable alternations of ferruginous sand-stone, flag-stone, and shale, containing in several places concretions of iron-stone, very often exhibiting impressions of plants; and one extended tract of country, containing at least three beds of culm or stone coal, associated with shales, contains many plants of species not known in the true coal measures. Though in a state of greater induration than the ordinary coal measures of England, and in most parts almost destitute of any trace of coal, yet even in these respects, it differs not from a great unproductive tract of the coal-field of Pembrokeshire.

*Wednesday, 24th August.*—Mr. Stutchbury read a paper on some newly discovered Saurian remains, from the magnesian congl-

merate of Durdham down. They were found in the magnesian conglomerate which rests upon the lime-stone, and must have been deposited upon the spot upon which they were found, without violent action; they are often injected with rocky paste. The structure of their vertebræ resembles strongly that of the crocodile.

*Mr. Hopkins* submitted his views on the phenomena of elevation. In reference to the mineral veins of Derbyshire, he had ascertained, that the direction of the axis of dislocation which had caused the fissure was true north and south, while that of the structure of the rocks was *magnetic* north and south; hence, shewing the connexion between magnetism and the theory of mineral veins.

*Thursday, 25th August.—The Ancient City of Memphis.*—The Marquis Spinetta read a paper, entitled “a report of the attempts made to ascertain the latitude of the ancient city of Memphis.” The details of this communication are of importance to geographers, as tending to elucidate a point on which Pocock, Shaw, Bruce, and other travellers, have differed. The question may now be considered to be set at rest, it having been clearly ascertained that it was in the present bed of the Nile, in latitude  $29^{\circ} 46'$  North, and longitude  $31^{\circ} 30'$  East from Greenwich.

The Chairman congratulated the Section on having heard these satisfactory details, and observed that the same process which had buried the ancient city of Memphis in the bed of the Nile—an accumulation of mud and drifted Lybian sands, in consequence of the demolition of the dykes, which once turned aside the water—had already sunk the beautiful fossil beds of Purton beneath the Severn.

Dr. Buckland stated that he had received engravings, prepared under the direction of M. Agassiz, of some of the splendid fossils in the Bristol Institution: and he also placed upon the table a copy of his own work on Geology, forming one of the Bridgewater Treatises.

The next paper was on the *Change in the Chemical Character of Minerals Induced by Galvanism.*—Mr. Fox mentioned the fact, long known to miners, of metalliferous veins intersecting different rocks containing ore in some of these rocks, and being nearly barren or entirely so in others. This circumstance suggested the idea of some definite cause; and his experiments on the electrical magnetic condition of metalliferous veins, and also on the electric conditions of various ores to each other, seem to have supplied an answer, inasmuch, as it was thus proved that electro-magnetism was in a state of great activity under the earth's surface, and that it was independent of mere local action between the plates of copper and the ore with which they were in contact, by the occasional substitution of plates of zinc for those of copper, producing no change in the direction of the voltaic currents. He also referred to other experiments, in which two different varieties of copper ore, with water taken from the same mine, as the only exciting fluid, produced considerable voltaic action. The various kinds of saline matter which he had detected in water taken from different mines, and also taken from parts of the same mine, seemed to indicate another probable source of electricity; for can it *now* be doubted, that rocks impregnated with or holding in their minute fissures different kinds of

mineral waters, must be in different electrical conditions or relations to each other? A general conclusion is, that in these fissures metalliferous deposits will be determined according to their relative electrical conditions; and that the direction of those deposits must have been influenced by the direction of the magnetic meridian. Thus we find the metallic deposits in most parts of the world having a general tendency to an E. and W. or N. E. and S. W. bearing. Mr. Fox added that it was a curious fact, that on submitting the muriate of tin in solution to voltaic action, to the negative pole of the battery, and another to the positive, a portion of the tin was determined like the copper, the former in a metallic state, and the latter in that of an oxide, shewing a remarkable analogy to the relative position of tin and copper ore with respect to each other, as they are found in the mineral veins.

The Chairman said it had been observed to them last evening, that the test of some of the highest truths which philosophy had brought to light was their simplicity. He held in his hand a blacking pot, which Mr. Fox had bought yesterday for a penny, a little water, clay, zinc, and copper, and by these humble means he had imitated one of the most secret and wonderful processes of nature, her mode of making metallic veins. It was with peculiar satisfaction he contemplated the valuable results of this meeting of the Association. There was also a gentleman now at his right hand, whose name he had never heard till yesterday, a man unconnected with any Society, but possessing the true spirit of a philosopher. This gentleman had actually made no less than 24 minerals and even crystalline quartz. He (Dr. B.) knew not how he had made them, but he pronounced them to be discoveries of the highest order; they were not made with a blacking pot and clay, like Mr. Fox's, but the apparatus was equally humble; a bucket of water and a brick-bat had sufficed to produce the wonderful effects which he would detail to them.

*Artificial Crystals and Minerals.*—Mr. Cross then came forward, and stated that he came to Bristol to be a listener only, and with no idea that he should be called on to address a section. He was no geologist, and but little of a mineralogist; he had, however, devoted much of his time to electricity, and he had latterly been occupied in improvements in the voltaic power, by which he had succeeded in keeping it in full force for twelve months by water alone, rejecting acids entirely. Mr. C. then proceeded to state that he had obtained water from a finely crystallized cave at Holway, and by the action of the voltaic battery had succeeded in producing from that water, in the course of ten days, numerous rhomboidal crystals, resembling those of the cave; in order to ascertain if light had any influence in the process, he tried it again in a dark cellar, and produced similar crystal in six days, with one-fourth of the voltaic power. He had repeated the experiments a hundred times, and always with the same results. He was fully convinced that it was possible to make even diamonds, and that at no distant period every kind of mineral would be formed by the ingenuity of man. By a variation of his experiments he had obtained gray and blue carbonate of copper, phosphate of soda, and 20 or 30 other specimens. If any

members of the Association would favour him with a visit at his house, they would be received with hospitality, though in a wild and savage region on the Quantock hills, and he should be proud to repeat his experiments in their presence.

Professor Sedgwick said he had discovered in Mr. Cross a friend who some years ago kindly conducted him over the Quantock hills on the way to Taunton. The residence of that gentleman was not, as he had described it, in a wild and savage region, but seated amidst the sublime and beautiful in nature. At that time he was engaged in carrying on the most gigantic experiments, attaching voltaic lines to the trees of the forest, and conducting through them streams of lighting as large as the mast of a 74-gun ship, and even turning them through his house with the dexterity of an able charioteer. Sincerely did he congratulate the Section on what they had heard and witnessed this morning. The operations of electrical phenomena, instances of which had been detailed to them, proved that the whole world, even darkness itself, was steeped in everlasting light, the first-born of heaven. However Mr. Cross may have hitherto concealed himself, from this time forth he must stand before the world as public property.

Professor Phillips said the wonderful discoveries of Mr. Cross and Mr. Fox would open a field of science in which ages might be employed in exploring and imitating the phenomena of nature.

*Coal Fields of South Wales.*—The Chairman then called on Mr. Conybeare to read his paper on this subject, but that gentleman said that the subject would now be so uninteresting after the splendid discoveries that had been detailed to them, that he should only point them to the map, and request them to imagine that he had read his paper, and that they had been asleep all the while.

Mr. Murchison read a paper "On the Geological Relations of certain Calcareous Rocks near Manchester;" after which the Section adjourned to the evening.

*Evening Sitting.—The River Severn.*—Mr. Murchison made a communication respecting the ancient Hydrography of the River Severn, and entered into statements respecting the drifts, in the course of which he expressed an opinion that the River Severn had been thrown into a southern direction by a convulsion of nature.

*Friday, 26th August.*—1. Lord Nugent described the sea rivulets at Argostoli, in Cephalonia, referred to by Mr. Babbage at Dublin. They flowed from the sea into the land, and one of them had been employed to drive a mill. Some persons accounted for this phenomenon, by supposing that there was a difference of level on different sides of the island, and that the streams flowing through a subterranean tunnel restored the equilibrium. Others thought that it was connected with volcanic action. Dr. Daubeny considered it a confirmation of the first volcanic theory of Davy.

2. Mr. Charlesworth expressed some doubts with regard to the correctness of the views of Mr. Lyell, as to the proportion of recent and extinct species in tertiary strata. M. Agassiz could detect no recent species of shells or fish in the Norfolk crag.

3. Professor Forbes stated, that he had traced a remarkable con-

nexion between the hot springs of the Pyrenees and the geology of the district. He had found that the granite of that country had acted upon the other rocks, in such a manner as to shew extreme cases of disturbance, and that round the lines that marked the connexion of these rocks with granite, these springs were uniformly found—mineral springs also abounded in the same situations. He stated, that he had examined the temperature of a spring, whose heat had been taken 100 years ago, and had found it exactly the same.

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SECTION D.—ZOOLOGY AND BOTANY.

*President*, Professor Henslow.—*Vice Presidents*, Rev. F. W. Hope, Dr. I. Richardson, Professor Royle.—*Secretaries*, John Curtis, Esq., Professor Don, Dr. Riley, S. Rootsey, Esq.—*Committee*, C. Babington, Esq., Mr. R. M. Ball, J. E. Bowman, Esq., Rev. Mr. Ellacombe, — Eyton, Esq., Hon. Charles Harris, Mr. Hewitson, Dr. Jacob, Mr. G. J. Jeffrys, Rev. Mr. Jenyns, I. L. Knapp, Esq., T. Mackay, Esq., Professor Nilsson, Rev. Mr. Phelps, Professor Scouler, Colonel Sykes, Richard Taylor, Esq., Professor Wilson, William Yarrell, Esq.

*Monday, 22nd August.*—1. *North American Zoology.*—Dr. Richardson read the introductory portion of his report on North American Zoology, comprising remarks on the physical geography and climate of the country. He noticed three distinct mountain chains, the principal one extending under the name of the Rocky Mountains, from the elevated plains of Mexico to the Arctic sea, rising within limits of perpetual snow, and affording on its declivities inclined zones of equal temperature, running nearly north and south. He next mentioned the comparatively low range of the Alleghanies running near the Atlantic coast, and the more broken Maritime Alps of California and New Caledonia, from which several lofty peaks arise. It was remarked that in these Maritime Alps in three ranges recent or active volcanoes exist, which it was suggested might be one of the causes of the higher temperature of that coast. The great valley lying eastward of the Rocky Mountains, and extending from the Gulph of Mexico to the Arctic sea, and watered by the Mississipi, Missouri, Saskatchewan, and the Mackenzie, was also noticed as exerting a manifest influence on the migrations of herbiferous quadrupeds, some tribes of birds, and fish. In the remarks on climate, the various ways in which the configuration of the land may contribute to influence the temperature of the atmosphere, were glanced at.

*The Arania or Mygale Avicularia.*—*Mr. Rootsey* exhibited a living specimen of this animal, which is of the spider tribe, and made some observations on the subject. It was not uncommon to meet with them in collections, and he had been informed by Mr. C. Babington, that one of them had been found in the London Docks. The animal in question was brought in a cargo of logwood, from the bay of Campeachy, but it was not known how it had subsisted, for, though pieces of meat had been near it, it had not eaten it; but it was supposed to have sucked the meat. The speaker

alluded to the statements made as to its poisonous qualities, it being more venomous than the serpent, and the extraordinary tales which were related of it; perhaps, however, they might be enlightened on this head by some gentleman better acquainted with the West Indies. With regard to its mode of procuring food, he stated, that it dropped from the branches of trees into the nests of birds, and preyed not only on the birds but on the eggs; in consequence of which it derived its name.

The *Rev. Mr. Hope* stated, that it was not the true avicularia described by Spix and Martius, but that it was named after Spix.

[The insect has been deposited at Mr. Miller's Nursery, Durdham Down, that it may have the advantage of stove heat.]

*Mangel Wurzel*.—*Mr. Rootsey* mentioned the result of various experiments he had made in extracting sugar, spirit, &c., from mangel wurzel, and converting the plant into malt, specimens of which were exhibited to the Section. The sugar was obtained in strong crystals, and the refuse of the plant was dried on a malt-kiln at a proper temperature, where it required the flavour and properties of common malt, and afforded an excellent beverage. The molasses were fermented into a spirit, the flavour of which was comparable to the peach brandy of America. Forty tons of the plant, which were sometimes raised upon an acre of land, afforded three tons of malt, and three and a half tons of molasses.

*Mr. Rootsey* then exhibited a specimen of the *Haltica nemorum*, or turnip fly, which he stated was the only insect which attacked the plant. Some discussion took place as to the best means of preventing the ravages of the insect, and it was thought, that by steeping the seed the insect would be destroyed; it was stated, that this was already done by farmers, but it was of no avail, as the eggs of the fly were not only deposited on the root, but in the hedges, and on the lands.\*

*On the Acceleration of the Growth of Wheat*.—*Mr. G. W. Hall* said, that the object of the present paper was to call attention to a statement of facts connected with the acceleration of the growth of wheat. The average length of time required for the growth of wheat was about ten months, but observations had led to the conviction, that much of this time might be saved, and the result has shewn, that five months has sufficed to produce an abundant crop of wheat (a sample of which was exhibited to the Section) by adapting the plant to the soil. The lighter silicious soils, when manured, possess a warm and stimulating character, and conduce to very rapid growth of plants, but they soon became exhausted; and it must be evident that an acceleration of the growth and ripening of the plants committed to a light soil, and a diminution of the time required for perfecting its crops, must not only be congenial to its character, but tend to economize and prolong its productive powers. These circumstances had been observed and acted on with the most beneficial results in various ways. The paper then touched at length on the means to be employed in accelerating the vegetable growth, and the evils attending it, and concluded by alluding to the benefit of the

\* The plan which has been adopted in Scotland is to sow the seed very thick.—  
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existence of the Society, met together to accelerate the progress of important truths. After some discussion on this paper, Dr. Daubeny stated to the Section the result of some experiments which he had made on the effects of arsenic on vegetables. A friend of his, Mr. Davies Gilbert, residing in Cornwall, had stated to him, that the soil in the neighbourhood of mines, being impregnated to the amount of fifty per cent. with arsenic, had a great effect on the vegetable kingdom. He had, therefore, tried some experiments at Oxford, and he found that the plants, which were barley and beans, did not suffer till nearly one-half the soil was composed of the sulphuret of arsenic. This proves that the statement of Mr. Gilbert was correct. Mr. Stephens stated, that the fish in some trout streams in the vicinity of mines were destroyed in consequence of the water drained from the mines having been turned into them. A coachmaker of this city had informed him that his horses had suffered very much in consequence of grazing in a field near spelter works.

*Tuesday, 23rd August.*—Dr. Richardson read a second portion of his paper on North American Zoology, embracing the Mammalia, his observations on the species having reference generally to the similarity of the North American Zoological division to that of Europe, and its comparatively small connexion with that of South America, notwithstanding their geographical approximation. Of the order Quadrumana, of which many with prehensile tails belong to South America, none range southwards of the Isthmus of Darien, while one has located itself in the southern extremity of Europe. Of the Cheiroptera, the North American species, are analogous to those of Europe, and very distinct from those of the southern division of either the old or new world. The North American Insectivora, on the contrary, differ greatly from those of Europe, the only genus common to both sides of the Atlantic ocean being *sorex*, or the shrews. Of the Marsupiata, three species are found within the limits of the United States, and they may be considered as ranging so far north, for their head quarters, in the southern zoological province of the new world. The existence of this order in America connects its zoology with that of New Holland, and distinguishes it from that of all other portions of the globe. To the family Carnivora, most of terrestrial quadrupeds, common to the new and old world belong, and a similar remark may be made of the birds of prey, for exclusive of the Phocæ, which, like the Amphibia and Cetacea, may be looked upon as belonging properly to the waters, the Falconidæ, of all birds, have the species most widely distributed. The American Seals are, without exception, also found on the shores of either Europe or Asia. The Gnawers, or Rodentia, serve to characterize the North American zoology from the great number of the species, exceeding those of any other quarter of the world, and the many peculiar forms they include. The small order Edintata, which forms a very characteristic and almost peculiar part of South American Zoology, is scarcely to be considered as belonging at all to North America, though three or four species cross the dividing line of the two provinces in Mexico. In former times the case was different, as the remains of very large and re-

markable animals of this order are much seen in many parts of North America, viz.—of Megatherium and the Megalonix. The order Pachydermata is remarkable at once for the size of the animals it includes, the number of extinct species, which more than double that of the living ones, and the small number that the new world now nourishes. Fossil Elephants and Mastodons have, however, been dug up in various distant parts of North America, and the bones of horses were procured by Captain Beachey under the cliffs of Kotezbue's Sound. The Ruminantia form an important and interesting part of North American Zoology, but only two, or at least three species are common to the new and old world.

*On the Longevity of Yew Trees.*—Mr. Bowman read a paper on the mode of ascertaining the age of yew trees, by counting the rings and lines of the trunk, and instanced several experiments which he had made. The mean average of the number of lines which a tree increased in a year was two, or  $\frac{1}{3}$ th of an inch, and the result of his experiment went to prove that Decandolle was wrong in his experiments in this respect; that he made the old trees too young and the young ones too old. With respect to the growth of yew trees in churchyards many reasons have been assigned for it, but it occurred to him that the longevity, the indigenous nature of the tree, and its being an emblem of immortality, led our forefathers to deck the place of the dead with them in lieu of cypress. This was one of the many customs which were engrafted on Christianity at its introduction, and it would be a barbarism to destroy an emblem that we might meet again hereafter. In conclusion, the paper urged on the scientific world the admonition of Decandolle, to pursue the subject by interrogating the annual rings of trees.

*Account of a new Species of Seal.*—Mr. Ball said that when young he was in the habit of observing seals, and he had only seen two species for a considerable time, but at length he found another seal, which he believed was the true phoca vitulina. Mr. Ball then exhibited skulls of several seals, and Dr. Riley also exhibited the skeleton of one which was caught near Weston-super-Mare.

Professor Nilsson described the animal exhibited by Mr. Ball, and gave it as his opinion that it was a specimen of a distinct species, forming a distinct genus from the vitulina, with which Linnaeus has confounded three. He terms it *Heliochærus griseus*.

Dr. Riley exhibited the stomach of the seal caught in the Severn, and stated that on preparing the skeleton of the animal he found from thirty to forty pebbles contained in it, which fact he mentioned, to point out the manner in which it is said seals catch fish. It is a prevalent opinion, that the seal when fishing attaches its legs to the bottom, and remains in a vertical position, and when a fish passes over its head it darts up and strikes it transversely. Now, according to the depth of water, it takes in a quantity of pebbles as ballast, as it is obliged to sink itself. Dr. Riley also exhibited the venous system of the seal, by which it was enabled to dive so admirably. It collected a quantity of blood on the right side, the same as they found was the case with those persons employed in diving for pearls.

*New Species of Scandent Norantia.*—Dr. Hancock exhibited a new species of this plant, which is a native of America, in form like

a withy, frequently of the thickness of a man's body, and growing round and to the top of trees, which it frequently destroys and pulls to the ground.

*Hermaphrodite Lucanus.*—The Rev. Mr. Hope exhibited an *Hermaphrodite Lucanus*, which called forth some discussion.

*On Certain Notions of Antiquity derived from the Ancients.* Mr. Hope then read an interesting paper on this subject. In the course of it, he said, that from the waters of the Nile spring into life myriads of insects, and with annual fertility the Egyptians were plagued with flies. It was curious that five out of the ten plagues of Egypt were from insects, viz., the plague of the waters of the Nile being turned into blood, which might have arisen from the insects contained in it, of lice, (from the soil,) of frogs and of flies, probably generated from the heaps of putrid frogs. Cleanliness not being much esteemed in Egypt, flies multiplied exceedingly, which led the people to erect and worship gods, who might be able to rid them of their tormentors. It was the general opinion in ancient times, that spontaneous generation was caused by fire, earth, and water; this opinion was prevalent so late as the 16th century, and was still held in Africa and Asia, and also by one class of naturalists in Europe. He should say that, reasoning from analogy, there was no such thing as spontaneous generation. Mr. Hope also referred to the transmigration of souls; the belief in this he thought originated from the changes in the animal kingdom, which he concluded by describing.

*Wednesday 24th August.—Fruits of the Deccan.*—Colonel Sykes said, that his duty, as statistical reporter in Deccan, had led him over the whole of that country, and he had, therefore, opportunities of observing, as, indeed, it was his duty to do, the products of the country, whether mineral or agricultural. The drawings which he would present to the notice of the meeting were executed with great care by a draughtsman in his service, who, having failed in that capacity in England, had enlisted in the Company's Artillery, and, on his landing in India, he fortunately was able to engage him. He would also remark, that these drawings were made from actual measurement of the plant, and as they were accompanied by a scale, it was only necessary to use a pair of compasses, and the real dimensions of any part of a flower or plant might be ascertained. The account of the subject which he intended giving, was derived from the sacred books of the Hindoos, five of which were in his possession, and the contents were extremely curious, the language used being Sanscrit; they were very ancient, and, in short, were a complete materia medica, and complete encyclopædia of agriculture, &c. The Colonel then read an elaborate and complete list of the wild and cultivated plants of the Deccan, stating their form, size, use, medicinal, and other properties, &c., and illustrating them with the drawings which are before mentioned.

He noticed the *Golden Plantain*, which is a tree of great luxuriance, and was remarkable for its bearing fruit but once, and becoming quite useless and exhausted, immediately after doing so. It had given rise to some adages among the natives, such as the

lioness litters but once, the plantain bears fruit, and a woman marries but once, (this being the custom of the country), and a brave man never retreats but once, meaning, that he loses his life in doing so.

*On the Geographical Distribution of Plants in Ireland and the West of Scotland.*—Mr. J. T. Mackay read a report which he had drawn up in accordance with a suggestion thrown out by a member of the British Association at its last meeting.

*Caoutchouc.*—Mr. Royle in visiting the manufactory of the elastic web from caoutchouc or India-rubber, which is now applied to a variety of purposes, was informed there was a difficulty in obtaining, from South America, a sufficient quantity of caoutchouc, or India-rubber for the purposes of the manufacture, and was, therefore, led to point out the variety of plants and countries from which the same substances might be obtained. A communication was first read from Mr. Sevier, the sculptor, who has made the principal discoveries in the properties of caoutchouc, and the commerce of caoutchouc, by which it appeared, that, since the removal of the duty, the importation of it had increased from 10 to 500 tons annually, and is soon expected to be 2000 or 3000 tons a-year, from its various uses as articles of dress, and ligatures of every kind, as well as for elastic ropes for the breaching of guns, and bands for driving machinery. The earliest accounts, by Condamine, Aublet, and Priestley, were alluded to, and the South American tree, yielding caoutchouc, was mentioned under the name *Siphonia elastica*, that of Penang as *Urceola elastica*, and the Indian as *Ficus elastica*, while other plants yield it in Madagascar, Mauritius, Singapore, and China. The natural families of plants, to which all those yielding caoutchouc belong, were stated to be *Cichoraceæ*, *Lobeliaceæ*, *Apocynæ*, *Asclepiadeæ*, *Euphorbiaceæ*, and *Artocarpeæ*, all of which have milky juice, and are in considerable quantities in tropical countries; there could be but little doubt, that many other plants of these families might be found to contain this useful substance, as well as those which are already known to do so. Besides these general results, it was observed, that many of the plants of this family were remarkable for the tenacity of their fibre, which fitted them for the purposes of *rope-making*, and that it was singular, that in the attempts to find substitutes for the mulberry-leaf in feeding the silk-worm, so many of the plants, which they prefer, next to the mulberry-leaf, should belong to the families which yield caoutchouc, as the lettuce-leaf, the leaf of *Ficus religiosa*, the *Artocarpeæ*, and the *Castor Oil Plant*. Considering that these facts were not likely to be accidental, the author was led to infer, that something of the same kind must be contained in the juice of the mulberry, especially, as it belonged to the family of *Artocarpeæ*, and, having requested Mr. Sevier to make the experiment, the author was informed, that he was perfectly correct in his indication, as the mulberry juice also contained caoutchouc, whence it was inferred, that the silk-worm requires some portion of this tenacious substance in its food to enable it to spin its silk, and the fact was communicated, as probably of some practical value, as well as of scientific interest.

Mr. Hope subsequently remarked, that the dandelion, which had been previously noticed as yielding caoutchouc, was one of those

employed as a substitute for feeding the silk-worm—a striking instance of the utility of men of different pursuits meeting and discussing subjects of this nature together.

*On the Luminosity of the Sea.*—Mr. Duncan said, that he brought this subject forward more for the purpose of gleanng information than giving it, with regard to this beautiful phenomenon. The communication arose from a conversation which he lately had with a physician residing at St. Leonard's, and he described the appearance which the sea presented, when illuminated by certain animalculæ, with observations. On the 28th October, the sea presented a most splendid spectacle, every wave appearing as a rolling mass of phosphorus, when the gentleman in question obtained some of the water, and 18 hours after, on being put in the least motion, it emitted phosphorescent sparks, which, however, it did not while remaining in a state of quiescence. When the water was still, innumerable disc-shaped animalculæ collected on the water, like minute drops of oil, on an average to the number of 60 or 80 on each square inch of water. They become more visible in placing a piece of black silk beneath them, and then they appeared almost white, or some transparent, except at one point which was opaque, and always situated at the margin of the disc, and they differed much in size. The water was kept for six days, at the end of which, the light was still perceptible, in the dark, on agitating the water, but gradually became less brilliant. It has been supposed, that the animalculæ float constantly on the surface of the water, and their presence in particular situations depends on certain winds, and circumstances over which they themselves have no control. Reasoning from analogy, he supposed that their appearance did not depend altogether on fortuitous causes; but, that, as a certain condition of the atmosphere seduces the glow-worm to venture from her obscure retreat, that so these animalculæ have been taught to rise only during certain states of their element from the depths beneath. This was the substance of the communication from his friend; but, what was singular, was that they could not observe any light from animalculæ till the water had been shaken; it, therefore, favoured the supposition, that it was necessary for them to imbibe oxygen to enable them to emit the same.

Colonel Sykes said, that in tropical countries he had himself seen the animalculæ, which were very minute; gelatinous masses, he stated, emit light without being agitated.

*Remarks on the Cow Fish, or River Cow (manatus fluviatilis.)*  
By Dr. John Hancock.

Mr. Rootsey read a paper describing this animal, a specimen of which was exhibited. The animal was now only found in the lakes far away from the European settlements, and the name chosen was very inappropriate. Some authors asserted that the animal frequently weighed 8000 lbs., and measured 28 feet in length, but he (Dr. Hancock) having seen many, and examined them, thought they very seldom exceeded 600 lbs. in weight, and six feet in length. The flesh of the animal is very good, very much resembling veal, very easy of digestion, and the soup made from it was delicious, and equal to turtle, though not so gelatinous; the flesh would also keep

wholesome without salt for many months. The bones were highly esteemed by the natives, and when taken in a powder were highly beneficial in complaints of the kidneys. It was also believed to bellow like a bull, and to fight desperately on some occasions. It moved through the water with great rapidity, not, however, by moving the tail laterally, as other fish, but vertically, up and down. It had been asserted, that this animal could not live on shore; but this he doubted, as it was unable to breathe like a fish, the respirative organs being nearly the same as those of terrestrial animals, and it was, therefore, obliged to come to the surface to respire, and always slept with its nose above water, under shielding banks. Indeed, nature seemed to have placed it in an element which it was not fitted to; it was unable both to breathe and procure food under water, and it was thought that had it legs to walk on shore it would abide there. It was also suggested, that it would be desirable to find pasturage for these animals connected with small pools of water, and thus droves of the sea cow might be found; a case was instanced of a sea cow being kept in a small lake, in one of the West Indian Islands for 26 years, which became so tame as to be pleased with the human voice, to come when called, and to swim across the lake with children on its back without plunging beneath the surface of the water. The upper part of the body approximated to the human form, and the posterior to the fish, and when it rose out of the water to gather food from the banks, it had much the appearance of what is called the mermaid; and, from it probably the fable of mermaids and the tritons originated; particularly as the Indians usually had painted on the sterns of the canoes a figure similar to that which the cow fish presented, when in the position described, which they styled, "the man of the waters."

*On the Mode of preserving Animal and Vegetable Substances.* Dr. Macartney read an interesting paper on this subject; in the course of which he stated, that by washing insects, skins of animals, or flowers in essential oil of cloves, or, indeed, in any essential oil, they might be preserved for a great length of time without injury.

The Rev. Mr. Hope read a communication from Mr. Raddon, *On the Means of obtaining Insects from Turpentine*, and exhibited two cases, containing a vast number of very fine specimens. Mr. Hope observed, that in future it would not be necessary to proceed to America to procure insects, as it was only necessary to go to the warehouses of those merchants who imported turpentine, and by searching through it when boiling, they might very shortly obtain sufficient specimens to form fine collections at a few shillings' expense.

*Thursday, August 25th.*—Before proceeding to business, the President announced, that the Sectional Committee had made arrangements for a holiday on Friday, in order that the Section might proceed on either of the excursions fixed for that day.

*Zoology of North America.*—Dr. Richardson read the concluding part of his report on North American Zoology, which treated principally of birds and fishes. He also incidentally referred to a small fish which drummed at the bottom of vessels on the North American Coasts, and so loudly as to shake the vessel, and to render utterly impossible for persons unaccustomed to it, to sleep. The

thanks of the Section were voted to Dr. Richardson for his valuable report.

*Criteria of Species.*—Mr. Carpenter read an elaborate communication on this subject, founded on the views of Dr. Prichard. This called forth a discussion, in which Mr. Vigers, Mr. Carpenter, the President, and Dr. Prichard took part.

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SECTION E.—MEDICAL SCIENCE.

*President*, Dr. Roget.—*Vice Presidents*, Dr. Bright, Dr. Macartney.—*Secretaries*, Dr. Symonds, G. D. Fripp, Esq.—*Committee*, Dr. O'Beirne, Dr. Bernard, Dr. James Bernard, S. D. Broughton, Esq., R. Carmichael, Esq., Dr. Carson, Bracey Clarke, Esq., E. Cock, Esq., J. W. Cusack, Esq., H. Daniel, Esq., J. B. Estlin, Esq., Dr. Evanson, W. Hetling, Esq., Dr. Hodgkin, Dr. Houston, Dr. Howell, Dr. James Johnson, R. Keate, Esq., O. King, Esq., Dr. Prichard, O. Rees, Esq., Dr. Riley, Richard Smith, Esq., J. C. Swayne, Esq., N. Vye, Esq., Dr. Yellowley.

*Monday, 22nd August.*—"A Report of the Dublin Committee on the Pathology of the Nervous System, by Dr. O'Beirne," was first read.

"The Committee feel compelled, on the present occasion, to confine themselves to an analysis of the cases of nervous affections which have come under their observation, during the short period which has elapsed since they have considered themselves to be regularly appointed. They are of opinion that, in order to arrive at accurate pathological conclusions on a subject so extensive and complicated, and upon which the most eminent authorities are found to disagree, a very great number of cases should be first submitted to their examination; then the symptoms of each case carefully registered, and subsequently, accurate post mortem examinations made in the presence of the committee, to ascertain the structural lesion or lesions with which the symptoms co-existed. As far as their investigations have as yet extended, they see that the subject, if considered in all its details, will require a considerable length of time before they can accumulate such a number of cases and matured observations, as would justify them in drawing general conclusions. Further, they have to state, that they have collected some valuable facts, relating to injuries and diseases of the nerves, which seem to throw light upon disputed points of the physiology and pathology of this portion of the nervous system. They are of opinion, however, that more extended observations on this branch of the subject are required to be made. They would also submit the necessity of repeating those experiments on animals, upon which so many authorities rely as a foundation for their doctrines.

"The Committee, influenced by the above considerations, have decided on avoiding, for the present, any attempt at drawing general conclusions. They consider it more judicious to collect and arrange for a future report, should they be re-appointed, the abundant materials which their opportunities enable them to supply.

"In furtherance of this object, they have been for some time

engaged in registering the history and symptoms of cases of nervous affections in the wards of the House of Industry, Dublin, and the different hospitals connected with it. This institution contains, independently of cases of paralysis (estimated at about 150), the following cases of mental and nervous affections, arranged as follows:—

	Males.	Females.
Chronic Insane . . .	74	179
Epileptic Insane . . .	21	33
Congenital Idiots . . .	69	62
Epileptic Idiots . . .	14	20
	178	294—Total, 472.

“ The number of cases which the Committee have hitherto been enabled to examine with sufficient accuracy amounts to 41. Of these they have made an analysis which is attached to their report. They also affix an index referring to 17 cases of affections of individual nerves, but regret that they have not had sufficient time to make it either as full or accurate as they could wish.

“ Dublin, August 17th, 1836.”

The second paper read was by Dr. O’Beirne, of Dublin, which was an “ Abstract of an unpublished work on Tetanus.” The reading of this abstract, which was very lengthened, excited much interest among the profession. The points of discussion which ensued were, first, as to what constituted the substantive disease of tetanus, and what was merely pseudo-tetanus, Dr. O’Beirne contending that treatment of cases of the latter kind, as detailed by Dr. Wallis, Dr. Hetling, Dr. King, and others, was not to be received as evidence of the presence of the real disease of tetanus. During the peninsular war, Dr. O’Beirne stated, that he had had frequent opportunities of observing the disease, and at Brussels, he had the sole charge of the officers wounded at Waterloo, among whom several cases of tetanus occurred, which invariably terminated fatally. The result of his experience was, that tobacco, injected by a tube into the bowels, was an anti-tetanic, removing constipation, and affording relief; and he was really at a loss, such had been the effect of the insertion of the tube in affording relief, to know whether to attribute more to the tobacco or the tube. There had been no disease which had been so confounded with tetanus as old cases of rheumatism, and paralysis was the very antipode of tetanus.

The third paper read was entitled “ Aneurism of the Arteria innominata, by Sir David I. H. Dickson.”

*Tuesday, 23rd August.—On the Treatment of some Diseases of the Brain, by Dr. Prichard.*—It has been said in former years that the art or practice of medicine has made much more rapid advances than the theory or science. It will hardly be disputed that the reverse of this observation holds good in the present time. For many years past, and especially since more precise investigation than was before pursued, has been practised by means of necroscopy, into the exact nature of organic changes, much accurate knowledge has been acquired, which is perhaps scarcely applicable to practical purposes. There is no class of morbid affections to which this

remark is more truly applicable, than it is to diseases of the brain, and its investing membranes. Nobody is less disposed than myself to estimate, at a low rate, the value of information obtained through the medium of researches, similar to those of Sir Charles Bell, Drs. Abercrombie, Bright, Hodgkin, Sims, and others in this country; and of M. M. Rostan, Foville, and many others on the continent. Knowledge of the precise nature of morbid changes has its value, even in a practical point of view, if not by directing us always to remedies, at least, by making us aware what we are to expect in particular cases, as the final results of disease, and as pointing out the limits of what is possible, or what ought to be attempted with reference to cure. Still we ought not to lose sight of the fact, that the recovery of patients, and not merely accurate pathology and diagnosis, is the ultimate object.

Perhaps all curative attempts in cases of disease affecting the brain, resolve themselves into the modifications which medical art is capable of effecting in the vascular state, of parts within the skull. We can promote by various means either fullness or inanition of the blood vessels in the brain. Whether anything beyond this is in our power is very uncertain. Besides general and local bleeding, all those means belong to the same class, which act by refrigerating or heating the surfaces either of the head or of other parts. Refrigerant applications to the head have the effect of contracting the calibre of the arteries, and thereby diminishing the quantity of their contents. Pediluvia, or other means of applying warmth to the lower extremities, produce a similar result by augmenting the capacity of vessels remote from the head, and causing a greater quantity of blood to be determined into them. All these means plainly owe their efficacy, to the modification which they bring about in the state of the vascular system of the brain. The only class of remedies respecting the "modus operandi" of which, any question can be raised, is those which produce what is termed counter-irritation, and perhaps the doubt which exists in this instance, arises from the obscurity of the subject. It is generally supposed, and perhaps correctly, at least, it is very difficult to find any other hypothesis on the subject that is more probable, that the means of counter-irritation, such as rubefacients, vesicatories, and issues, produce their effect by lessening a hyperplethoric state of the vessels in internal parts, and that they bring this to pass by increasing the fullness of the vessels in surfaces to which they are immediately applied. There are facts which it is very difficult to reduce under this sort of explanation, as for example, the relief obtained in cases of pneumonia or of bronchitis, by means of blisters applied to the parietes of the chest, there being in these instances no continuity of structure that might render the proposed explanation in some degree intelligible. On the other hand, there is little doubt that such remedies are most efficacious when they are applied over surfaces nearly in juxta-position with the seat of disease, and this fact, if not called in question, goes far towards establishing the notion before alluded to, as to their mode of operation.

As the means which are within our reach for treating disorders of the encephalon are so circumscribed, it appears so much the more necessary, to endeavour to apply in the most efficacious manner such

resources as we possess. I am not disposed to believe that any material improvement can be made in the ordinary rules for the use of evacuants or measures of depletion, but I have no doubt, that an important advantage may be gained, by directing in a particular manner, the mode of counter-irritation, and it is chiefly with the view of recommending this attempt that I have premised the foregoing remarks. Long experience has convinced me that the most efficacious way of applying counter-irritation in diseases of the brain is a method not often practised in other places, which has been for many years in almost constant use at the Bristol Infirmary. An objection would probably arise in the minds of those who have not witnessed the application of this remedy, on account of its apparent severity. I hope to convince the medical section, and through this opportunity, to make more general than would otherwise be done, the persuasion that the method of treatment to which I refer is by no means so painful or severe a remedy as it might be supposed to be, and that it greatly exceeds in efficacy any other means by which physicians have attempted to relieve diseases of the brain on a similar principle. The application I recommend is an issue produced either by means of a soft caustic, or what is much better, by an incision over the scalp. The incision is most frequently made in the direction of the sagittal suture, from the summit of the forehead to the occiput. The scalp is divided down to the pericranium. The incision, when that method is used, or the aperture left by the slough, when caustic is employed, is kept open by the insertion of one or two, or in some instances three rows of peas. The discharge thus occasioned is considerable, and it obviously takes place from vessels which communicate very freely with the vessels of the encephalon. It would appear, *a priori*, very probable, than an issue in this particular region, just over the sagittal suture, would have a greater effect on the state of the brain, than in any other situation, and the result of very numerous trials has abundantly established the fact. I can venture to assert, that in all those cases of a cerebral disease in which counter-irritation is at all an available remedy, an issue of the kind now described is, next to bleeding, by far the most important of all the means which have yet been, or are likely to be discovered. The kinds of cerebral disease in which counter-irritation is beneficial, include, according to my experience, all those complaints which are accompanied by stupor or diminished sensibility, excluding all affections, attended by over-excitement, such as maniacal and hysterical diseases. In the latter, I believe all such measures to be for the most part highly injurious.

A case has lately occurred in my practice at the Bristol Infirmary, which strongly exemplifies the efficacy of the treatment which I have recommended, and which I have fortunately an opportunity of bringing before the Medical Section in the most convincing way. A youth aged about eighteen, came into the Infirmary labouring under complete amaurosis, which had been coming on gradually for a week or ten days before his admission. At that time it had become so complete that vision was entirely lost, and the pupils were totally insensible to light even when the rays of the sun were suffered to fall immediately into the open eyes. At first he was freely and repeatedly

bled from the arm and temporal artery, had leeches applied to the scalp, blisters to the nape of the neck, and took calomel so as to render his gums sore. Finding that no effect whatever was produced by these measures, I gave up the expectation which I had at first entertained of his recovering sight, but was resolved to give the remedies a complete trial. I ordered him to be bled, *ad deliquium*. This took place after a small quantity of blood had flowed from his arm while he was in an erect posture. After a few days, he was still perfectly dark. An incision was now made over the sagittal suture from the forehead to the occiput. It was filled with peas. In three or four days, precisely at the time when suppuration began to take place, the patient declared that he perceived light, but was scarcely believed, since the pupils were still widely dilated and quite insensible to a strong light. In the course of a few days it was quite evident that he saw, as he could tell when two or three fingers were held up. For some weeks, the iris was still quite irritable, though vision had become in a great degree restored.

The subsequent treatment of the case consisted chiefly in occasional leechings, purging, and low diet; when the issue healed which was not till it had been kept open some months, a seaton in the neck was substituted; under this treatment the case has terminated in a complete recovery of the blessing of sight. I shall not detain the medical section longer upon this topic, but have procured the presence of the patient, and any gentleman who wishes to examine him, either as to the accuracy of what I have related, or to the degree of suffering occasioned by the remedy, or to observe the slight vestiges which it has left, will have an opportunity.

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## II.—*Death of Dr. Henry of Manchester.*

It is with painful feelings that we announce the death of this excellent man—more particularly from the melancholy circumstances under which it took place, and from the daily communication which those interested in chemical pursuits held with him, during the meeting of the Bristol Association, where he took an active part.

Dr. Henry finished his education in the university of Edinburgh. To this College he manifested through life a great attachment. During his studies in that celebrated establishment, he was exceedingly fortunate. He attended the lectures of the illustrious Dr. Black, one of the fathers of chemistry; and he was the associate and friend of Brougham, of Jeffrey, of Mackintosh, and a number of others, who have since attained, like himself, a high degree of celebrity. Lord Brougham in his address to the Manchester Mechanics' Institution, in 1835, referred to Dr. Henry in the most respectful terms as a fellow student. "I met," says he, "an old and worthy friend of mine, a man of great ability and learning, your townsman, Dr. Henry. We were fellow Collegians, and learned Chemistry together—though, God wot, he learned a great deal more than I did."

Dr. Henry was intended for the medical profession; but very delicate health, and the necessity of his co-operation in his father's lucrative pursuits, which he subsequently so greatly extended, induced him, after some practice, to relinquish that arduous and har-

raising occupation. A taste for chemical research had also, no doubt, its influence upon his determination.

In private life, Dr. Henry had qualities calculated to excite and to rivet esteem and admiration. His conversation was peculiarly attractive and insinuating. Pregnant with varied and extensive information, he knew how to impart it in the most alluring manner. His anecdotes, of which he had a copious selection, were always aptly introduced, and felicitously narrated. Intended to enliven or to illustrate at the time, they generally left upon the memory impressions worthy of subsequent reflection. He was a master of the science of conversation. He was never overbearing or dogmatical; and no one, how humble soever his talents, was in private intercourse made to feel an inferiority, except by a silent comparison, which was, in many cases, almost unavoidable. He never appeared to speak for the purpose of display. He always seemed to talk for others, not for himself. He was always anxious to inspire the most diffident with confidence. He had no repulsive airs, but many admirable graces; and no one, it is believed, ever enjoyed his conversation without feeling that, high as was his reputation, it afforded a very inadequate estimate of his merits. It might justly be said of him, in the words of an eloquent statesman, that "he was the life and ornament of polished society."

In all the relations of private life he was most exemplary. As far as the writer can judge, no man was more highly regarded and more warmly beloved by his relatives. The combination of kindness with mental superiority was his most marked characteristic; and it attached to him every one who came within the sphere of its influence.

Occupying a splendid establishment, he displayed commensurate hospitality. He was particularly distinguished for the liberal and active patronage which he readily afforded to those aspirants in science who attracted his attention. In such cases, he required no solicitation. The encouragement was on his part spontaneous. It was the emanation of his nature. When he formed a favourable opinion, he was very unlike an ordinary patron. His kindness never ebbed and flowed. It was always equable. Any one who tried to deserve it might calculate upon it, at any time, with absolute certainty. He not only possessed high talents himself, but he was almost a creator of talent in others. The younger members of the Literary and Philosophical Society of Manchester will deeply lament the loss of him who peculiarly encouraged and stimulated their earliest efforts. That association will, in all its ramifications, mourn the absence of him who has been "as water which was spilt upon the ground, which cannot be gathered up again."

Dr. Henry has conspicuously shewn that a due and regular attention to business, is not incompatible with very high success in science. Soon after the termination of his collegiate education, he delivered in Manchester, several courses of lectures on Chemistry. These lectures were illustrated by a very expensive apparatus, and contained experiments of a very highly interesting character. The notes of these courses ultimately led to the publication of a small volume on the science, which has, in successive editions, gradually become a detailed and excellent treatise on the subject. This work has long

been remarkable for the precision of its information, and for the characteristic elegance of its style.

Besides this publication, he has contributed to the Transactions of the Royal Society of London, to the Memoirs of the Literary and Philosophical Society of Manchester and to several periodicals, a number of papers of a very interesting and important character. When coal gas was applied to the purpose of illumination, he was one of the first to determine its constitution; to point out the best mode of analysis; and to suggest the most effective methods of obviating the inconveniences, to which, in its early applications, it was liable. His papers on this subject present a fine specimen of inductive research. His investigations on the combinations of the gases by volume; the absorption of different gases by water; the application of Doberienner's spongy platina to gaseous analysis, and a great number of other interesting subjects, have exhibited great philosophical acumen, and unsurpassed precision in manipulation. Never was there a more careful, a more impartial, a more accurate experimenter. It may be mentioned, as an instructive illustration, that on one occasion, when a young friend was assisting him in his operations, the former proceeded, before the termination of an experiment, to calculate the result. "Stop," said the Doctor emphatically, "don't try what the result should be, or there will be danger of *coaxing the experiment* so as to make it to correspond with the estimate."

As a literary character, Dr. Henry deserves a much higher reputation, than he has in this respect, yet obtained. His character of Priestley, of Davy, and of Wollaston, are some of the finest specimens of that species of composition in the English language. The discrimination which they manifest, and the elegance and accuracy of the style, will render them models of the highest value to those who are required to exercise their powers upon such topics.

To the death of Dr. Henry it is necessary to refer. In ancient times, to shorten the natural period of life was, in certain cases, regarded with applause; Cato, Brutus, Seneca, and others were lauded by their countrymen, for an act, which has received from modern times unqualified censure. Yet, even in modern times, illustrious instances have occurred. Romilly, Whitbread, and others fixed the limit of their own earthly existence. Such an act cool reflection cannot justify; but we should not be disposed to admit the right of erring human beings to dictate to Providence in presumptuously assigning a penalty for the offence. In the case of Dr. Henry there was every circumstance which might preclude or could mitigate condemnation. Months had elapsed during which he had not slept, his ever active mind was perfectly exhausted; and he was himself conscious that, as others too clearly observed, his mind was acquiring, by perpetual excitement and want of repose, a tendency to "wander from its dwelling." It is, perhaps, in the very constitution of superior intellects, too continuously exerted, that they should be peculiarly liable to be shaken from their equilibrium. Even Newton's transcendent mind was repeatedly subjected to this condition of humanity. The pious and amiable Cowper was also a martyr to mental alienation. It has been so with very dissimilar dispositions and characters. The last days of Tasso, of Collins, and of Swift,

were obscured by the same mysterious visitation. The human intellect may be, to a certain extent, compared to the dew-drop in the sun-beam,—the brighter it shines, the more rapidly it fades away!

Dr. Henry was 61 years of age. He died on the 2nd, and was interred on Wednesday morning the 7th of September, 1836, in the burial-ground of the Chapel in Cross Street, Manchester, and his coffin was deposited upon that of his distinguished father.

This very hurried and equally imperfect tribute has been drawn up by one who has reason to cherish Dr. Henry's memory with mingled feelings of gratitude and admiration. At a period when the pressure of his loss is so heavily felt by those who could appreciate his talents and estimate his worth, it is impossible to do justice to his character. When the agitation of grief shall have subsided, his career and his virtues will, we trust, be detailed by an abler pen, under more favourable circumstances.

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### III.—*Artificial Production of Metallic Sulphurets, &c., by Electrical Action.*

THE Geological Section of the "British Association for the Advancement of Science," having received as novelties some communications on this subject, we think it due to M. Becquerel to state, that he obtained by this means a very considerable number of substances, above seven years since. His apparatus consisted of a tube bent into a syphon shape U, the curved portion being filled with moistened clay, (l'argile pumectée,) and the legs with solutions of the substances of which combinations were sought, and connected by a wire.

The *crystalline metallic* bodies which he obtained were,

Metallic copper.

Red oxide of copper.

Vitreous copper.

Grey copper (fahlertz).

Metallic silver.

Vitreous silver.

Chloride of silver.

Sulphuret of lead.

Carbonate of lead.

Sulphate of lead.

Oxy-sulphuret of antimony (kermes).

Besides a considerable number of alkaline sulphurets, chlorides, bromides, and many double sulphurets, salts, &c.

Full details will be found in M. Becquerel's work "de l'Electricite et du Magnetisme, Tome i., 332—350."

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### IV.—*Royal Geological Society of Cornwall.*

Twenty-third Annual Report of the Council.

DURING the past year considerable additions have been made to the Museum and Library, and the funds of the Society continue in a prosperous state: but the council have the painful duty to report that the quarterly meetings have been discontinued in consequence

of the uniform non-attendance of the members. This is the more to be regretted because these meetings, if properly supported, might have been the means of exciting a more general taste for geological pursuits; and it is to be hoped that the attempt which will be made, during the ensuing year, to revive them will be more successful, as your council feel assured that such meetings will greatly tend to promote the welfare of the Society.

The publication of the transactions in annual parts has been again brought before the council, and has been strenuously advocated as a measure which would insure the more frequent communication of valuable memoirs. The papers already laid before the Society will appear in the fifth volume, which it is expected will be finished against the next anniversary: and the council recommend the immediate publication of such as may be hereafter presented, in the hope that such a regulation may elicit a more abundant supply of scientific communications than has been received on the present occasion.

The council, however, whilst regretting the inactivity of the Society during the past year, have great satisfaction in being able to state that considerable progress has been made towards the attainment of a more accurate knowledge of the geological structure of Cornwall, by the able and indefatigable labours of Mr. De la Beche, who has kindly acceded to their request of giving the members some information concerning the result of his investigations.

By order,  
HENRY S. BOASE, Secretary.

September 2nd, 1836.

The following papers have been read since the last report:—

I. A chemical examination of a peculiar substance incrusting the roof of a cavern in Cornwall. By Henry S. Boase, M.D., Secretary of the Society.

II. On Slikensides, and whether they afford evidences of mechanical origin. By W. J. Henwood, F.G.S., Lon. and Paris, Hon. M.Y.P.S., Assay Master of Tin in the Duchy of Cornwall, Curator of the Museum.

III. On a granite vein and the phenomena which accompany it at Polmear Cove. By Henry S. Boase, M.D.

IV. On periodical variations in the quantities of water afforded by springs. By W. J. Henwood, F.G.S. Cor. Mem. Plymouth Institution.

V. An account of the Quantity of Tin produced in Cornwall and Devon, in the year ending with the Midsummer Quarter, 1836. By Joseph Carne, Esq., F.R.S., F.G.S., M.R.I.A., &c., Treasurer of the Society.

VI. An account of the Quantity of Copper produced in Great Britain and Ireland, in the year ending the 30th June, 1836. By Alfred Jenkin, Esq.

#### OFFICERS AND COUNCIL FOR THE PRESENT YEAR.

*President*, Davies Gilbert, Esq., D.C.L., F.R.S., &c., &c.—  
*Vice Presidents*, Robert Were Fox, Sir W. Molesworth, Bart., M.P., F.R.S., Rev. Canon Rogers, John Scobell.—*Secretary*, Henry S. Boase, M.D.—*Treasurer*, Joseph Carne, F.R.S.—*Curator*, W. J. Henwood, F.G.S.—*Librarian*, Richard Hocking.

# Meteorological Journal, — For September.

Kept at the Manse of the Parish of Abbey St. Bathans, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea.  
By the Rev. JOHN WALLACE.

DATE.	THERMOMETER.						HYGROMETER (Leslie's).						BAROMETER. At 32° of Fahrenheit.						Rain in Inches. Weekly.	Direction of Wind at	REMARKS.
	I. A.M.	II. A.M.	III. P.M.	IV. P.M.	V. P.M.	P.M.	I. A.M.	II. A.M.	III. P.M.	IV. P.M.	V. P.M.	P.M.	I. A.M.	II. A.M.	III. P.M.	IV. P.M.	V. P.M.	P.M.			
1835.																					
Th.	53	51	57	54	50	46	0	0	32	7	29-031	29-034	29-033	29-093		W.	Brisk wind, A.M. rain, cumuli on blue sky with occasional showers, evg. calm				
Fr.	48	51	58	44	10	16	29	0	16	29	29-049	29-026	29-908	29-064		W. by S.	Gentle wind, A.M. cloudy, P.M. cumuli on blue sky, evening calm and clear				
Sa.	51	51	53	48	50	44	0	18	20	0	29-180	29-184	29-118	28-974		W. by S.	Gentle wind, A.M. nearly cloudless, P.M. sky overcast, evg. rain, wind rising.				
Su.	51	51	53	52	9	9	0	9	9	0	28-859	28-854	28-805	28-702	1-063	E. by E.	Gentle wind, A.M. overcast, P.M. continued rain and fog.				
Mo.	48	50	52	44	9	18	19	0	18	19	28-683	28-699	28-789	28-835		W. S. W.	Brisk wind, A.M. cloudy, slight showers, P.M. cumuli on blue sky, evg. clear.				
Tu.	49	50	51	48	2	5	15	0	28-737	28-738	28-746	28-863		S. E. by E.	Calm, A.M. cloudy, P.M. partially clear, evening hazy clouds rising in the N.						
We.	51	53	51	49	9	11	3	5	29-061	29-085	29-156	29-227		S. W.	Brisk wind, frequent and heavy showers, evening calm and cloudy.						
Th.	51	52	53	43	10	14	14	0	29-131	29-123	29-084	29-070		S. W.	Brisk wind, A.M. cumuli abundant, P.M. gradually overcast, evg. calm, rain.						
Fr.	49	48	49	53	41	19	16	29	0	29-167	29-173	29-163	29-194		NW by W	Calm, A.M. fleecy cumuli and cirri abundant, 4 P.M. heavy shower, evg. clear.					
Sa.	46	48	47	42	12	13	13	2	29-241	29-249	29-261	29-361		NW by W	Calm, A.M. fleecy cumuli prevalent, P.M. shower, evg. clear, aurora borealis.						
Su.	45	47	50	48	4	0	13	0	29-426	29-450	29-574	29-642	0-500	N. W.	A.M. strong wind and rain, P.M. wind moderated, overcast, evening showery.						
Mo.	46	49	49	49	4	5	0	0	29-694	29-710	29-712	29-750		N. W.	A.M. Gentle wind, fog and rain, P.M. calm with rain.						
Tu.	50	51	51	45	3	4	8	0	29-750	29-754	29-760	29-803		N. E.	A.M. cumuli prevalent, occasional showers, P.M. fair but cloudy.						
We.	50	50	51	47	3	4	16	4	29-807	29-821	29-833	29-809		N. E.	Brisk wind, cumuli prevalent, P.M. overcast.						
Th.	51	52	53	47	2	4	9	0	29-792	29-799	29-802	29-808		E.	Brisk wind, dense cumulous clouds prevalent, P.M. sky overcast.						
Fr.	51	50	50	47	12	1	13	4	29-800	29-792	29-773	29-743	0-236	E.	Brisk wind, sky overspread with dense cumulous clouds.						
Sa.	49	51	48	47	10	10	12	14	29-807	29-799	29-790	29-870		E.	Gentle wind, occasional gusts heard, overcast or lowering, P.M. slight showers.						
Su.	48	47	49	45	4	4	12	3	29-664	29-664	29-603	29-544		N. W.	Gentle wind, A.M. shower, P.M. overcast and lowering, evg. calm and cloudy.						
Mo.	49	49	47	53	9	10	12	0	29-426	29-435	29-455	29-480		N. W.	Brisk wind, A.M. frequent showers, P.M. gradually clearing, evening calm.						
Tu.	41	43	42	36	6	12	17	14	29-394	29-386	29-429	29-329		N. W.	Calm, A.M. shower, P.M. heavy masses of clouds, evening partially clear.						
We.	40	42	44	32	17	20	26	3	29-582	29-587	29-600	29-643		N. W.	Calm, cloudy, P.M. cirrocumuli prevalent, evening partially clear.						
Th.	45	47	51	48	9	11	10	0	29-561	29-560	29-449	29-372		W.	Gentle breeze, A.M. cloudy, P.M. overcast, evening calm with rain.						
Fr.	52	53	57	52	0	0	0	0	29-153	29-153	29-135	29-261		S. E.	Calm and soft, frequent showers, P.M. wind rising, evening nearly cloudless.						
Sa.	56	56	54	51	8	12	4	3	29-323	29-340	29-232	29-300		S. W.	Brisk wind, sky overspread with soft clouds, showery, evening nearly cloudless.						
Full M.	54	50	50	51	14	17	22	3	29-323	29-355	29-393	29-410	0-060	S. W.	Brisk wind, soft clouds prevalent, occasionally in large masses, evening rain.						
Mo.	50	50	51	60	55	0	2	3	29-386	29-389	29-284	29-203		S. W.	Calm, sky overspread with soft hazy clouds, evening showery.						
Tu.	56	51	58	49	4	6	5	0	29-127	29-118	29-107	29-093		S. W.	Calm, overcast and lowering, in the evening rain.						
We.	50	53	49	45	5	11	0	0	28-917	28-889	28-848	28-794		S. W.	Calm, sudden and heavy showers, evening cloudy but fair.						
Th.	48	50	50	43	10	13	13	0	28-768	28-768	28-761	28-783	1-152	W. by S.	Calm, A.M. cloudy, P.M. occasional showers, evening heavy rain, wind rising.						
Fr.	42	43	43	38	5	10	24	5	28-859	28-859	28-726	28-861		N. W.	Brisk wind, cirri, cumuli and cirrostrati, evg. calm and clear, aurora borealis.						
Means.	49.7	50.9	51.4	46.1	8	10	13	2	29-290	29-293	29-281	29-302	4-011			Means Therm. 48°-5 } Mean temperature of spring water 46°½ for the Hygr. 6° } Mean point of deposition 46°-3 Month Bar. 29-298 in. } Moisture in a cubic inch of air = .002138 grs.					

# RECORDS

OF

## GENERAL SCIENCE.

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### ARTICLE I.

#### *Biography of M. le Comte Lagrange.*

(Concluded from page 251.)

DURING his residence in Berlin, M. de Lagrange married, not so much for any inclination for the state, it is said, as because it was customary for the academicians to be married. This union was followed by several misfortunes. M. de Lagrange had a child, who died, we are informed, while young. His wife died, likewise, after a tedious and painful disease. M. de Lagrange took care of her during her illness with the most inviolable attachment, hardly ever leaving her, and contriving new methods for her cure. This second loss rendered his abode in Berlin disagreeable: besides, he was afraid that the tranquillity of Prussia would be interrupted. These motives caused him to listen to the offers which were made to induce him to go to France, where he hoped to enjoy greater tranquillity. He reached that country in 1787, and was soon after surprised by the Revolution. He passed through it without experiencing any personal misfortune. About this period his *Mecanique Analytique* appeared. M. de Lagrange had sent the manuscript from Berlin, and had entrusted the publication of it to one of the most celebrated French mathematicians. It had been printed for two years before Lagrange even thought of opening it; and when a gentleman, to whom he communicated the circumstance, expressed his astonishment at so much indifference, "I was disgusted," said he, "with these

kinds of combinations, and I set myself to learn chemistry, which I now find easy, for it may be learned in the same manner as algebra." It is necessary to be a Lagrange to seek in algebra a model of facility. It is remarkable, that the taste for mathematics may be thus destroyed, and revive again. D'Alembert seems to have undergone the same kind of change.

In 1792, M. de Lagrange married, a second time, a young and beautiful lady, daughter of M. Lemonnier, one of his fellow members of the Academy. She rendered his life very happy. He observed in his last moments, that he found death easy, and that his regret in leaving an excellent wife could alone make it painful. When, after the events of Thermidor, public instruction was again re-established, M. de Lagrange was named Professor of the Normal School. The lectures which he there delivered have been printed. When the Polytechnic School was formed, he was likewise one of its first Professors; and those who had the happiness to hear him know with what respect he was listened to. It was then that he published his *Calcul des Fonctions Analytiques*, his *Traité des Fonctions*, and his *Resolution des Equations Numeriques*. These works composed for the Polytechnic School, were not one of the least causes of its celebrity. When the Institute was formed, M. de Lagrange was necessarily named the first member in the section of Geometry. When the Board of Longitude was established, he was appointed one of its members; and till the very last period of his life, nobody was more exact than he in his attendance at the meetings of both these learned bodies.

At the epoch of the 18th Brumaire, he was named Senator, and successively Grand Officer of the Legion of Honour, and Grand Cross of the Order of Reunion. The eclat of rank and fortune did not seduce him for a moment. He retained always the same mode of life, the same habit of study, the same simplicity. This wise conduct was the more necessary for him, because he had always been of a feeble constitution; and it was to this extreme moderation, in every thing but study, that we must ascribe the length of his life and his old age free from infirmity. He had likewise the rare good fortune to preserve his genius to the

end of his life. Indeed, if we examine the whole of his works, we shall find in them marks of the progress of the science, but no indication of old age. He had undertaken, at the latter period of his life, to give a new edition of the *Mecanique Analytique* considerably augmented. He published the first volume, in which, among other remarkable additions, we admire his fine investigations of the most general questions of astronomy and mechanics. He laboured with the most indefatigable industry at the two remaining volumes, in which he intended, it is said, to treat of the great phenomena of the system of the world; but this labour hastened the period of his death. It is said, that the manuscript of the second volume exists, written entirely with his own hand. It is to be wished, for the good of the sciences, that the publication of this precious monument be committed to persons who will acquit themselves with promptitude and fidelity.

The character of the genius of Lagrange has been exactly appreciated by a philosopher whose name in the sciences has been long associated with his own. If we durst add any thing to that judgment, it would be to confirm it, by recalling to memory the impression made upon the mind by the perusal of the works of Lagrange. It is not only the pleasure that results from a clear and accurate arrangement, it is a ray of light which darts upon the mind, removes the obscurity from the most complicated objects, and discovers to your astonished eyes the certain and direct road which leads to the object that you wish to obtain. When we have once read and understood a memoir of Lagrange, we have never any occasion to recur to it again; we have learned the whole, and never can forget it. In this generality of his views he rises above Euler. Euler, indeed, possesses other advantages: in the immense variety of his works, he lays open a multitude of extraordinary means, and a fertility of invention, which nothing can stop. Mathematicians, by reading him, learn all the secrets of the science of mathematics; but M. de Lagrange alone can offer them the model of that perfection, almost ideal, which we ought to endeavour to attain.

Notwithstanding what we have said, we should leave a very imperfect character of Lagrange, if we did not notice

his wit. He possessed it in such perfection, that it alone would have raised the reputation of any other person but M. de Lagrange. What a turn of thinking must he have had, who, by way of relaxation from the most abstract studies, made choice of the history of religion and of medicine! It is true, that in consequence of this investigation, he lost all confidence in medicine; but this scepticism was so simple and tolerant, that if it was an error, it was impossible not to forgive it. This philosopher, who knew so many things, was exceedingly ready to acknowledge his ignorance. These simple words, *I do not know*, were his favourite expression. He generally began and finished in this manner the statement of his doubts. He was not apt to be satisfied with words, nor to stop at the surface of things. He deprived opinions and things of the envelope with which they are usually covered; and when he had thus exposed them naked, he gave his thoughts respecting them, usually in an original and lively manner, as remarkable for depth of sense, as for fineness of expression. Many of his sayings are well known. One of his friends was speaking to him of an opinion which, alternately adopted and rejected, admitted and modified, by philosophers, had become at last a popular prejudice. "What!" said M. de Lagrange, "are you astonished at that? It is the very thing which always happens. Prejudices are nothing else than the cast clothes of philosophers, in which the rabble dress themselves." We state this anecdote because it points out well the nature of observations.

Though his figure was good, he would never permit his portrait to be drawn. He thought, that the productions of the mind were alone entitled to survive. If his face remains unknown, the remembrance of his genius will last as long as civilization continues to dwell upon earth.

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#### ARTICLE II.

*Abstract of a Letter from M. Cacciatore, Director of the Observatory of Palermo, respecting the Moveable Star observed by him in 1835.*

THE author began, in 1835, a series of observations on stars, which he proposed to follow out four evenings in succession,

with Ramsden's moveable vertical circle, in order to deduce corrected observations of the error in the line of collimation. These observations were principally on stars, which had peculiar motions, because he thought, that by comparing them with such as he had made before, much precision and accuracy might be attained. For, although the interval of time between the observations did not exceed 45 years at most, and was often 40 or 30 only; yet, as they were made with the same instrument, by the same methods of observations, calculation, and by the same eye, their differences were not subject to the small inequalities, which might effect observations made with different instruments and observers. The nature of the great circle of Ramsden, in his possession, not enabling him to make precise observations on the revolutionary movements of the double and triple stars, he was confined to the investigation of their peculiar motions. According to this method, it is easy to observe other stars which occasionally accompany, in the field of the eye-glass, the star under examination. Cacciatoŕe is in the habit of noting the difference in their passage of the vertical wire, and the difference in their zenith distance. When there are several stars in the field, he is in the habit of estimating these differences at sight, and experience has fully shewn, that his estimate is never far from the truth.

He had in vain, during several evenings in the month of May, 1836, prepared his instrument for observation, until the 11th, when he began his series of observations. Among the stars, which he observed, was the 503rd of Mayer, which is the 17th of the 12th hour of Piazzini; it is of the 7th or 8th magnitude, and has, according to the catalogue, a peculiar angular motion in right ascension —  $0^{\circ}.33$ . Near it he saw another star of less lustre, and a little smaller, which followed it at a distance of about two seconds of time to its passage, and which was about  $2\frac{1}{2}$  minutes farther to the south. This constituted the whole of the observation; the star was noted, and no more was thought of it. Next day the weather was unfavourable, and it was only on the 14th, that the observations were resumed. When his assistant read to him the note which he had made relating to the small star, he was surprized at not finding it—he feared that he had committed an error—he diminished

the light of his glass, and quickly perceived a star of the 8th magnitude preceding the principal by 8 seconds in right ascension, and only  $1\frac{1}{2}$  minute to the south. He noted these facts, and left off his observations, resolved to leave, on the following evening, the star of Mayer for the purpose of following the new one. But unfortunately the sky became more unfavourable than on the preceding night; it continued constantly in a cloudy or rainy state till the end of the month, and, when on the evening of the second of June he resumed his research, the twilight was so strong, that he could do nothing. He had hopes, that in the course of the present year, he might be able to ascertain something of a satisfactory nature; but from the month of September, 1835, to March, 1836, the season continued so remarkably unpropitious, that he was able to make but a very limited number of observations, even on Halley's comet. For 38 years that he has resided in the Observatory, he states, that he never knew of a similar occurrence. He had made, however, during some mornings in the month of January, all possible observations, being strongly encouraged by the circumstance, that near the 503rd star of Mayer, there was no other star. He pursued his investigations in those regions which appeared most convenient, abandoning, for this purpose, all his other labours, in order to devote himself entirely to this research, to prevent any of the rare intervals from the covered state of the sky from being lost; but all in vain.

Disappointed by such loss of time, and fruitless labour, he conceives, that he would be wanting in his duty, if he did not invite astronomers to look out for the new star, which, from the facts described, undoubtedly exists in the heavens. He has given up all idea of discovering it himself. But who knows if some more fortunate astronomer may not discover quickly, near the constellation of the Virgin, this star which presented, in three days, a motion less than a minute of a degree in right ascension, and less than half a minute of declination. If we consider it placed beyond Herschel, as appears natural, it ought to have a semi great axis double of that of Herschel, as Cacciatore pointed out in his memoir on the return of Halley's comet, published in May, 1835, at the time when he was in full hopes of

again seeing the new star. Cacciatore has addressed these observations to the *Bibliothèque Universelle*, for the purpose of attracting the attention of astronomers; and we have inserted them here, in order that the scientific men of this country may be aware of the facts.

ARTICLE III.

*On the Minerals containing Columbium.* By THOMAS THOMSON, M. D., F. R. S., L. & E., Regius Professor of Chemistry in the University of Glasgow.

THE minerals containing columbium are so scarce, that I am not aware of any attempt hitherto made to draw up a mineralogical description of them; far less to subject them to a chemical analysis. Dr. Torrey of New York, was kind enough during the course of the present summer to send me several specimens of columbite, from a new American locality, together with a request to subject the mineral to a chemical analysis. It turned out on examination to be a new species, not hitherto mentioned by mineralogists. This led me to examine the Bohemian columbite, a specimen of which, liberally presented to me more than 20 years ago, by Mr. Heuland, I have in my cabinet. This last is the variety to be found in mineral collections, and seems to be the same with the specimen of columbite in the British Museum, originally examined by Mr. Hatchett. From a statement in Haidinger's edition of Mohs's Mineralogy,\* it appears that this mineral has been analyzed by Vogel and Count Borkowski, the results of whose analyses are given; though I do not know where they were published.†

The tantalite from Kimito in Finland, originally examined by Ekerberg, and afterwards analyzed by Berzelius, constitutes a third species.‡ While another specimen from

\* Vol. ii. p. 392.

† These results are as follows:—

Columbic acid, . . . . .	75	74
Oxide of tin . . . . .	1	0·4
Oxide of iron . . . . .	17	20
Oxide of manganese . . . . .	5	4·6
	98	99

‡ Afhandlingar, iv. 262.

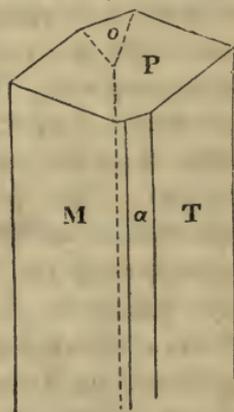
the same locality discovered by Nordenskjöld, and likewise analyzed by Berzelius, constitutes a fourth species.\* I propose in this paper to give a short account of the characters and chemical constitution of these four species.

### 1. *Torrelite*.

I give this name to the new species, which I have just received from New York, by the liberality and kindness of Dr. Torrey. I have been induced to name it after my much respected friend, as a slight acknowledgment to him for the many interesting and new minerals with which he has from time to time favoured me.†

Torrelite has been found lately in a granite rock at Middleton, in the state of Connecticut, where it is occasionally disclosed by the workmen, who are blasting out felspar for a porcelain manufactory.

It occurs most commonly in irregular masses about the size of a filbert; though occasionally, as appears from a fragment of a crystal in my possession, it is crystallized. The figure in the margin represents this fragment. It is a four sided prism, the base of which, P, is so rough and irregular, that we cannot decide whether it forms a right angle with the faces of the prism. The longitudinal faces of the prism are sensibly equal. The edge between M and T is re-placed by a narrow face *a*. The only measurements that I could make are the following :



M on T (mean of 5 trials)	. . .	84° 20'
T on <i>a</i> (mean of 3 trials)	. . .	152°
M on <i>a</i> (mean of 3 trials)	. . .	110° 20'

\* Afhandlingar, vi. 237.

† The name *Torrelite* has been already given by Professor Renwick to a mineral of a vermilion colour, which he subjected to analysis, and which he found to contain oxide of cerium, but which was afterwards examined by Mr. Children and Mr. Faraday, without their being able to discover any cerium (see *Annals of Philosophy*, 2nd series, ix. 217 and 221). Ten years having elapsed since these experiments were made, and no mineralogist, so far as I know, having noticed Mr. Renwick's torrelite, I take it for granted that its claims to rank as a new species have not been admitted.

The longitudinal faces of the prisms are quite smooth. It is probable, from this crystal that the primary form of torrelite is a right oblique prism terminated by a rhombic bar, whose angles are  $84^{\circ} 20'$  and  $95^{\circ} 40'$ . The triangular face *o* is too rough on the surface for measurement. It is sensibly an equilateral triangle, and its inclination to the adjacent edge, is about  $143^{\circ} 45'$  from a mean of several trials.

The colour is black, or at least much darker than that of columbite. The surface is iridescent, with a play of blue and red colours.

Lustre imperfectly metallic, almost resinous, being very similar to that of cherry coal. In one direction (parallel to face M) it is imperfectly foliated. Cross fracture granular. Opaque.

Hardness 4.25. Specific gravity 4.8038.

Before the blow-pipe both with carbonate of soda and borax, it fuses into a dark red bead, shewing the presence of iron. With a great excess of carbonate of soda, the green colour characteristic of manganese makes its appearance. When reduced to a fine powder the colour is dark chocolate brown.

1. 100 grains being exposed to a red heat, lost 0.35 grains of weight, which was considered as water.

2. 20 grains of the mineral in fine powder were mixed with 160 grains of crystallized bi-sulphate of potash, and gradually heated in a platinum crucible, over an Argand's spirit lamp. Care was taken to keep the heat moderate till the ebullition of the salt was at an end. It was then raised to redness, and the whole was kept in fusion for, at least, a quarter of an hour.

3. The red mass on cooling became white with an exceedingly slight tint of slate blue. It was softened in water and finally digested in muriatic acid for 24 hours. The whole was then thrown on a filter, and the white powder collected on the filter was thoroughly washed with boiling hot water. It was then dried in the open air, and digested for 24 hours in a solution of caustic ammonia.

4. The ammoniacal solution was then filtered off, and saturated with muriatic acid. A white flocky precipitate fell, which, after ignition, assumed a shade of brown, and weighed 0.09 grain.

Tested by the blow-pipe, it fused with effervescence, with carbonate of soda forming a very white opaque bead. With borax and bi-phosphate of soda, it fused into transparent beads, the latter of which, had a very slight tint of yellow. From these phenomena, I considered it as columbic acid very slightly contaminated with iron. It was evidently not tungstic acid, in order to discover which, I had been induced to digest the columbic acid in ammonia.\*

I was not aware before that columbic acid is soluble in ammonia; but verified the fact by subsequent trials. The quantity dissolved, however, is always very small; and after ignition columbic acid becomes quite insoluble in ammonia.

5. The residual columbic acid being ignited, weighed 15·24 grains. While red hot, it had a distinct tint of yellow, and when allowed to cool it retained a perceptible brownish tinge. It was therefore, mixed with six times its weight of anhydrous carbonate of soda and fused.

The fused mass, when cold, had a light green colour shewing the presence of manganese. It was softened with water, digested in muriatic acid, and the whole thrown on a filter to collect the columbic acid; which was thoroughly washed with boiling water, dried and ignited. It weighed after ignition 14·69 grains. Thus, making the whole columbic acid 14·78 grains (adding what had been dissolved in ammonia). It was beautifully white, and apparently pure.

The muriatic solution, containing the soda and impurity from the columbic acid, was boiled with an excess of carbonate of soda in a flask. A dark brown matter fell, which beingedulcorated, dried, and ignited, weighed 0·55 grain, and was red oxide of manganese.

6. The muriatic acid solution (paragraph 3) was as nearly neutralized as possible, and then mixed and digested with benzoate of ammonia. The iron was thrown down in the state of benzoated peroxide. After ignition, it weighed 3·48 grains = 3·13 grains protoxide of iron.

7. The liquid thus freed from iron was mixed with an excess of carbonate of soda and boiled. The precipitate that fell, afteredulcoration and ignition, was red oxide of manganese, and weighed 1·23 grains: making, with the

\* Tungstic acid had been discovered by Ekeberg in *tantalite*, and his discovery had been confirmed by Berzelius.

0.55 grain, (of paragraph 5) 1.78 grains of red oxide of manganese = 1.6 gr. protoxide.

Thus, the constituents of torrelite are,

Columbic acid, . . . . .	14.78	73.90
Protoxide of iron, . . . . .	3.13	15.65
Protoxide of manganese, . . . . .	1.60	8.00
Water, . . . . .	0.07	0.35
	<hr/>	<hr/>
	19.58	97.90

To determine the atomic constitution of this mineral, we must recollect, that the atomic weight of columbic acid is 25.75, of protoxide of iron 4.5, and of protoxide of manganese 4.5. If we divide the preceding numbers by these atomic weights we obtain,

	Atoms.
Columbic acid, . . . . .	2.87 or 1.6
Protoxide of iron, . . . . .	3.48 or 1.96
Protoxide of manganese, . . . . .	1.77 or 1.

These numbers leave no doubt, that the true constitution of the mineral is,

- 1½ atoms columbic acid
- 2 atoms protoxide of iron
- 1 atom protoxide of manganese

The atoms of the bases being twice as many as those of columbic acid, we see that torrelite is composed of dicolumbates: and, as there are twice as many atoms of protoxide of iron as of protoxide of manganese, it is obviously composed of

- 2 atoms dicolumbate of iron
- 1 atom dicolumbate of manganese

So that the formula indicating its constitution is,  $2f^2 \text{Cl} + mn^2 \text{Cl}$ .

## 2. *Columbite*.

The name *columbite*, given by Mr. Hatchett to the original specimen in the British Museum, may with propriety be applied to the Bohemian specimens; because, there are strong reasons for considering the constitution of both as very nearly alike.\* I do not know who was the discoverer of it at Bodenmais, in Bohemia. But as I have already mentioned, I got a specimen of the Bohemian columbite

\* This, indeed, has already been done by M. Gustav Rose.

about 22 years ago from Mr. Henland, and I think it probable, that it was about that time, or not long before it, that this mineral was discovered. No allusion is made to it in Klaproth's paper on the analyses of the Finland Tantalite written in 1809.\* And, what is more singular, Hoffman in the last volume of his Mineralogy, published in Freiberg in 1817, makes no mention of the Bohemian columbite, and says, that the specific gravity of the tantalite, as stated by Wollaston and Klaproth is too low,† shewing clearly, that at that time he was ignorant of the existence of the Bohemian columbite, though, before that period, I had a specimen of it in my cabinet. Haüy in his *Traite de Mineralogie*, 2nd edition, published in 1822, (tom. iv. p. 391) notices the Bohemian columbite, which he calls tantalite, and which, he says, had been recently discovered at Bodenmais, in a granite which contained also beryls, iolite and oxide of uranium. But it is obvious from his statement, that he was not aware that any difference existed between the Bodenmais columbite and the tantalite of Finland. For he gives 6.5 as the specific gravity of tantalite, shewing that he had never seen, or, at least, never examined the Bohemian columbite.

Mr. William Phillips, in the third edition of his Mineralogy (page 270), published in 1823, notices the Bohemian columbite. But he gives its specific gravity 6.464, and states its primary form to be an oblique four-sided prism of  $94^\circ$  and  $86^\circ$ . A statement inconsistent with the measurements of a crystal in possession of Mr. Brooke, and which he himself gives on the authority of that eminent crystallographer. A comparison of these measurements with those given in Haidinger's translation of Mohs's Mineralogy (vol. iii. p. 390), leaves no doubt, that Mr. Brooke's crystal was from Bodenmais, and that it is specifically different from the Finland tantalite.

M. Gustav Rose, in his *Elemente der Krystallographie*, published in 1833, has divided the minerals, previously confounded together on the continent of Europe under the name *tantalite*, into two species. The one, consisting of the specimens found at Bodenmais and the Massachusetts specimen in the British Museum, he calls *columbite*: the

\* Beitrage, v. 1.

† Hoffman's Handbuch der Mineralogie, iv. 2, 193.

other, consisting of the specimens from Finland, discovered by Nordenskjöld, and which give a cinnamon brown powder when pounded, he calls *tantalite*. He mentions a fine crystal of *columbite* in the Royal Collection at Berlin, in which the faces on both sides of the crystal are exposed; but he was not able to measure its angles, nor to determine its structure.

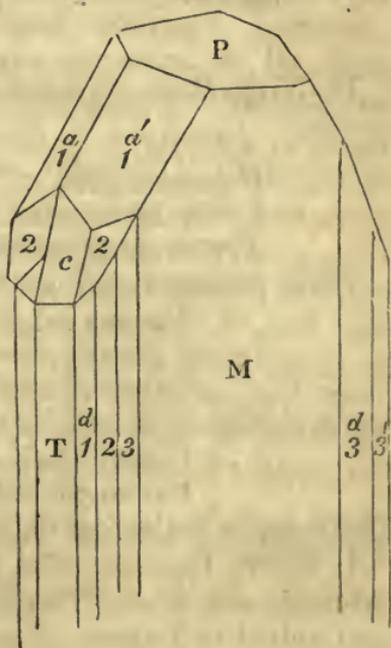
The specimen of Bohemian columbite in my cabinet is a portion of a crystal about an inch in length, and wanting both the terminations. Its length was originally about  $1\frac{1}{2}$  inch; but I broke a fragment off it to enable me to subject it to a chemical analysis.

Its colour is *black*, but it is lighter than that of torrelite. And, when reduced to a fine powder, it still retains its black colour. But, when the powder is heated to redness, it changes to chocolate brown like that of torrelite; though it loses only  $\frac{1}{20000}$ th of its weight.

The structure is foliated. Only two faces of the crystal, which is a flat four-sided prism, are smooth enough for measurement. They meet at an angle of  $90^\circ$ , shewing that the prism is rectangular; and Mr. Brooke has ascertained, that the primary form is a right rectangular prism. The faces of the prism are streaked longitudinally.

The following are the measurements by Mr. Brooke of a crystal in his possession :

P on M or T . . . . .	$90^\circ$
M on T . . . . .	90
P on $a^1$ or $\acute{a}^1$ . . . . .	$136\ 30'$
P on $c$ . . . . .	120
T on $d^1$ . . . . .	$156\ 30$
T on $d^2$ . . . . .	$114\ 30$
T on $c$ . . . . .	150



Opaque. Lustre semi-metallic, inclining to resinous. Fracture imperfect conchoidal.

Hardness 6 or 6·25. Specific gravity by my trials 6·0380. This is exactly the specific gravity given in Haidinger's translation of Mohs's Mineralogy, shewing that his description is of the same mineral with that in my possession.

The phenomena, before the blow-pipe with columbite, are the same as with torrelite. The presence of iron and manganese being indicated.

I analyzed 20 grains of Bohemian columbite precisely in the same way as I had already analyzed torrelite. But as my quantity was limited, I was at more pains to prevent any loss. The consequence was (as is frequently the case under such circumstances), that I obtained a slight excess. It is proper to mention also, that when the peroxide of iron extracted from the mineral was ignited, and then digested in muriatic acid, it left 0·2 grain of columbic acid. Thus, shewing that columbic acid, before ignition, is not only slightly soluble in ammonia, but also in muriatic acid. The result of my analysis was as follows :

Columbic acid, . . . . .	15·93	79·65
Protoxide of iron, . . . . .	2·80	14·00
Protoxide of manganese, . . . . .	1·51	7·55
Oxide of tin, . . . . .	0·10	0·50
Moisture, . . . . .	0·01	0·05
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	20·35	101·75

Dividing these numbers by their atomic weights, we obtain,

	Atoms.
Columbic acid, . . . . .	3·09 or 1·987
Protoxide of iron, . . . . .	3·11 or 2·
Protoxide of manganese, . . . . .	1·67 or 1·07

These numbers approach very nearly to

2 atoms columbic acid  
2 atoms protoxide of iron  
1 atom protoxide of manganese

which evidently shews the constitution of columbite to be  
1 atom dicolumbate of iron  
1 atom columbate of manganese

The formula indicating its composition is,  $f^2 \text{Cl} + mn \text{Cl}$ .

It differs from torrelite by containing half an atom of columbic acid more. Torrelite consists of  $1\frac{1}{2}$  atom columbic acid united to 3 atoms of oxides of iron and manganese,

while columbite consists of 2 atoms columbic acid united to 3 atoms of the same bases.

Dr. Wollaston analyzed 5 grains of the original specimen of columbite in the British Museum, and obtained,

Columbic acid, . . . .	4	or 80
Protoxide of iron, . . .	0.75	15
Protoxide of manganese,	0.25	5
	5.00*	100

This approaches pretty nearly to my analysis. We cannot expect minute accuracy in an analysis conducted on so small a scale. But it is near enough, I conceive, to leave no doubt about the identity of the columbite in the British Museum and that found at Bodenmais. This is farther corroborated by the specific gravity of the British Museum specimen, which Mr. Hatchett found to be 5.918.

### 3. *Tantalite*.

The specimens of tantalite from Finland, by the examination of which Ekeberg discovered the metallic substance to which he gave the name of *tantalum*, were sent him by M. Geyer; and it is not accurately known from what part of Finland they came. Berzelius conjectures that they had been found at Skogsböle, in the parish of Kimito, where an attempt had been made to obtain tin, for the oxide of which tantalite had been taken. At Ekeberg's death, his mineral collection was purchased by Dr. Mac-michael, who was liberal enough to give to Berzelius all the specimens of tantalite which it contained. One of these consisted of a small piece, labelled by Ekeberg as having a specific gravity of 7.236. The rest had been reduced to powder, obviously for analysis.

Berzelius gives no description of this tantalite, and the quantity of it was so small, that he was able to use only one gramme or 15.433 grains; and, though some anomalies occurred in the analysis, it was not in his power to remove them by repeating it. Klaproth obviously employed the same mineral in his analysis of tantalite. He gives the following short description of it:

Colour, iron black, amorphous. Lustre, semi-metallic,

\* Phil. Trans. 1809, . p. 248

hard, brittle. Colour of streak, grayish black with a shade of brown. Specific gravity 7·300.\*

Its constituents, as determined by the analysis of Berzelius, are

Columbic acid, . . . . .	83·2
Protoxide of iron, . . . . .	7·2
Protoxide of manganese, . . . . .	7·4
Oxide of tin, . . . . .	0·6
	98·4 †

Dividing these numbers by the atomic weight of each, we obtain,

	Atoms.
Columbic acid, . . . . .	3·23 or 2·018
Protoxide of iron, . . . . .	1·6 or 1·
Protoxide of manganese, . . . . .	1·64 or 1·025

These numbers leave no doubt, that the constitution of tantalite is

- 2 atoms columbic acid
- 1 atom protoxide of iron
- 1 atom protoxide of manganese

Thus, the number of atoms of the acid and of the bases is equal. It is composed obviously of

- 1 atom columbate of iron
- 1 atom columbate of manganese

So that the formula indicating its constitution is  $f\text{Cl} + mn\text{Cl}$ .

It differs from columbite in containing 1 atom less of protoxide of iron, united with the same proportion of columbic acid and columbate of manganese.

#### 4. *Ferrotantalite*.

Among the specimens of tantalite found in Ekeberg's collection, there was one in powder, which had the colour of rust, and which was marked as obtained from a single tantalite crystal, whose specific gravity was 7·936. This powder Berzelius attempted to analyze in 1815; but the result was unsatisfactory. In 1818, he received from Nordenskjöld a specimen of a tantalite from Kimito, which, when pounded, gave a cinnamon brown power, which he

\* Beitrage, v. 2.

† Afhandlingar, iv. 262.

subjected to analysis.\* This variety, or rather, this new species, I distinguish by the name *ferrotantalite*; because it consists almost wholly of columbic acid combined with protoxide of iron.

Colour black.

In irregular masses, with some indications of crystalline faces; though it is impossible to make out the shape of the crystal.

Lustre metallic, and, in general, greater than that of common tantalite. Internal lustre often less, owing to large rents in the mineral, the surfaces of which exhibit a rainbow tarnish.

Fracture uneven. Hard enough to scratch glass. Specific gravity 7.655.

Powder dark reddish brown, becoming lighter the more finely it is pounded.

Not acted on by acids.

Before the blow-pipe, *per se*, not altered. With borax, when in solid pieces, it dissolves very slowly, or not at all. In fine powder, it dissolves very slowly. The glass has a green colour, in which white particles float: and it does not become milky by flaming. In bi-phosphate of soda, it dissolves much more easily, and the glass has the same colour as when common tantalite is employed. The addition of saltpetre indicates a small quantity of manganese.

With carbonate of soda it does not dissolve. But when it is heated with a mixture of carbonate of soda and borax on charcoal, and exposed to a good reducing heat grains of tin are obtained.

Being subjected to analysis by Berzelius, he obtained,

Columbic acid, . . . . .	85.85
Protoxide of iron, . . . . .	12.97
Protoxide of manganese, . . . . .	1.61
Oxide of tin, . . . . .	0.80
Lime, . . . . .	0.56
Silica, . . . . .	0.72

102.51†

\* Afhandlingar, vi. 237.

† Afhandlingar, vi. 243. The excess is probably owing to over-rating the quantity of columbic acid in the mineral. Had the amount been 83.43, the atoms of acid and bases would have been the same. Now, the difference between 85.85 and 83.43 is 2.42, which is very nearly the amount of the excess.

Dividing these numbers by the atomic weights of the bodies, we obtain,

	Atoms.
Columbic acid, . . . .	3.33 or 1.
Protoxide of iron, . . .	2.88 } or 0.97
Protoxide of manganese, .	0.36 }

obviously, 1 atom columbic acid

1 atom protoxide of iron and manganese.

The difference between it and common tantalite is, that instead of 1 atom protoxide of iron and 1 atom protoxide of manganese, united with two atoms of columbic acid, it consists of 2 atoms of columbic acid, combined with 1.777 atoms protoxide of iron, and 0.223 atoms of protoxide of manganese. Or, which comes to the same thing, we may consider it as composed of

9 atoms columbic acid

8 atoms protoxide of iron

1 atom protoxide of manganese

So that the formula indicating its constitution is,  $8f\text{Cl} + mn\text{Cl}$ .

Such are the characters, and such the constitution of the four species of minerals composed of columbic acid, united to protoxide of iron and protoxide of manganese. The formulas indicating their constitution are as follow :

1. Torrelite, . . . .  $2f^2\text{Cl} + mn^2\text{Cl}$ .

2. Columbite, . . . .  $f^2\text{Cl} + mn\text{Cl}$ .

3. Tantalite, . . . .  $f\text{Cl} + mn\text{Cl}$ .

4. Ferrotantalite, . .  $8f\text{Cl} + mn\text{Cl}$ .

Let us now compare their specific gravities with each other.

1. Torrelite, . . . . 4.8038

2. Columbite, . . . . 6.0380

3. Tantalite, . . . . 7.3000

4. Ferrotantalite, . . 7.6550

Thus, as the columbic acid increases, the specific gravity increases, shewing the high specific gravity which must belong to columbic acid.

The crystalline shape of torrelite and columbite is different. It is probable, that tantalite and ferrotantalite have each a peculiar crystalline shape, although, from the variety of their minerals, and the imperfect state of the crystals, that point has not been ascertained.

Torrelite and columbite differ in their hardness, that of the former being 4.25, of the latter 6.25. Tantalite and ferrotantalite are described as hard; but we know not the exact amount.

From the preceding observations and analyses, I conceive, that no doubts can remain either with mineralogists or chemists, that the four minerals described in this paper constitute four different species.

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#### ARTICLE IV.

##### *Experiments and Observations on Visible Vibration.*

By CHARLES TOMLINSON, ESQ.

(Continued from page 20.)

#### ON THE VIBRATION OF PORCELAIN AND EARTHENWARE VESSELS.

116. The investigation of the vibratory action of vessels of other material than glass has been purposely deferred until now, because, from a few phenomena already alluded to (75), it would seem, that the vibration of porcelain, earthen, and wedgwood-ware vessels is peculiar, and, therefore, deserving a separate inquiry.

It has been said (75), that two tones can be produced from earthenware mugs, &c., and Mr. Dodd found that with a common blue cylindrical cup, two notes D and E were produced separately, and alternated four times during one revolution of the finger: that is, there were four points producing D, and four other points producing E. The additional phenomena noticed (75) are not very precisely stated, as the mode of vibration of these vessels, then adopted by Mr. Dodd, "by striking the edge gently with a quill," is a very imperfect one, and not capable of producing the decisive results, which the judicious application of a well rosined bow effects. I, therefore, propose in this paper, to commence a statement of the results of my inquiry into the vibratory action of vessels of porcelain, china, earthenware, metal and wood. The very curious results which I have obtained, may, probably, be thought sufficiently important to allow me to state them somewhat fully.

117. The circumstance of two notes so near together as D and E, being produced from one vessel, is, at first view, by no means in accordance with the principles endeavoured to be established in my last paper on the fundamental and secondary tones, and nodal divisions of a glass goblet (95 to 115). With earthenware vessels, Mr. Dodd states, that between the two notes produced, he never obtained an interval of more than three semi-tones:—that these two notes occurring alternately at eight distinct points of the vessel, led Mr. Dodd to suppose the existence of eight vertical nodal lines, each of which, separated two vibrating arcs, which produced different notes (75). I have shewn, that in glass vessels, the fundamental tone is always due to a quadripartite division, and that eight divisions produce a note generally in the second octave above the fundamental note: the octopartite division is, in fact, my second secondary tone, and as I could not think but that a certain harmonious relation, both in theory and practice must exist between the modes of vibration in glass and porcelain, &c. I suspected some fallacy in the theory, which sought to explain the production of the two notes D and E above referred to. I, therefore, proceeded as follows:

118. I procured a circular block of wood, from the centre of which rose a short cylinder of the same material, two inches in length and one inch in diameter: the top of this cylinder was covered with soft leather, and a female screw passed down it. I then procured a thin wedgwoodware evaporating dish with a flat bottom and a lip:—the dish was five inches in diameter, and about one inch deep. In the centre of the dish I drilled a hole, through which a thumb screw with a leather collar was passed, and so fixed the dish to the top of the wooden cylinder. In this way the dish was quite firm and secure, and vibration produced no jarring or unsteadiness, and being held in the centre, the tones of the vessel were not damped.

119. Matters being thus arranged, I vibrated the vessel at four equidistant points of the rim by means of a bow, and obtained E within the stave, and at four other points midway between, I obtained the third below, namely, C. This note C was produced at the lip, at the point opposite,

and at the two points midway between. I then poured coloured water into the dish, which reduced the notes to B flat and D sharp : each of the notes produced four fans, thus distinctly indicating a quadripartite division in each case, and presenting the apparent anomaly of *two* fundamental notes from the same vessel.

120. Analogy led me to suppose, that the secondary tones would be doubled also ; that is, that I should get two secondary tones due to sex-nodal division, each producing six fans on the surface of the water ; two secondary tones due to octo-nodal division, each producing eight fans, and so on. I was not long in producing the first secondary tone, which was D sharp in the second octave producing six fans, and I also obtained another note G in the third octave also producing six fans ; thus, offering another apparent anomaly of *two* first secondary tones. The D sharp was produced at six equidistant points on the rim of the basin including the lip, and the G was produced at six other points midway between, and which did not include the lip. I was not able to produce more than one octo-nodal tone, the vessel not being sufficiently elastic, and the lower notes tending to prevail.

121. I then procured another wedgwood-ware basin, and mounted it in a manner similar to the last. The dimensions of this basin were larger, its diameter being  $6\frac{1}{2}$  inches, and depth  $1\frac{1}{2}$  inch. When empty, this vessel at four points of the rim gave F, and at the other four points B flat, both within the octave, shewing an interval of a fifth. It also yielded two secondary tones, E and G, in the first octave, thus shewing an interval of a third. Containing water, the two fundamental notes gave four fans each ; and the two first secondary tones six fans each, and thus the agreement with the former wedgwood-ware basin was complete.

122. I have submitted to experiment a great number of tea cups, both of china and earthenware, and also common blue cylindrical half pint cups, &c., and have always obtained from each two fundamental notes, the interval between which, has varied from a second to a fifth, and where I have been able to get secondary tones of the first kind due to sex-nodal division, I have always obtained two,

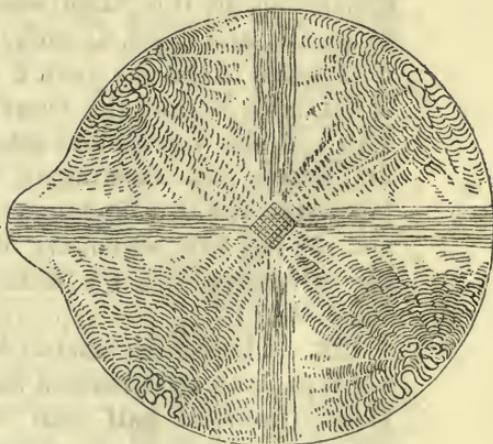
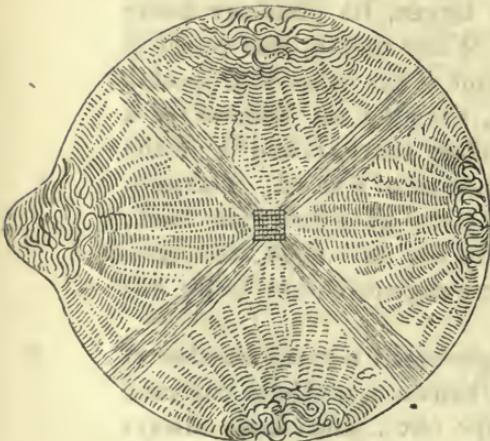
the interval between which has been a second or a third ; but with small vessels it is difficult to obtain secondary tones.

123. The explanation of the above phenomena, where vessels with lips and handles had been employed, appeared very easy ; but if the same phenomena were producible from vessels of china-ware, &c., *without* lips or handles, such as saucers, basins, plates, &c., then the theory would be very obscure. I, therefore, vibrated a variety of such vessels as were not furnished with lips or handles, and in no case produced more than one fundamental note, and one secondary tone of each kind ; that is, I could procure the first and second secondary tones, but neither these latter nor the fundamental were ever doubled.\* In these cases every thing agreed with my past experience, and the views before detailed (99), which I then stated, I believed would apply to vessels of porcelain and crockery ware, as well as of metal.

124. The cause, then, of the production of two fundamental notes, and of two secondary tones of the same kind from the same vessel, is to be found in the lip or handle, and a reference to the two following figures will at once explain the whole cause.

Fig. 1.

Fig. 2.



\* I have examined basins of many kinds, where the rims are plain, and where they describe waving lines ; also basins where the rim is turned down, and presents a horizontal surface, as a wash-hand basin, and in none of these cases was the notes of each kind double.

Whenever a note is produced, where the centres of vibration include the lip or handle, the lower fundamental note will be produced, as in Figure 1; but where the centres of vibration do not include the lip or handle, then the upper fundamental note will be produced, as in Figure 2. The difference between these two cases is, that in Figure 1, where a centre of vibration is in the lip, the velocities of the vibrations of the vessel are diminished, the lip clogging the whole vessel: the result is, therefore, a lower note than in Figure 2, where the lip occupies the place of a node, and the four vibrating arcs being scarcely impeded by the lip, the result is a higher tone. A similar explanation applies to the production of the two secondary tones of the first kind, except that, with the lower note of the two, one-sixth part of the vessel is damped by the lip or handle instead of one-fourth, as in the former case. A vessel with both a lip and a handle does not alter the result, double fundamental notes and secondary tones being produced. If a common blue cylindrical cup with a handle be employed, my theory can be proved in an interesting manner. Such a cup will yield two fundamental notes with an interval of about a third between them; but if the handle be struck off, the lower fundamental note ascends nearly, and in some cases, quite to the upper, and the two first secondary tones merge into one. It is necessary that the handle be removed carefully:—a small saw moistened with turpentine effects the purpose best, and the part where the handle was affixed must be filed down smoothly, and on a level with the exterior surface of the cup. In one case, where this amputation was nicely performed, an interval of a fifth between the two fundamental notes of the cup with a handle, was reduced to unison when that appendage was removed, the upper note of the two, of course, always being the one fundamental note remaining.

125. Supposing this explanation to be correct, I saw no reason why, *à priori*, vessels of glass, provided they were lipped or furnished with handles, should not yield similar results. In this supposition I was not disappointed, for, on vibrating a small lipped chemical test glass  $1\frac{3}{4}$  of an inch in diameter containing coloured water, I got two fundamental notes, each producing four fans. One note

was C sharp in the second octave, and the other D in the third octave; and although, this result obtained with a variety of small lipped glasses, and only an interval of half a tone was obtained, yet this interval was decided, and tended to confirm the theory of these curious phenomena. On extending the inquiry to large glass vessels with lips or handles, I obtained intervals varying from a second to a fifth between the two fundamental tones, and thus, the agreement between vessels of glass, and of earthenware, &c., was complete.

126. Another result was interesting. A wedgwood-ware funnel, fluted on the inside, gave two tones with an interval of not quite half a tone between each. This funnel had neither lip nor handle, and the results obtained from it appeared at first to contradict the explanation before given; whereas, they confirm it:—thus, it often happens, that an apparent exception to a rule, if viewed in a proper light, becomes as strong a confirmation of its truth as a direct experiment does, and in this case, the ribs or flutes of the funnel being so many bulgings which increase the thickness of the funnel at those parts, act in a similar manner to lips and handles in the other vessels. When the funnel contained water four fans were produced on its surface from each note, and the lower note was produced when the bow was applied to a point of the rim of the funnel immediately above a rib or flute; the upper note was produced when the bow was applied at a point of the rim between two ribs. Another funnel not ribbed, but containing on the exterior near the rim a thick ornament for the maker's name, afforded similar results; this ornament performing the part of the handle or lip in other vessels.

127. The wedgwood-ware basins above referred to, have each yielded one second secondary tone due to an octopartite division of the vessel; but in consequence of their high pitch, I could not determine their value. In each case, the eight fans were well marked, and on applying the bow between two fans with a view to elicit a different note also due to octo-nodal division, the four lower notes always so prevailed, that I could only with difficulty get one second secondary tone.

128. Thus far I had proceeded with vessels with lips and handles, when it occurred to me, that the very same phenomena could be produced from glass goblets, provided one portion of the rim of these vessels were damped. I, therefore, procured a damper (made of steel), which was screwed firmly upon the upper part of the glass goblet, pressing both surfaces, and the points of contact were covered with leather.

Upon vibrating the glass containing coloured water, I obtained two fundamental notes:—two secondary tones of the first kind, and two secondary tones of the second kind. Thus, the analogy with the lipped vessels was complete. I need only describe here the production of the two fundamental notes.

If we take the whole rim of the glass, and call that part to which the damper is fixed zero, the bow applied at  $90^\circ$ ,  $180^\circ$ , and  $270^\circ$ , produces a note about a fifth lower than the fundamental note of the glass without the damper, because in this case, the damper is on a centre of vibration. But if the bow be applied at  $45^\circ$ ,  $135^\circ$ ,  $225^\circ$ , and  $315^\circ$ , the fundamental note of the glass is unchanged, because the damper in such case, occupies the place of a node.

When two or more dampers are employed, interesting results are obtained, which will be stated hereafter, as they do not belong to the immediate subject of this paper.

129. A disk of glass 12 inches in diameter, and fixed in its centre in a horizontal position had a damper screwed to its edge. The surface of the plate was covered with a layer of water, which latter was retained on the disk by means of a ridge of bees' wax passed round the disk a little within its circumference.\* This disk afforded two fundamental notes, and double secondary tones of the first and second kinds. The results, then, as obtained from a glass goblet with a damper, and a glass disk similarly furnished are precisely the same, as when vessels with lips and handles are employed.

130. From what has been already stated, it will be seen that it is easy to determine, previous to experiment, the exact points on the periphery of a lipped vessel, or on a

\* In this way a disk of glass exhibits most of the phenomena of a goblet containing water. Indeed, the disk may be considered as a very shallow glass vessel.

vessel with a handle, as well as on a glass goblet or disk furnished with a damper, where the lower fundamental tone can be produced, and where the upper. Thus, suppose a cylindrical china cup with a handle be chosen. The vessel may be about half filled with coloured water, and the point of the rim exactly opposite to the handle, and at  $90^\circ$ , on each side of the handle will yield the lower fundamental tone, and four fans will be seen upon the surface of the water, the fan which proceeds from the quadrant which contains the handle being smaller in size, and more feeble in action than the other three fans. At the two points  $45^\circ$  on each side of the handle, and at two other points  $45^\circ$  on each side of the point diametrically opposite to the handle, the upper fundamental tone is produced, the handle and the three points, whence the lower fundamental tone was obtained, now forming the nodes in the upper fundamental tone, when it will be observed, that the four fans are all equal, because there are *four* vibrating arcs in action unimpeded by lip or handle; whereas, with the lower fundamental tone unimpeded action pertains to *three* only of the vibrating arcs, the fourth containing the lip or handle.

With the same vessel two secondary tones of the first kind may be produced. Each one of these divides the vessel into six vibrating sectors. To produce the lower secondary tone, the handle must, as before, be included in a vibrating sector, and from the handle a radius of the circle described by the rim of the cup will divide the periphery into six equal parts, so that if one leg of a pair of compasses rest on the point at the handle, and the space between the two legs be equal to a radius, the second leg will extend to a centre of vibration, from which the lower secondary tone can be produced, and so on all round. The upper secondary tone due also to sex-nodal division will be found at the two points situate  $30^\circ$  on each side of the handle, and at four other points from these, the length of a radius being between any two points.

131. I have employed the term *points* in order to give a precision to my meaning, but it must not be understood literally, because as a vibrating sector always includes  $90^\circ$  for the fundamental tone in any circular vessel with a lip or a handle; and  $60^\circ$  for the first secondary tone, the bow

may include a range of so much, and produce the same note from any part of the sector; but the centre of vibration is always the centre of a sector, and the part to which the bow is applied, becomes, therefore, both a centre of vibration and the centre of a sector. It will, therefore, be seen, that, with vessels with lips or handles, the bow may be allowed considerable range, provided, that in producing the lower tones, the handle or lip be included in one of the vibrating arcs, and in producing the upper tones care must be taken that such be not the case.

132. In glass vessels, or, indeed, in vessels of any material, the extent of a vibrating arc can always be determined by a very simple rule:—Let the vessel contain water, and for any given note count the number of fans produced on the surface, and divide 360 by this number, and the quotient will give the extent of each vibrating arc. Thus, from a glass goblet, suppose a very acute note be produced, and 12 fans be seen upon the surface of the water proceeding from equidistant points of its inner circumference; 360 divided by 12 will give  $30^\circ$ , the extent of each vibrating arc producing a dodeca-nodal note.

*Salisbury, 26th July, 1836.*

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ARTICLE V.

*On Accidental Colours, and Coloured Shadows.*

By PAUL COOPER, ESQ.

THE following remarks upon the paper of your correspondent, Mr. Tomlinson, published in your Numbers for September and October, are made chiefly with a view to promote the discussion of the interesting subjects to which he has directed our attention. The investigation of opinions thus introduced, appears to me, the likeliest method of arriving at truth; and, as this is the common object, when the discussion is conducted with the good feeling which such an object is calculated to inspire, it must be indifferent to the parties on which side it is found.

The point at issue seems to be, whether light produces a continued sensation of uniform intensity; or, whether it gradually declines after the first impression, so as to render

the eye less sensible to its influence, when long exposed to its action: for, if the latter be the case, the various phenomena of accidental colours are necessary consequences; and, independently of the objection to the introduction of a new principle, which has not been traced to any other purpose, it would be contrary to the rules of philosophy to admit two unconnected causes to account for one effect.

Mr. Tomlinson objects to a comparison between the functions of the eye and the palate, because the senses are fitted for such opposite uses; (7) but, when the analogy is so general, and so complete, as in the present case, though it may not furnish conclusive evidence, it is entitled to some consideration.—Every person must have observed, that the most nauseous smell in a short time becomes less offensive; and, that those who live in situations, which expose them to nuisances of this kind, soon become quite insensible to them. The same principle prevails with scents of a more agreeable character; we are frequently delighted with the fragrance of a flower, but if we attempt to prolong the passing gratification, it quickly degenerates, and, if not intermitted, soon becomes insipid.—The sense of feeling is liable to the same gradual decay after the first impression; the points of the fingers, which at first are very susceptible of its influence, soon cease to convey any sensation, unless the contact be intermitted, or otherwise renewed; the blind, whose sense of touch is very delicate, when they endeavour to ascertain the qualities of bodies through this medium, may be observed to keep their fingers in constant motion.—The sense of hearing is also subject to decay; and most persons will recollect instances in which sounds long continued have become almost, if not quite inaudible.—The sense of taste exhibits this principle in a striking manner; and, from some few experiments which I have made on the subject, I think it probable, that the flavour of bodies, when their composition is not too complicated, may be analyzed by means of it, upon the same principle that we analyze compound light, by rendering the palate insensible to the different ingredients of which it consists in succession: thus, strong tea sweetened, so that neither flavour may prevail, will alternately become sweet and astringent, if we render the palate insensible to

the opposite flavour, by means of strong tea without sugar in one case, and sugar without tea in the other, continued for a sufficient time to produce the effect.—Epicures have generally some acquaintance with this branch of philosophy, and they would be enabled to add to their gratifications by studying it further; if we wish to make the most of our pleasures, discretion and moderation must regulate their enjoyment.

It appears, then, that if the sight preserves an uniform state of intensity, it is an exception to a general law; but, that this anomaly has no existence may be proved by the most direct evidence.

I have noticed in a former paper, the very striking experiments of going from a strong to a weak light, with which all of us must be well acquainted; the *total* insensibility of the eye, under these circumstances, proves more than is at present required of us; we are now only called upon to shew, that the sensibility of the eye is *lessened* by the continued action of light of the *same* intensity.

Much confusion has arisen among writers upon the subject of accidental colours, in consequence of their having made no distinction between pure red and the different shades of colour produced by a mixture of red and violet, which approach, more or less, to crimson, and, when diluted with white light, to pink: these mixtures produce accidental colours of the various shades of green, approaching the nearer to blue as the proportion of violet is less in the primary colour; whereas the red of the spectrum, when pure, constantly produces a blue accidental colour.—When red is diluted with white light, it forms the various shades of pale red, some of which are frequently mistaken for orange, and thus furnish another source of confusion.—There is also a want of distinction between the various shades of blue; and, I mention it here, because it is probable, that it led to that part of the theory adopted by Mr. Tomlinson, which makes violet a compound of blue and red.—The blue which forms white light with red is the bright shade of this colour which adjoins the white central light, when the spectrum of the sun is received upon a screen placed at a short distance from the prism; but, besides this blue, which is rather of a light colour, it

is well known, that the different shades of dark blue and indigo, including all the colours formed by a mixture of the blue of the spectrum with various additional portions of violet, are also called blues;\* when, therefore, red is added to any of these colours, it converts the green, with the proper proportion of violet, to white; leaving the superfluous portion of violet, if the red has been added in the proper proportion to neutralize the green, free from any other colour.†—If the red be added in too large a proportion, it gives to the violet a tinge of purple, and, if in too small a quantity, it leaves it a little inclined to blue.

The experiment with which Mr. Tomlinson commences his paper, and which I shall have occasion to repeat to shew the gradually diminishing sensibility of the eye, furnishes an instance of this want of distinction; he says, that if we view a sheet of white paper through a disk of green glass, the sudden removal of the disk from the eye will cause the paper to assume a bright red appearance: Now, upon repeating this experiment, I find the accidental colour is pink, which, as it increases in intensity, approaches to crimson. If the glass be a blue green, the accidental colour will be nearer to a red, in the same proportion that the disk is nearer to a blue.

I make this statement with the less reserve, because, in a subsequent experiment (22) Mr. Tomlinson has himself corrected the error. He says, “I have adopted a very useful mode of testing the true colour of the shadow, by receiving it first upon white paper, and then upon coloured paper; as, for example, boracic acid in alcohol yields a fine light green flame, the shade is *pink* on white ground, violet on blue ground, orange on yellow, &c.”—I could not

\* The accidental colours of these dark blues are various shades of orange, which approach the nearer to yellow as the blue approaches to violet.

† I observe, that some of your pages have been lately occupied in giving a practical account of the art of dyeing; it is a subject to which I some years since devoted considerable attention, both upon a large scale and experimentally, and I am convinced, that there is no theoretical principle of greater importance to the art, from which it removes many anomalous appearances, than the discovery of simple colours by the neutralization of one of the constituents of compound colours, in the manner mentioned here and in my former paper, (vol. ii. p. 113.) In adding ingredients during the process, to produce an exact shade of colour, a knowledge of this principle is absolutely necessary to insure success.

have given an experiment more completely in accordance with my theory; the diminished sensibility of the eye to green light, renders the white ground pink (violet and red diluted with white); the blue ground (violet and green) violet; and the yellow ground (red and green) orange, the eye not being wholly insensible to green light, which would be necessary to produce an accidental colour of pure red. But if blue and yellow are simple colours, from what source is the violet produced on the blue ground; or the red, to form the orange on the yellow ground?

The accidental colour is always the complement of the colour to which the eye has been previously exposed, with reference to the ground upon which it is seen; or, more definitely, the colour of the ground, minus the colour with which the eye is impressed, in the degree to which it has been rendered insensible to this colour.

If we look at a sheet of white paper through a piece of green glass, in the manner Mr. Tomlinson has directed, (1) the paper appears upon the first impression to have a decidedly green tint; but if we continue our view, this tint quickly declines in intensity, until, at length, it becomes gray with only a slight tinge of the colour of the glass.— If we suddenly remove the glass from the eye in the different stages of this experiment, we shall find that the intensity of the accidental colour will be inversely in proportion to the intensity of the primitive colour; if, for instance, we remove the glass after the first impression, the paper will appear white; the following moment it will have a slight tint of pink, and this tint will increase, as the eye becomes more impressed, precisely in proportion to the decline of the primitive colour, until, under favourable circumstances, it becomes crimson.

Here, then, we have a direct proof, that the sensation produced by light declines immediately after the first impression; but we have no reason to apprehend the evils which Mr. Tomlinson supposes must result from it; (7) for the transition from one state to the other is made with such rapidity, that the motion of the eye from one colour to a different, adjoining colour, gives sufficient time for the recovery of its functions; and even the short intermission caused by the motion of the eye lids, which is probably

intended for the purpose, must be conducive to the same important object. If we can prove the existence of the arrangement, we may safely rely for its utility upon the wisdom and benevolence which made it part of the general scheme. I admit, however, that as philosophers it is our duty to trace the arrangement to its object.

Mr. Tomlinson very properly objects to the want of comprehensiveness in the different theories which have been advanced to account for accidental colours; (27) and I fully agree with him, that no theory is entitled to our confidence that does not embrace the whole of the phenomena within its circuit; but there may be a question, whether the fault is not in the application of the theory, rather than in the theory itself; and this is the question which I shall now endeavour to decide.

If the prevailing colour of light disappears in the manner we have been led to infer from the preceding experiment, and which will be still more apparent, if, instead of looking at the white paper, we view the stronger light of the atmosphere through the same, or any other coloured glass, it is evident, that we can have no *fixed* standard for white light; the light, which, under different circumstances, we recognize as white, may be formed of the coloured rays in very different portions.

That this is the case, we have abundant evidence; the light of the sun, and the light of a lamp, or a candle have been proved, by numerous experiments, to be very different in colour, and, of course, to be composed of the coloured rays in different proportions; yet, objects which appear white in one of these lights, are recognized as white in the other, and we only discover the difference by bringing them into contrast.—The method of doing this by suffering the light of the sun, reflected by the atmosphere, and the direct light of a candle, to fall upon different parts of the same white paper, is well known.—I lately observed a pleasing variety of this experiment: while holding a sheet of thin writing paper before a fire, the back of the paper being rather obscurely illuminated by the light from an opposite window, I observed the whole of the writing, which was black and turned towards the fire, appeared of a beautiful light blue. I readily discovered that the ink

had rendered the written part of the paper opaque, while the remainder of it transmitted the red light from the fire; of course, the former reflected the atmospheric light only, and the latter, together with this reflected light, transmitted the direct light from the fire; the eye, turned towards the fire, had recognized its light, particularly with regard to the paper, as the standard for white, and, consequently, the reflected light of the sun produced the impression of blue; as it would have done, if the eye had been prepared by looking through a piece of red glass.—Another variety of the experiment may be made, by admitting the light of the moon, through a small aperture, into an apartment illuminated by a candle or a lamp; the light of the latter being the standard, the former, of course, will appear blue.

If the light of the sun and the light of a candle, when reflected by an object usually considered white, give the impression of white light separately, we may readily suppose, that, when mixed together, they produce the same impression. In the first of the experiments we have just noticed, the white paper is illuminated with both the light of the sun and the light of the candle, and the reflexions from the mixed lights form the standard for white; when, therefore, any part of the paper is seen illuminated with one of these lights only, it appears to be of the prevailing colour of this light compared with the mixed light; thus, the light from the atmosphere appears blue, and the light from the candle red.

The same explanation applies to all other cases of coloured shadows; but to produce the *full* effect, it is necessary, that the intensity of the two lights should be nearly equal, and that they should fall upon the screen in such different directions, that the shadows of any opaque object, formed by the different lights, may be separated.—In the last of the experiments before noticed, the light of the moon was admitted by a small aperture, and, consequently, the light of the apartment, which formed the standard, was not perceptibly affected by it; but, if the two lights had been brought to nearly an equal state of intensity, the mixed light would have formed the standard, and an opaque object, at a proper angle to their different directions, would

have produced a red and a blue shadow, upon the principle already explained.

But we may procure light of different shades, without having recourse to artificial means for its production; the fact is, that the light which is reflected towards us in all directions is never uniform in colour, and it sometimes varies so much, particularly in the morning and the evening, as to produce very distinct shadows; these shadows, when they are only two, are always complementary, because the standard for white is formed by the mixed sensations produced by their joint action.

I have usually slept, during the last six months, in a room with a west aspect overlooking the sea, the window being considerably elevated; and in the morning, I have uniformly observed, that the reflexion of the light of the rising sun, from the atmosphere, and from the water, produced upon the white blind, which performed the part of a screen, two distinct shadows of the horizontal window bars; the upper shadow being a pale blue, and the under shadow a faint red.—In this experiment, the standard for white is formed by the light which enters the window from both reflexions, and, as these reflexions form a large angle, the shadows produced by the window bars diverge rapidly, and fall upon the blind quite distinct; that formed by intercepting the light reflected by the atmosphere being red, and that by intercepting the reflexion from the water blue.

I have frequently observed the same appearance in the shadows formed by window bars, both vertical and horizontal, under other circumstances, particularly, when the light came directly from the atmosphere in one direction, and was reflected from an opposite building in the other. I shall mention only the following instance:—An apartment obscurely lighted by a single window, having a north aspect, receives its light in three different directions; through an avenue of evergreens in a direct line, by reflexion from the atmosphere from the right, and by reflexion from the yellow front of a building nearly opposite on the left; this mixed light produces the usual sensation of white, but the window bars, and all opaque objects within the apartment either form distinct shadows of violet,

yellow, and green, or, when the shadows are not separated, they are fringed with these colours: the green is obscure, but the yellow reflected from the opposite, and the complementary violet reflected by the atmosphere are remarkably distinct.

The following experiment, which will lead to the explanation of another class of these phenomena, will throw further light upon the subject. If we look through the green glass with one eye, as before, and after observing the complementary colour upon the removal of the glass, open the other eye, we shall find, that the paper seen with both eyes approaches nearer to its proper colour, than when seen with the impressed eye only; if we then close the impressed eye, and view the paper with the eye which was previously closed, we shall observe that its colour is a pale green; and by opening and closing the different eyes in succession, it will be discovered, that the alternations produce colours complementary to each other. In this experiment, the standard for white is formed by the mixed sensation produced by the impressed and the unimpressed eye, as may be readily perceived by viewing the paper with both eyes open; and, of course, the paper is seen in complementary colours with reference to this standard.

If we impress the eye, by looking through a crimson instead of a green glass, the effect will be reversed, and the accidental colour will be green. If, in this state of the eye, we hold a narrow strip of white paper vertically, about a foot from the eye, and fix both eyes upon an object at some distance beyond it, the impressed eye will see the paper of a pink colour, while to the other eye it will appear green. If the two images are made to overlap each other, the overlapping parts will appear white. This experiment, which varies only in form from that which precedes it, is similar to Mr. Smith's, as stated in Brewster's *Optics*, page 310, with only this difference, that in Mr. Smith's experiment, one of the eyes is strongly impressed by the light of a candle placed near it, from which the other eye is protected, instead of being impressed by the light transmitted by the crimson glass.

It appears from this investigation, that these phenomena may be divided into three classes; in the first, the eye

being impressed with the light by which it is surrounded, adopts it as the standard for white, and, consequently, sees light varying from it in the proportion of its different rays, of the colour of that ray which predominates, when compared with the standard to which it is referred.—In the second class, the standard is formed by the surrounding light as before; but this light, being composed of light, the different parts of which are differently proportioned and differently directed, forms different shadows; and these shadows must necessarily be complementary, because, when the lights, by which they are respectively formed, are mixed together, they form the light which is referred to as the standard.—In the third class, the standard is formed by the mixed sensation produced by the two eyes under the influence of different impressions, which are, therefore, complementary to each other, with reference to this standard.

In the first class is included, in addition to some of those already mentioned, the experiment of M. Meusnier; (21) the standard in this case, is formed by the light admitted through the curtain, which refuses transmission to light of certain colours; the beam of the sun's light, therefore, which is admitted in its original state by a hole a quarter of an inch in diameter, must appear to an eye thus impressed, of the colour or colours which the curtain refuses to transmit.

The azure colour of the atmosphere is also included in this class. The surrounding light, which forms the standard, is deprived of part of its more refrangible rays, by being reflected from coloured objects in which the less refrangible colours prevail, as well as by the greater depth of atmosphere which it must traverse before it meets the eye, the red light being known to penetrate with greater facility than the other colours; the direct light of the atmosphere, therefore, appears blue when compared with this reflected light, upon the same principle, that the beam of light, in the preceding experiment, appeared green.

The second class includes the experiment, in which the light of the sun and the light of a candle fall upon different parts of the same white paper; and also, the various

coloured shadows, which are complementary to each other, whether formed by light different in colour, in the different directions in which it arrives, or rendered so by the coloured objects that reflect it within the apartment in which the shadows are seen.

The third class is not often produced except by artificial means. The effect in the experiments belonging to this class, to which we have before referred, may be considerably heightened, by looking at the same time, with the different eyes, through two glasses complementary to each other, green and crimson, for instance; the distinguishing colours will quickly be followed by an uniform gray, more or less dark, as the glasses exclude more or less of the coloured rays; but upon removing the glasses, and looking at a white object alternately with the eyes thus differently impressed, the complementary colours will be very distinct: or a still greater effect may be produced, by quickly changing the glasses after the eyes have been sufficiently impressed, when the gray will be converted to its vivid complementary colours; which, however, will soon be lost in the uniform appearance they previously presented. Nothing can more clearly prove the gradually lessened sensibility of the eyes, when exposed to any prevailing colour, than these experiments.

I shall now make a few remarks upon some other subjects adverted to in Mr. Tomlinson's paper; but having already very far exceeded the limits I had prescribed to myself, I shall confine them to such points as are connected with the explanation of my own views.

Mr. Tomlinson, in his first experiment, states, that if the green disk be instantly re-placed, after looking at its accidental colour upon a sheet of white paper, the eye will be unable to distinguish any thing for a second or two previously to the re-appearance of the green; and he accounts for this, by supposing that the super-position of the three primitive colours produce black.

I have never been able to observe this interval of darkness, although I have been led to expect it upon theoretical principles. When the eye is impressed with green, so as to be rendered insensible to this colour, and then turns to a white surface, it sees only the colours reflected by it,

which are complementary to green, viz., violet and red; the crimson image, then, formed by these colours, is as much an original or primary image, as the image formed by the green light transmitted by the glass, and, therefore, if this image be of sufficient intensity, it ought to produce in the eye a lessened sensibility to the colours by which it is formed; but the eye was previously insensible to the complementary colour, green, and, consequently, when the glass is instantly re-placed, the eye, under the influence of both impressions, insensible to the three colours, or to the whole of the light reflected by the white paper.

This brings us to an explanation of the experiment of M. Plateau; [6]\* which is introduced by the rather paradoxical statement, that while two real complementary colours produce, together, *white*, two accidental complementary colours produce, together, *black*.—In this experiment the eyes, by viewing the complementary colours in succession in the manner he has directed, become insensible to both these colours, which, together, constitute white light, when it is presented to them in the weak state to which it must be reduced in penetrating to the eyes closed and covered; and it produces a black accidental image, precisely as it would have done if the colours had been viewed together upon a white surface. The complementary colours are seen on the right and left of the black image, because these parts of the eyes have been exposed during the progress of the experiment to one colour only.

I have discovered only one difficulty in the theory which I have been endeavouring to support, and that difficulty is found in the present experiment. When the light is excluded from the eyes in the manner described by M. Plateau, it appears difficult to conceive, that it can be present to form the accidental colours which he afterwards observed; but the following experiment will at least render it probable, that by some means, which we cannot explain, it still penetrates to the eyes, though in a state of very low intensity. If, after looking at a coloured surface, we close the eyes, we see the complementary colour of the previous impression: if we then pass the hand before the eyes, so as to intercept the light which falls upon the eye lids, the

\* Records of General Science, vol. ii. p. 232.

colour disappears while the hand is before the eyes, and re-appears when it has passed beyond them, and this may be repeated several times; but if we suffer the hand, or any other object which intercepts the light, to remain for some time before the eyes, the accidental colours will re-appear after a short interval, though much lessened in brilliancy and distinctness.

Now, with such evidence before us, that in every other instance the accidental image is dependent upon the light which has access to the eyes, and that in many cases it is modified by the character of this light; and this evidence being brought up to the very point at which the difficulty occurs, it would, in my opinion, be contrary to the rules of sound philosophy to introduce any other cause to account for it; particularly in our present state of uncertainty with regard to the ultimate destination of the light which disappears when it is intercepted by opaque bodies.

M. Plateau's theory appears to be founded entirely upon this single fact. [2] The hypothesis by which he endeavours to explain it, that the retina spontaneously assumes an opposite state after the cessation of direct impressions, [3] has no other support; and the numerous experiments in which the accidental image is modified in colour by the colour of the surface upon which it is formed are so decidedly opposed to it, that it appears to me to be quite untenable.

He states, "that the accidental colours of impressions destroy direct corresponding impressions;" [5] and in the experiment which is brought forward to support it, concludes that the direct impression of a small piece of red paper, upon a black ground, is destroyed when the eye is directed to a larger piece of paper of the same colour, by the accidental impressions, *green*. According to our theory, no accidental green image can be formed upon a surface where the only colour present is red: but the part of the eye impressed with the small piece of paper of the same colour, being in a great measure insensible to red light, the image of the paper will appear somewhat black, when seen in contrast with the surrounding paper of the same

colour, which falls upon a part of the eye, not previously brought into action.\*

Mr. Tomlinson, in order to establish his hypothesis that the superposition of complementary colours produce black, appears to have fallen into a similar mistake. He says, "if we view red lead through a disk of green glass, the red powder will appear as black as lamp black."(28) But in this experiment, the only colour reflected by the powder is red, and this colour the green glass refuses to transmit; so, that instead of a superposition of complementary colours, it is the absence of light of all colours which produces black.\*

If accidental colours are produced by rays reflected by the surface upon which they are seen, the images formed by these colours are to all intents and purposes as completely primary as the images which arise from the colours that produce the first impressions; and, as we have already observed, if they are of sufficient intensity, and their action upon the eye be continued for a sufficient time, they ought to produce accidental colours complementary to the colours of the images by which they are formed, and, therefore, corresponding with the original impressions.

This view of the subject is illustrated and supported by the experiments of Newton, Aepinus, Brewster, and others; in which they impressed the eye with the image of the sun, and then, by directing it to a white ground, produced an accidental spectrum, which was invariably surrounded with a border, the colour of which was generally complementary to the centre.†

I have observed in a former paper, that when we look at any object with a view to impress its image upon the retina, with whatever determination it may be opposed, there is an involuntary motion of the eye, apparently of vibratory character, which brings a part of the retina surrounding the image into the alternate directions in which it sees the

\* These explanations must be modified, if we take into consideration, the white light which usually accompanies the prevailing colour reflected by coloured surfaces; but this would not effect the general principle.

† It may be remarked, that these complementary colours, as observed by Sir David Brewster, and given in his *Optics*, p. 308, exactly correspond with our theory of primitive colours.

edge of the object and a narrow border of the ground upon which it is placed, in succession. These alternations in the direction of the eye produce the accidental colour which appears to play about the edge of the object, and, also, by the intermissions which they occasion, preserve the colour of this part of it from being impaired. If, for instance, the eye be directed to a red seal upon white paper, after a little time there will be observed a faint blue border surrounding the seal, which will increase in intensity as the colour of the seal declines; and the edge of the seal to a depth nearly corresponding with the breadth of the blue border, will be found to preserve its original strength of colour.

When the eye is impressed with any moderately illuminated object, such as the seal in the last experiment, the complementary colour surrounding it, which is seen upon the white ground, is not of sufficient intensity to produce a corresponding accidental colour, and the image formed by directing the eye to a different part of the ground is not surrounded with any border; but when the eye is impressed with the strong light of the sun, the accidental colour, formed at the same time upon the edges of its image by the white surrounding atmosphere, corresponds in intensity with the original impression, and the accidental colours of both are seen together.

There is an interesting paper on Coloured Shadows, by Count Rumford, published in the *Philosophical Transactions* for 1794, and also in the 1st volume, quarto series, of *Nicholson's Journal*; and a memoir on the same subject, by Citizen Hassenfratz, published in the *Journal de L'Ecole Polytechnique*, tom. iv.; and a translation of it in the 6th and 7th volumes, octavo series, of *Nicholson's Journal*.

This paper was written when I had no opportunity of consulting these works; but, upon since referring to them, I do not find it necessary to make any alteration.

In one of Count Rumford's experiments, he prepared two Argand lamps, and by intercepting their light at a proper angle, by means of a flat ruler, he procured two broad shadows which were projected upon white paper and found to be perfectly colourless. He then directed a tube about twelve inches long, and an inch in diameter, lined

with black paper, against the centre of one of the broad shadows, while an assistant repeatedly interposed a sheet of yellow glass before the lamp whose light corresponded to the shadow he was looking at through the tube; when, "so far from being able to observe any change in the shadow upon which his eye was fixed, he was not able even to tell when the yellow glass was before the lamp, and when it was not; and though the assistant, often exclaimed at the striking brilliancy and beauty of the blue colour of the very shadow he was observing, he could not discover in it, the least appearance of any colour at all. But, as soon as he removed his eye from the tube, and contemplated the shadow with all its neighbouring accompaniments, the other shadow rendered really yellow by the effect of the yellow glass, and the white paper, which had likewise from the same cause acquired a yellowish hue, the shadow in question appeared to him, as it did to his assistant, of a beautiful blue colour." The standard to which both shadows are referred in this experiment, is the mixed light from both lamps, one coloured and the other white.

M. Hassenfratz discovered by numerous experiments, that the light reflected by the atmosphere, and the direct light of the sun is always different in colour; and that the shadows produced by the separation of these two kinds of light, vary with the state of the atmosphere, the latitude of the place, the season of the year, and the hour of the day. He noticed that in coloured rooms, "when several lights direct or reflected concur with the light of the atmosphere to enlighten the plane on which the shadow is observed," the number of shadows is almost always two or three, sometimes four or five, and it has even happened, that six were distinguished.

There are some interesting experiments in this memoir, on the colours of the shadows produced by the separation of two lights procured from different sources, when compared with each other; from which it appears, that the light reflected by the atmosphere is blue, when compared with all artificial lights; and that the light from hydrogen or other combustibles in which it abounds, is blue, compared with those in which there is a larger proportion of carbon. In all these experiments, the shadows are com-

plementary, generally different shades of blue and red, and in every instance, strictly in accordance with our theory, as it relates to the primary colours.

Mr. T. Smith, in a note added to the paper in which the experiment we have already noticed is described, (London and Edinburgh Philosophical Magazine, vol. i. p. 255.) has given some experiments with coloured tubes, which nearly correspond with those of Mr. Tomlinson; these experiments shew the facility with which the eye may be impressed, of which the proofs are very numerous. One of the most delicate tests of this with which I am acquainted, is produced by looking for an instant at a white surface through a coloured glass with one eye only, then at the same surface, but without the glass, with both eyes, and afterwards with the eyes alternately; when the complementary colours will be seen, in a slight degree, although they arise from an impression made almost instantaneously. Perhaps, an improvement upon this might be made, by looking with both eyes, after the impression has been made upon one of them, at a strip of white paper held at the distance of about a foot, the eyes being directed to a distant object; when the overlapping parts of the complementary colours would exhibit the standard, while one of these colours would be seen at the same time on each side of it.

I have confined myself in this paper to the generalization of known facts: at some future time, it is my intention to proceed a step farther, by shewing the connexion of these facts with others which have been traced to general principles; but at present, this paper is too long to admit of the extension which it would require, and there will be less danger of its being considered hypothetical, when my theory of light has been more fully explained. It would be found, however, that light *whatever may be the circumstances attending it*, is transmitted upon the most general and invariable principles.\*

P. COOPER.

Bowlsh, November 7th, 1836.

(To the Editor of the Records of General Science.)

\* See Abstract, paragraph 23.

## ARTICLE VI.

*Notice of some Recent Improvements in Science.*

## ELECTRICITY.

*Electricity by Contact.*—M. Karsten, in a letter of 150 pages addressed to Humboldt, has discussed this subject. The paper has excited much attention in Germany, but it contains few new facts, and many speculations. His conclusions are, 1st. The metals, and perhaps, all solid bodies become positive when plunged into a liquid; the latter becomes negative. 2nd. When the body is not wholly immersed, its two ends take opposite electrical states. 3rd. Solid bodies differ much in regard to their electro-motive force with the same liquid, and it is in this difference that the electrical activity both chemical and magnetic of the pile depends. 4th. When two electromotors of different forces are plunged into the same liquid without being in contact, the feeblest takes the opposite electricity to that of the strongest, and becomes consequently negative. 5th. The feeblest part of the solid electromotor, placed out of the liquid, possesses equally the opposite electricity to that of the part immersed. 6th. The electromotive energy of a liquid depends on its power of acquiring by the presence of two dissimilar solid electromotors, a state, by virtue of which, it yields more or less readily electricities opposite to the same electromotors. All bad conducting liquids, possess in general this property, but it belongs neither to perfectly isolating liquids, (such as oils, &c.,) nor to very good conducting liquids, (such as mercury and liquid metals.) The electro-motive energy of a liquid, does not, however, depend only on its conducting power, but also apparently on several other conditions not yet properly known. 7th. The electro-motive effects of two metals forming with a liquid, a closed circuit, result from the disengagement and re-combination of opposite electricities in the liquid. These effects are excited by the electro-motive relation of the two unequal electromotors to the liquid; they are favoured by the electro-motive relation of the strongest electromotor to the weakest, and are accelerated by the immediate contact of the two electromotors, accord-

ing as they are good conductors of electricity. 8th. The chemical changes in the pile are certainly allied to the recombination by means of the solid bodies in the pile of the electricities set free, but there does not exist between these phenomena any dependence as cause and effect. 9th. In a re-union of the elementary piles (pile of Volta) the opposite electricities are completely neutralized, by the solid bodies of each element, and there is no transmission of electricity from one element to the other.\*

2. *Action of nitric acid on the oxidizable metals.*—This curious subject has been well illustrated by the experiments of Herschel and Schönbein, as well as by those of Keir, Wetzlar, Fischer, Fechner, and Faraday. The deduction from their researches is, that iron wire is often exhibited under certain circumstances, as a metal easily acted on by acids, and that sometimes on the contrary, in consequence of slight modifications of the experiment, it resists oxidation with the greatest obstinacy. Schönbein accounts for this curious fact, by supposing, that a peculiar action is induced by which the natural affinity of the metal is altered, while Mr. Faraday ascribes the cessation of action to the presence of a thin layer of oxide, which, however, is not perceptible to the eye. M. Mousson not satisfied with these explanations, considers the following more satisfactory: 1. It is not necessary in order to explain the phenomena of the action and passiveness of iron to have recourse to a new hypothesis. 2. That the phenomena in different metals only differ in degree, not in the nature of the action. 3. That they depend essentially on the incapacity of concentrated nitrous acid to attack several metals, (perhaps even any) and of the double mode of decomposition of which it is susceptible. 4. That the commencement of the state of passiveness is always accompanied with an oxidation and corresponding current. 5. That the same current, according to its action on the acids, according as it favours or prevents the formation, and close contact of a layer of nitrous acid produces electro-chemical changes which the metals present in the same nitric acid.†

3. *Negative more readily dissipated in the air than positive electricity.*—Professor Belli has made several interesting

\* *Bibliothèque Universelle*, September, 1836. p. 165.

† *Ib.* 154.

experiments which go to prove this position. Having placed an electrometer upon an isolated horizontal conductor, he found by a mean of 3 trials, that after having electrified the conductor with positive electricity, the electrometer took 10 minutes and 2 seconds to descend from  $20^{\circ}$  to  $10^{\circ}$ . With negative electricity, the electrometer only required 4 minutes and 30 seconds to pass over the same number of degrees, under precisely the same circumstances. He infers, that the force generally less which negative electricity furnished by a machine possesses, compared to that of the positive electricity which the same machine furnishes, does not depend alone on the less advantageous disposition of the conductors destined to collect the first electricity, but also on the more ready loss which it sustains.\*

*Electric spark obtained from the Torpedo.*—The attempts which Dr. Davy made to obtain a spark from the torpedo were not attended with success (*Records*, vol. i. 306.) M. Matteucci has, however, been fortunately successful in his trials. He employed for this purpose an apparatus perfectly similar to that employed by Mr. Faraday for obtaining a spark by means of a single voltaic pair. The principle object of this arrangement is to prevent the electricities from being neutralized directly by the medium of the conducting body which develops them, and of forcing them to unite exteriorly across the thin layer of air where the spark is produced. Matteucci has also succeeded in magnetizing steel needles in the same manner.\* In a letter from Matteucci, read by Donne before the Institute, we learn that an electrical discharge may be obtained from the torpedo, although the skin of the organ be removed, and, even when portions of the substance of the electrical apparatus have been cut away. When the torpedo does not discharge electricity, it is impossible to obtain, even in the interior of the organ, the least trace of electricity in any point whatever, either with the galvanometer or condenser. The intensity of the shock is reduced by diminishing the number of nervous filaments which go to the organ. In the act of discharging, the electrical current always passes from the back to the belly. Three grains of muriate of morphin introduced

\* Bibliothéque Universelle, June, 1836.

into the stomach of a torpedo killed it in ten minutes; its death is accompanied with stronger shocks than usual, and with convulsions. When the torpedo has ceased to give electrical shocks, even when irritated, if we lay bare the brain, and touch gently the posterior lobe of the brain which gives nerves to the electrical organ, stronger shocks than usual (3 or 4) are produced, but having the same direction from the back to the belly. If, in place of simply touching the surface of the brain, it is wounded without taking the same precaution, then very strong shocks are renewed, but without having the same constancy in the direction of the current. These facts, and especially the latter, are sufficient to shew, Matteucci thinks, that the electricity of the torpedo is not produced in the organs situated on each side of the animal; that the current receives its direction from the brain, and that in the electrical apparatus, the electricity is only condensed as in the Leyden jar.

*Development of Electricity.*—M. De La Rive has published some very elaborate memoirs, in which the question is discussed at great length. How is voltaic electricity developed? He has drawn the following conclusions:—

1. That in his memoirs he has endeavoured to corroborate by new facts the deductions which he had formerly drawn relative to the necessity of a chemical action for the production of voltaic electricity, and the impossibility of developing electricity by simple contact.
2. The attention which he has paid to the effects of current, and the dynamical effects have led him to observe, that the quantity of electricity accumulated at the poles under the form of tension is greater in proportion as the two electrical principles have less facility in uniting through the pile itself, and as the pile contains a greater number of pairs. In the same manner, it is necessary for the dynamical effects, that the pile be little of a conductor, and contain, consequently, a sufficient number of pairs, in order that the two electrical principles may re-unite in greater proportion, by the medium of conductors placed between the poles than through the pile itself.
3. Having found that the quantity of free electricity disengaged in a given time from each pair of plates exercises no sensible influence on the tension of the poles of a pile, since this kind of effect is not instantaneous,

while it exercises a very great influence upon the intensity of the dynamical effects, and so much the stronger in proportion to the goodness of the conductors; he has deduced some practical consequences applicable to a more advantageous construction of a voltaic pile in each particular case. 4. The examination which he has made of the influence of metallic diaphragms, placed either on the track of a current between the poles of a pile, or in the interior of a pile itself, has shewn that this influence is very different according to the nature of the conductors placed between the poles, and may be explained by a greater or less alteration in the conductibility of the homogeneous conductors in which the diaphragms are placed. 5. In endeavouring to appreciate in all its extent, the influence of a number of pairs, De La Rive has observed, that the number sometimes increases, sometimes diminishes the intensity of the effects of a pile, and that these variations depend on several circumstances belonging, some to the pile, others to the nature of the conductors interposed between the piles. He infers from the study of these circumstances, that the phenomena to which they give birth are a consequence of the chemical theory of the pile.\*

## CHEMISTRY.

*Composition of atmospheric air.*—M. T. de Saussure has taken advantage of the property which small shot possesses of absorbing oxygen when moistened and agitated with atmospheric air at common temperatures, to analyze common air. He employed a matrass possessing a capacity of from 150 to 250 centi-metres cubes closed hermetically with a metallic stopper, which is fastened with screws to a firm socket at the end of the neck of the matrass. The lead shot ought to contain 80 to 100 shots to the gramme, (15.438 grs.) Its weight is nearly the fifth of that of the water which the matrass contains. The water for moistening the shot should be equal to the seventeenth of its weight. A larger or smaller quantity retards the oxidation. Three hours of constant agitation are sufficient to deprive the air of its oxygen. Saussure measures the quantity of oxygen absorbed by the weight of the water which

\* Ann. de Chim. lxii. 206.

the atmospheric pressure causes to enter the flask. He affirms, that the lead takes up all the oxygen from the air which undergoes a diminution of volume of 21.05; but as lead absorbs carbonic acid also, amounting, in his experiments, to .0004; it follows, that the quantity of oxygen in the atmosphere is 21.01 per cent. Gay Lussac has suggested another eudiometer, viz., a plate of copper moistened with sulphuric, muriatic or acetic acids.\*

*Molecular composition of bodies.*—Persoz, in a paper read before the Institute, has come to the following conclusions on this subject, 1. Chemical atoms are only molecular groups, the relative value of which is expressed by the atomic weight. 2. These groups are all divisible by a constant number (70), which expresses the volume of vapour, that the atomic weight of a body furnishes. 3. If we multiply this number (70) by a factor corresponding to the volumes of a molecular group, and take the product as the divisor of the relative weight of this group, the quotient will be the weight of a litre of the vapour, by means of which we may deduce at pleasure (taking into account only the molecular change which supervenes in its passage from the solid to the gaseous state) the density of a body compared to that of air or water. 4. The density being intimately connected with the relative weight of the atoms, we can, by means of the specific gravity of a body, verify the analytical results obtained by chemists. 5. By a knowledge of the density, we may establish the molecular composition of bodies, and distinguish that of certain gases with the composition of the molecular groups which produces them. This would lead to the knowledge of the laws of the dilatation of gases.

We are glad to find, that important views like these, which, in many respects, are just those advocated in the pages of this Journal, and previously in the "First Principles," are beginning to attract attention in France.†

*Solubility of some Carbonates, &c. in Sal Ammoniac.*—According to Vogel, carbonate of lime, when newly precipitated, Iceland spar, white marble, carbonates of barytes and strontian, precipitated from these solutions, and also witherite are all soluble in sal ammoniac. Hence, a sub-

\* Ann. de Chim. lxii. 219.

† L'Institut, 181.

stance must not be considered carbonate of magnesia as is usually done, because it is soluble in this salt. The solution of these earthy salts may perhaps be attributed to their decomposition, by the formation of carbonate of ammonia and earthy muriates.\*

*Action of Anhydrous Sulphuric Acid on Metallic Chlorides.*—When the vapour of this acid, either with or without water, is brought in contact with common salt at an elevated temperature, the sodium is oxidized by the sulphuric acid, while chlorine with sulphurous acid gas is disengaged. The result, according to Rose, is different when the chloride is finely pulverized, and placed in a vessel surrounded with a freezing mixture. The vapour of the acid is then rapidly absorbed without decomposing it. The whole is transformed into a transparent mass, which is a compound of chloride of sodium and anhydrous sulphuric acid. When heated, it decomposes, being resolved into sulphate of soda, while chlorine and sulphurous acid are disengaged. Chloride of potassium, muriate of ammonia act in the same way; but the chloride of copper and barium in the anhydrous state do not. The same acid unites with the nitrate and sulphate of potash, and with sulphate of ammonia, all of them anhydrous salts.†

*Decomposition of Copper Salts by Phosphorus.*—According to Vogel, when a stick of phosphorus is kept in a solution of sulphate of copper, the liquid gradually loses its blue colour and become colourless. In this state, the solution does not contain even a trace of copper. On evaporating the fluid, and heating the residue strongly, sulphuric acid flies off, and phosphoric acid remains. The phosphorus which is covered with a metallic coating of copper becomes partly black in the space of a short time. Below these leaves of copper, there is observed, on the surface of the copper, another thin layer in plates of a black colour without metallic lustre easily detached from the phosphorus and very fragile. It possesses the properties of phosphate of copper. In a solution of nitrate of copper the same phenomena occur. The green solution of chloride of copper becomes at first black when phosphorus is placed in it, and then nearly colourless in a concentrated

\* Journ. für prak Chim. vii. 453.

† Journ. de Chim. Med. Oct., 522.

solution; quite colourless in a dilute solution, the copper being wholly thrown down. A solution of acetate of copper becomes pale coloured, and gradually becomes a whitish powder by the action of phosphorus. This powder is phosphate of copper, which is not re-dissolved by the free acetic acid.\*

*Reduction of Mercurial Salts by Copper.*—If we plunge a plate of polished copper into an aqueous solution of corrosive sublimate, the copper becomes blackish gray, the solution milky, and shortly afterwards a white powder is deposited, which is sub-chloride of mercury, or calomel mixed with some globules of mercury. This was observed by Fischer of Breslaw. But Vogel has since noticed, that the copper plate, instead of presenting a silvery appearance, is covered with a black layer destitute of all metallic lustre. He left a plate of copper for 24 hours in a concentrated solution of corrosive sublimate; the liquid had become green, and a quantity of calomel had been formed which rendered the liquid milky; the greater part was, however, deposited at the bottom of the vessel. The surface of the copper was covered with a very thin black coating, and with small globules of mercury which did not adhere, but fell on the slightest touch. The plate of copper was washed with water and dried. The black layer adhered so strongly to the copper, that it was difficult to remove it by friction with paper. This black matter dissolved in muriatic acid without effervescence, and the liquid decanted from the copper contained deuto-chloride of copper, in which some flocks of calomel were observed to swim. The copper, after digestion in the acid, was white, and acquired the lustre of mercury by friction. Solutions of sublimate in alcohol and ether, act upon copper in the same way. Calomel, though almost insoluble in water, may be decomposed by copper. If, when suspended in water, a plate of copper be immersed in the liquid, copper soon begins to be dissolved, and the plate becomes black. The decomposition of calomel is still more rapid, if the water in which it is suspended be kept in a boiling state. The copper is soon covered with a black coating, beneath which there is a metallic layer.

\* Pharm. Central blatt, Oct., 1856, p. 628.

It, therefore, appears that the action of copper upon the chlorides is different from that upon the nitrates of mercury. In the latter case, the mercury is reduced and deposited on the copper plate.\*

*Solubility of Oxide of Lead in Water.*—According to Bonsdorff, the oxide of lead when prepared either by the wet way, viz., the action of water containing air upon metallic lead, or by the dry way, from nitrate of lead, is completely soluble in water. One part of lead requires 7000 of water for solution, which is not so inconsiderable when we remember that 1 part of magnesia requires above 5000 parts of water to dissolve it. The solution of oxide of lead in water possesses a strong alkaline re-action, both on fernambuc and violets, and is an excellent test for carbonic acid.†

*New compounds of Platinum.*—The potash cyanuret of platinum of Gmelin, obtained by mixing together solutions of chloride of platinum and ferrocyanodide of potassium, evaporating and procuring long fine rhomboidal prisms, which are sometimes yellow and sometimes blue, according as they are viewed, and consisting of  $K\ Cy + Pt\ Cy + 3\ Aq$ , forms with protonitrate of mercury a beautiful small blue precipitate. This powder may be washed with cold water, acidulated with nitric acid, and then dried without altering its colour, but if boiled with water it becomes quite *white*. If a solution of protonitrate of mercury be now poured on this bleached precipitate, and be allowed to remain in contact with it for several hours at the usual temperature, it becomes as beautiful as before. When heated on a platinum plate the coloured precipitate detonates, throwing out sparks and smoke. It dissolves in muriatic acid when heated, giving out nitrous and prussic acids. The colourless precipitate burns when heated without detonating, and leaves about 38 per cent. of spongy platinum. It dissolves in muriatic acid without disengagement of gas. The solution is precipitated by potash, and leaves a residue which decomposes into prussic acid, chloride of mercury, and cyanodide of platinum. When the white precipitate is heated in a small glass retort, it is resolved into cyanogen, running mercury, and cyanodide of platinum.

\* Pharm. Central blatt, Oct. 1836, p. 629.

† Ib. Aug. 1836, p. 520.

It consists of 48 cyanodide of platinum and 52 of cyanodide of mercury, or Pt Cy + Hg Cy. *The Cyanodide of Platinum* left by the decomposition of the hydrargyro cyanodide of platinum, is a beautiful olive coloured powder, insoluble in water, acids and alkalies; combustible, leaving by combustion 79 per cent. of pure platinum; giving with oxide of copper and heat, carbonic acid and azote, in the proportion of 2 to 1 volumes, and consequently composed of Pt Cy. If the hydrargyro-cyanuret of platinum diffused in water be treated with sulphuretted hydrogen, sulphuret of mercury is produced, and a colourless strongly acid liquid, which contains in solution a combination of cyanodide of platinum with hydrocyanic acid. If the water is driven off by evaporation, this new combination appears in the form of a greenish yellow substance, with the metallic lustre, presenting on its surface the colour of gold and copper, which deliquesces in the air; is very soluble in water and absolute alcohol, and combines with the alkalies to form the double cyanurets of platinum. Doberëiner, who is the discoverer of these facts, terms this substance *hydroplatinocyanic acid*. It consists of Pt Hg Cy<sup>2</sup>. When dissolved in absolute alcohol, and allowed to evaporate, peculiar crystals are observed, with a fine play of colours similar to the chameleon. If the dry acid is allowed to deliquesce in a moist atmosphere, and then allowed to evaporate in dry air or in the solar light, extremely beautiful crystals are formed, grouped in the form of stars, with the metallic lustre, of sometimes a golden and sometimes a copper colour. This acid undergoes no change below 212°; above this it is decomposed into prussic acid and cyanodide of platinum. If its solution in alcohol be mixed with a little nitric acid, a liquid is produced, which when evaporated on a plate of glass and heated strongly, forms a very beautiful platinum mirror. There is also a *hydriridiodicyanic acid* possessing similar properties.\*

*Analysis of iron ores.*—Berzelius states the following to be a rapid mode of analyzing these ores. He boils them with chloride of copper slightly acidulated with muriatic acid, then on boiling the residue with carbonate of soda, washing the result, drying and weighing, its weight indicates that of the carbon.†

\* Poggendorff's Ann. xxxvii. 545.

† L'Institut, 170.

*Acid Salts.*—In a memoir by Mitscherlich, it is observed that soda and potash unite in two proportions with sulphuric acid in order to form acid salts, which may be considered as compounds of hydrous sulphuric acid and neutral salts, as was suggested by Mr. Graham. In well formed crystals we have acid sulphates of potash and soda, in which the sulphuric acid in a hydrous state is in the same proportion as the sulphuric acid in the neutral salts; then an acid sulphate of soda  $\text{NO}, \text{SO}^3 + \frac{1}{3} \text{HO}, \text{SO}^3$  and lastly a sulphate of potash, in which the hydrous acid is only the 4th of the sulphuric acid of the neutral salts. Ammonia unites with sulphuric acid ( $\text{NH}^3 \text{HO}, \text{SO}^3 + \frac{1}{3} \text{HO}, \text{SO}^3$ ) as potash does with manganic acid, ( $\text{KO}, \text{MnO}^3 + \frac{1}{3} \text{HO}, \text{MnO}^3$ ) but only in a proportion to form an acid salt. The acid salts of chromic acid, are on the contrary, only compounds of chromic acid and a base; the acid chromate of potash contains in the same quantity of base twice as much as another acid salt, which is obtained by dissolving the first acid salt in nitric acid, and collecting the crystals precipitated in the solution. The acid seleniate of potash ( $\text{KO Se O}^3 + \text{HO}, \text{Se O}^3$ ) has the same form as the corresponding acid sulphate. The acid sulphate of potash has the same form as sulphur; after being fused it assumes a form similar to that assumed by melted sulphur. The acid manganate of potash and sulphate of ammonia have the same form and composition.\*

*Decomposition of Iodide of Mercury by Light.*—The iodide of mercury, when dried in the light of the sun, becomes dark olive coloured. According to Artus, there is no free iodine separated; but hydriodic acid is given out, so that the combination, formed by the light, consists of protoxide of mercury and iodide. Water must, therefore, be necessarily present. Iodide of mercury dried in the water bath is, however, almost indifferent to light.†

*Preparation of Antimony free from Arsenic.*—According to Artus, this may be effected as follows: mix one part of finely powdered crude antimony with two parts of common salt. Digest the mixture in a retort with five parts concentrated sulphuric acid and two parts water for six or eight hours; boil it then for one hour; dilute the solution with an equal volume of rectified spirit of wine, or with as

\* L'Institut, 170.

† Journ. für prakt Chemie, viii. 63.

much water as it can take up without decomposition; allow it to stand at rest for some time; filter; precipitate the filtered liquor with water; filter again; wash and edulcorate the filtered basic chloride of antimony; press it gently, and dry it at a gentle heat. This basic chloride is, when properly prepared, completely free from arsenic, and it is only necessary to heat 100 parts of it with 80 parts of carbonate of soda and 20 parts of carbon powder for a quarter of an hour, to obtain  $61\frac{1}{2}$  parts of antimony completely free from arsenic.\*

*Separation of Basic Phosphate of Lime from the Ammonia Phosphate of Magnesia by Acetic Acid.*—This method has been proposed in consequence of the insolubility of the former, and the solubility of the latter salt in acetic acid. Du Menil found, that acetic acid of the specific gravity 1.04, when digested for four hours with one-sixth of its weight of phosphate of lime acquired no trace of lime, when the salt was heated; only a very slight trace, when it was dried at  $212^{\circ}$ ; but a great quantity, when precipitated fresh, and acted on when moist. Ammonia phosphate of magnesia, dried at  $212^{\circ}$ , dissolves rapidly in six times its weight of acetic acid. When the solution was heated, a portion of the salt separated in the form of a crystalline crust. A similar effect was produced by doubling the quantity of acid.†

*Adulteration of Succinic Acid with Succinate of Lime.*—Schwenke has procured as much as 15 per cent. of lime from a specimen of succinic acid.‡

*Cochineal of Ararat.*—In that part of Armenia which is now incorporated with the Russian empire, in the province of Erivan and in the vallies of the Araxes, a species of cochineal insect is found, which, according to M. Hamel, appears to be unknown to naturalists. It is met with principally in the villages of Schorly, Sarwanlar, Nedschely, Hassan Abad, &c. M. Hamel, by giving a view of the different authorities who have mentioned it, shews that it enjoyed an important rank in commerce until the period when the American cochineal shut it out of the market. It is very distinct from the cochineal of Poland. A pound

\* Pharm. Central blatt, Oct. 1836, 638.

† Arch. der Pharm. vi. 73. ‡ Pharm. Central blatt, August, 1836, 558.

of Armenian cochineal contains only from 18 to 23 thousand insects, while that of Mexico contains 20 to 25 thousand, and that of Poland 100 to 130 thousand. It contains also more colouring matter in an equal weight than the Polish. It is found abundantly on the roots of the *Ærolupus laevis*, (*Trinius*), a plant which grows abundantly in Erivan. Brandt proposes to call it *Porphyrophora Hamelii*.\*

*Inferiority of English to China Ink.*—The directors of the Bengal bank lately refused payment for a number of bank notes, in consequence of their containing no signature. It appeared that they belonged to a Hindoo, who had kept them in a copper box. He asserted that they originally possessed the signatures of the director, comptroller, cashier, &c., but that they had been effaced. The notes on which the signatures had been written with China ink remained uneffaced, but all the writing with English ink had completely disappeared. Mr. Princep, in order to determine the question, placed a paper covered with writing in English ink between two plates of copper. After a short space of time he found that the copper had decomposed the ink, and that the writing was completely effaced. He concluded that the account of the Hindoo was correct, and that the bank ought not to refuse payment.†

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#### ARTICLE VII.

*Determination of the Obliquity of the Ecliptic at Edinburgh.*

By W. GALBRAITH, A.M.

(To Dr. R. D. Thomson.)

*Edinburgh, 11th November, 1836.*

Dear Sir,

As a continuation of my former paper on astronomical observations, I hereby send you the results of my recent determination of the obliquity of the ecliptic in June, 1836, with a new circle somewhat improved. It has three verniers, each showing 10", the scale of the level is divided so as to indicate 3" and a third, or at base, a half of each division may be readily estimated. The diameter of the

\* L'Institut, 183, 374.

† Asiat. Society Journal, and L'Institut, 182, 368.

circle is like that used in the former observations, six inches, having a telescope provided with eye glasses possessing magnifying powers of 20 or 30. The circle seems, however, to have a small bias or irregularity on inverting the telescope after reversing the circle, amounting, at a maximum, to about 5", and proportional to the sine of the zenith distance, which has been allowed for. Further observations will enable me to investigate this more exactly.

1836. Mean obliquity for January 1st, from observations made,

	°	'	"
June 14th . . . . .	23	27	29.7
„ 15 . . . . .		27	35.3
„ 16 . . . . .		27	31.3
„ 17 . . . . .		27	32.4
„ 20 . . . . .		27	40.2
„ 21 . . . . .		27	46.8
„ 23 . . . . .		27	50.2
„ 24 . . . . .		27	54.6
„ 25 . . . . .		27	32.6
„ 28 . . . . .		27	36.1
<hr/>			
Mean of the whole of these . . . . .	23	27	38.9
Bessel gives . . . . .	23	27	38.4
My general tables give . . . . .	23	27	39.4

Though there are considerable discrepancies in the preceding observations, yet they are not greater than might be expected, from the size of the instrument and power of the telescope, especially in such unfavourable weather as we had last June. They were reduced by the formulæ given in my last paper, in which there are one or two errors of copying, and some typographical, but these commonly do not affect the accuracy of general formulæ or final results. I was able to get only two or three rather unsatisfactory observations on the late equinox, which I consequently think scarcely worth transmitting you.

I am, dear Sir,

Yours sincerely,

WILLIAM GALBRAITH.

## ARTICLE VIII.

*The Art of Dyeing.**(Continued from page 375.)*

WHEN calico printed with iron and copper mordant is dipped in a solution of 1 lb. ferro-prussiate of potash and 40 lbs. water, it acquires a dark brown colour, from the production of ferro-prussiate of copper.

Except in the case of logwood colours there appears to be no advantageous action from this relation of cotton mordanted with iron mordant to copper mordant. Its influence is rather deteriorating. Thus a cochineal blueish gray, with iron alum is not obtained, but a violet colour, when the calico is treated with copper mordant in the manner described, before dyeing with the cochineal. When lighter grounds which have iron for their basis, after dyeing are immersed in copper mordant, No. 2, various shades of colour are obtained of greater permanence, although with most of the dark colours of compounds containing iron, the changes are not so remarkable as in the colours with alum mordant.

ON THE ACTION OF DIFFERENT ADJUNCTS IN DYEING WITH  
MADDER, QUERCITRON, AND LOGWOOD.

1. *Madder with starch, flour and bran.*—When the alum mordant to be printed is thickened with starch, the madder red produced by it comes out clearer than when the thickening medium is gum. This fact might lead us to the suspicion that the clearing action of bran depends on the proportion of starch in it. Starch was therefore added to a solution of madder, and dyeing performed as on the addition of bran.

The result was different from what might have been anticipated. The addition of starch deteriorated the action. In the proportions of 1 lb. starch, 12 lbs. madder, and 12 lbs. mordanted cloth, the colour produced was much the same as when no starch was added. With 3 lbs. starch to 12 lbs. madder, the colour was considerably paler. On the edge of the dye pot a pasty looking matter was deposited, and much red varnish swam on the surface of the solution. As the red varnish is neither produced by the use of bran

itself nor by the proportion of 3 bran to 1 madder, it is obvious that bran does not receive its efficacy in dyeing from the starch contained in it, but from some other constituent.

Wheat flour acts with as little advantage but less disadvantageously than bran. In the proportion of 1 lb. flour to 6 lbs. madder and 6 lbs. mordanted cloth, the colour is only a little more red than madder-red dyed without an adjunct. When the quantity of flour is increased to 3 lbs. it acquires a similar action to the bran, but which is not stronger than is obtained by 4 lbs. bran, 6 lbs. madder, and 6 lbs. mordanted cloth. It follows, therefore, from this, that the action of the bran in madder dyeing, is not to be ascribed to the flour which is still contained in the wheat bran. These results led Runge to make the inquiry, to which of the constituents of bran does it owe its well known property, since it is neither due to the starch nor to the flour containing gum. He first examined the husks of the bran. To obtain these in a state of purity, wheat bran was washed so long with cold water as it continues to dissolve and remove starch or flour, and then the remaining brown coloured husks were added to the madder solution. It was then found that the husks of bran had a stronger action upon avignon madder than the same quantity of unwashed bran.

From what was said formerly, the action of bran appeared very doubtful. It seemed to redden brownish madder-red, and to distribute the madder-red over a greater surface. By the first action of the bran, a clearer red is obtained than usual; but the second, the production of a very saturated red is prevented. In respect to the husks of bran, they affect a distributive action. They equally hold the colouring matter, and render it difficult to combine with the mordanted cotton. The following experiment shews this:

In a solution consisting of 12 Avignon madder, and the husks of 36 bran, 3 of mordanted cloth were successively dyed three times with an increasing, and at a boiling temperature.

The result of the first dyeing with 3 cloth was a clear red not half so dark as the first dyeing with madder and bran, noticed in a former part of this treatise. In the

second and third dyeing, the colours were lighter, so that from this it appears, that the husks of 36 bran keep back the half of the colouring matter of 12 madder, while the corresponding quantity of bran in its natural state gives it up and deposits it on the calico. This seems to shew, that by washing the bran with water, substances are removed which by the use of unwashed bran are precipitated simultaneously with the colouring matter upon the mordanted calico, and assist in forming the madder-red. This also explains the higher shade of the madder-red dyed with bran. It yet remains to be explained, why cloth mordanted with alum mordant, by boiling with bran, acquires the property, after proper washing, of becoming blue in solution of iodine. This property of becoming blue certainly only demonstrates the presence of starch in the calico; but it renders it highly probable, that other constituents of the bran also combine with the calico. To ascertain this last point, the relation of the washings of the bran to madder in dyeing were also tried. The milky washings, but free from husks, of 36 bran were mixed with 12 madder, and therein 3 of mordanted cloth were three times dyed in succession. Colours were obtained, which were equal, though, when closely inspected, scarcely so clear as those obtained in the usual way with bran.

The last circumstance induced the following trial to be made. The milky bran washings at 68° deposited, after standing for two or three hours, a white mealy sediment, and the solution assuming a dirty yellow colour. Both the mealy deposit and the cold bran infusion were tested as to their relations to madder. The action of the mealy deposit, when well washed and dried, was similar to that of wheat flour.

Hence, it appears that the cold infusion of bran exhibits the same action as frequently appears in dyeing. For its employment, two lbs. of sifted bran were stirred for the space of an hour with 20 lbs. of water, then the solution was let off, and after filtration added to the solution of madder in different proportions instead of the bran. It gave in the proportion of 3 lbs. of the cold infusion to 6 lbs. of Avignon madder in the first dyeing with 3 lbs. of mordanted cloth, a very clear light red. The 3 lbs. of

cloth, by the second dyeing, obtained a dark very clear pink colour, and the 3 lbs., by the third immersion, was not half so dark. Unfortunately, it is not possible to give more depth to this red by altering the proportions. The cold infusion acts in this case like the husks. It prevents the formation of a dark saturated colour. When the proportion is diminished, 1 to 2 lbs. of infusion to 6 lbs. madder, the red is so much darker, that one can scarce distinguish whether bran alone was employed.

In the last place, the chemical properties of this solution were tested, especially the white precipitate, which a clear solution of sugar of lead produces in it. This was after proper edulcoration with water precipitated by hydro-sulphuric acid; the solution was clarified by heating it, and, after filtration, added to the madder solution. In this experiment its advantageous action on the madder-red was exhibited, although in a less degree than when common bran is employed.

Although the bran prevents the formation of very dark madder colours, this property, in certain circumstances, is a means of lightening the power of the madder colours in a considerable degree. An addition of bran causes a definite quantity of madder to give out more colour than happens when there is none present. This remarkable action, which occurs with no other matter yet known, goes so far, that solutions which already, in the usual way, are exhausted to such a degree by mordanted calico, that they produce only a reddish yellow colour, are rendered capable, by the addition of bran, of dyeing red again.

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#### ARTICLE IX.

##### ANALYSES OF BOOKS.

I.—Philosophical Transactions of the Royal Society of London for 1836. Part I.

##### MATHEMATICS AND PHYSICS.

*Discussion of Tide Observations made at Liverpool.* By J. W. Lubbock, Esq., F. R. S.

*Researches on the Tides.* 4th Series. *On the Empirical Laws of the Tides in the Port of Liverpool.* By the Rev. W. Whewell, M. A., F. R. S.

In a previous paper, the author endeavoured to obtain the mathematical laws of the inequalities of the tides from the results of the

London tide observations for 19 years. A similar table of the Liverpool tides having been published since, he uses these results in the present paper to test and improve the formulæ, to which he was led by the London observations. He shews, in a very satisfactory manner, that the Liverpool observations have confirmed his formulæ, the results of the means of large masses of observation agreeing with them, with a precision not far below that of other astronomical phenomena, as for example, a fraction of a minute in the times, and a fraction of an inch in the heights.

*Researches towards establishing a theory of the Dispersion of Light.* By the Rev. Baden Powell, M. A., F. R. S.

In a paper, inserted in the last part of the Transactions, the author commenced a comparison between the results of M. Cauchy's system of undulations, expressing the theoretical refractive index for each of the standard rays of the spectrum, and the corresponding index found from observation in different media. This comparison is there carried on for all the results obtained by Fraunhofer. But these include only a limited range of transparent bodies; and close as is the accordance in these instances, the theory cannot be considered as fully verified until we shall have extended a similar examination to a greater number of media, and especially to those of higher dispersive power. The author is at present engaged in this research. But has submitted a portion of his results to the public in the present paper. He compares them with Rudberg's experiments, which closely approximate. The substances examined are calcareous spar, quartz, aragonite, and topaz. From these researches it appears, that the hypothesis of undulations assigns the law and cause of dispersion in ten new cases in addition to the ten considered in his former paper.

*Researches in the Integral Calculus.* Part I. By H. F. Talbot, Esq., F. R. S.

The first inventors of the integral calculus observed, that only a certain number of formulæ were susceptible of exact integration, or could be reduced to a finite number of terms involving algebraic, circular, or logarithmic quantities. When the result could not be attained, they were accustomed to develop the integral in an infinite series. But this method is inadequate in an analytical point of view to supply the place of the exact integral. Fagnani, about 1714, made a great improvement. Euler further improved this branch of mathematics. Abel carried the improvement still further; and the author, in the present paper, details his interesting additions, and the steps of the processes by which he was conducted to make them.

(To be continued.)

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## II.—*On the Gales and Hurricanes of the Western Atlantic.*

By W. C. REDFIELD, Esq., of New York.

THE object of this pamphlet is to shew, that the violent gales which so frequently visit the Western Atlantic are not of an erratic and abnormal character, and to demonstrate, that they are guided by comparatively great regularity. The author, after a careful ex-

amination of the evidence of various storms, states, that he has found them to pursue generally a uniform course, which is always north-westerly in the tropical latitudes, and till they approach the latitude of 30° N. "In the vicinity of this parallel, the storms turn to the northward, and then their course becomes north-easterly on a track which appears to incline gradually to the east as they sweep over the higher latitudes of the Atlantic. The course thus pursued is entirely independent of the direction of wind which the storm may exhibit at the different points over which it passes; the wind in all such storms being found to blow after the manner of a whirlwind around a common centre or vortex during their entire progress, in a circuit which is commensurate with the lateral extent of the storm; and in a determinate direction or course of rotation, which is from right to left (that is, in the direction from west to south) horizontally." The direction of these gales would appear, therefore, to be that of the Gulf stream. The remainder of the paper is occupied with important details relative to particular storms, and will be found of great interest to meteorologists and nautical men.

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## ARTICLE X.

### SCIENTIFIC INTELLIGENCE.

#### I.—*British Association for the Advancement of Science.*

##### SECTION E.—MEDICAL SCIENCE.

(Continued from page 395.)

The second paper read this morning was by Dr. Houston, descriptive of a *Twin Fœtus*, born without *Brain, Heart, Lungs, or Liver*. The placenta was double, and there were separate membranes and cords for each fœtus. The umbilical vein of the imperfect infant opened directly into the vena cava, from which branches, *totally devoid of valves*, passed to all parts of the body. The arterial system, commencing from the venous capillaries and gradually running into larger trunks, formed a sort of aorta, like that in fishes, from which the umbilical arteries arose. No communication existed any where between the arteries and veins except at the capillaries; so that by whatsoever vessels the blood entered the body, by the same it must have been distributed. A round tumor, which existed in the substance of the cord, outside the umbilicus, had produced effects on the vessels calculated to throw light on the course of the circulation—the vein was varicose from the tumour as far back as the placenta, and the arteries were dilated on the side next the body of the fœtus.

There is much difference of opinion respecting the course of the blood in abnormal fœtuses of this nature, and such distinguished individuals as Sir Astley Cooper, Sir B. Brodie, Tiedemann, Monro, Blandin, Breschet, &c., have been engaged in the controversy. Sir A. Cooper has lately made a discovery, that there is a free anastomosis in the placenta, between the vesels of the cords, from which

he concludes that the circulation in the imperfect fœtus is carried on by the action of the heart of its companion in utero. Some of these authors argue that the blood proceeds from the placenta to the infant by the veins, and is returned therefrom by the arteries; others, that it passes in the same course by the arteries, and is returned again by the veins. Dr. Houston's observations go to establish, as a fact, that the course of the blood in the cords and placentæ of the two infants is the same, but that in thin bodies it is different; he considers that while the blood enters both bodies at the same time by the umbilical veins, it is transferred in the perfect infant to the aorta, through the foramen ovale of the heart, and is thence distributed in the ordinary manner; whereas, in the imperfect infant, it is conducted all through the body, without such transfer from vein to artery, and is thus made to take an inverted course—the veins of the body assuming the function of arteries, and, *vice versa*, the arteries that of veins.

Dr. Houston considers that the presence of anastomoses between the cords should rather lead to the inference, that the blood traverses both placenta in the same course, than that it takes a direction in the one different from that which it follows in the other. The accidental effect, produced by the tumour in the cord of the fœtus examined by him, proves to demonstration, that, in that instance at least, the blood entered the body by the veins, and returned by the arteries; and the absence of valves in the veins accommodated these vessels for the reception of the blood, and for its transmission in a retrograde direction through them.

Dr. Houston suggested several good reasons in support of his opinion, that the heart of the perfect fœtus can exert very little influence in propelling the blood into the vessels of the imperfect one; and considers that we must look to some other cause than a *vis a tergo* for the accomplishment of this object. He is of opinion, that the theory of "vital attractions and repulsions," though conveyed in terms which may be considered rather as expressive of the facts than as explanatory of them, approaches more nearly to the true one than any other which has been yet broached.

Dr. Houston suggested arguments to prove that the placenta possesses the same vital powers of attraction and repulsion as the living fœtus itself, and considers, that by such powers in mutual and reciprocal operation, the blood may be carried to and fro along the vessels of the cord without any farther mechanical influence whatsoever.

Several drawings of the fœtus were exhibited; and the reading of the paper led to a discussion, in which Dr. Prichard, Dr. Carson of Liverpool, Dr. O'Beirne of Dublin, Mr. Carpenter, and Dr. Macartney took part. It was a conceded point, that the circulation in the capillary vessels was independent of the action of the heart, and it was stated, that there was no case on record, in which a monster fœtus existed without a perfect fœtus; nor had there been ever any case discovered in which a single child had been found without a heart.

The third and last paper which occupied the attention of the Section this day was one by R. Carmichael, Esq., on *Tubercles*.

The reading of this paper, many of the propositions of which were illustrated by preparations in a high state of preservation, occupied one hour and a quarter, and commanded much attention. The following abstract will give our readers an outline of its contents:—

Mr. Carmichael commenced his paper on Tubercles with some remarks upon the great prevalence of these formations, and then proceeded to detail their appearances according to the descriptions of Lænnec and Carswell. He adverted to the use of the term scrophula, which he considers a cloak for ignorance; and having stated, that Drs. Todd, Clark, and Carswell believe in the identity of scrophula and tubercle, disputed this position, and likewise their opinion, that tubercles are inorganizable deposits. Among other objections, he urged the inconsistency of representing enlarged cervical glands, and pulmonary tubercles as identical, since it is well known that the former may be injected, but not the latter; and of maintaining the non-inflammatory origin of tubercles, together with the view that these bodies are lifeless matter; since, if such is their nature, they must excite inflammation in the tissues which contain them. He allows, however, that the scrophulous constitution disposes to tubercles, but only in the same manner as to cancer.

Mr. C. next adverted to the generally recognized connexion between scrophula and disordered digestion, and claimed the priority of this observation, by reference to a work which he published in 1810. He then proceeded to argue, at considerable length, in favour of the parasitical origin of tubercles, pointed out the absence of vascular communication between these bodies and surrounding parts, and observed, that so long as the former retained their vitality no inflammation takes place. The author declared his opinion, that carcinoma must likewise be arranged among the entozoa, and having indicated the division of a cancerous formation into a medullary and a cartilaginous portion, assigned to the former an independent vitality, the latter being only a barrier which nature sets up against the parasite; and showed that the containing cyst belongs to the surrounding tissue. The cartilaginous portion, he stated, might be injected, but not so the medullary. Tubercles he considers more allied to carcinoma than to scrophula. Having spoken of a difference between fungus medullaris, and fungus hæmatodes, he proposed to arrange the formations which had passed under review, as constituting four species of entozoa:—1. Tubercles found in the lungs; 2. Tubercles found in the abdominal organs; 3. Fungus medullaris and hæmatodes; 4. Carcinoma.

The author concluded by stating, that the doctrines of the independent vitality of tubercles was making considerable progress among the physicians of Germany. It was the business of the profession to point out the means of prevention rather than the cure of this disease. Wholesome nourishment, pure air, temperance and exercise, were the great preventatives. It was remarkable that the agricultural population were comparatively free from these affections—a circumstance which he accounted for, by the digestive organs being in a more healthy state, and furnishing the alimentary canal with fresh supplies of nourishment to supply the waste which was constantly

going on in the system. Multitudes, it was true, came into the world with this disease entailed on them; but this he considered a result of the breach of the moral laws of the governor of the universe, and a punishment inflicted on children reflected back from the parents, until, according to the statement in the decalogue, in the third or fourth generation, it ceased to propagate its contaminating influence.

*Wednesday, 24th August.*—The first paper read was by Dr. Hodgkin, on the connexion between the veins and absorbents. Dr. Hodgkin observed that the Committee appointed to make inquiries into this subject had been very fortunate, in the opportunities afforded them of examining the bodies of subjects in whom the lymphatics were much developed. There was great difficulty in injecting the lymphatics, it requiring a sharp eye and a delicate hand to be anything like successful. Mercury injected into the lymphatics will sometimes pass off by the veins, and some are disposed to admit a natural communication between these structures. In injecting subjects at Guy's Hospital, it was found that the mercury passed easily from the glands into the veins, in very recent subjects. The idea of transudation through the sides of the vessels must be rejected in mercurial injections, though it may happen when water is injected. Mr. Bracy Clark, in injecting the vessels in a horse, found a direct communication between the receptaculum chyli and the lumbar veins. Breschet is inclined to adopt the opinion that in the villi of the internal canal, the lacteals communicate by minute openings with the veins. If water is thrown into the arteries, it will almost immediately fill the lymphatic vessels. Dr. H. has seen lymph flowing in the thoracic duct tinged with blood. Mr. King has observed the fact that the thyroid gland contained a number of small cells, which were filled with a fluid differing from any other, and it is almost proved that there is a communication between the internal surfaces of these cells and the lymphatics of the organ. The most remarkable observations on the lymphatics have been made on the inferior animals. Dr. Hodgkin observed, that he believed the communication of the veins and lymphatics occasionally happened, but that they were not found at will. Dr. H. then explained the construction of the valves of the veins of the different vessels, and illustrated his description by diagrams.

Dr. Read then read his paper, entitled "A short Exposition of the Functions of the Nervous Structure."

Dr. Gayward then read to the Section a paper, by Mr. Alcock, containing some particulars on the Anatomy of the Fifth Nerve.

Dr. Macartney exhibited to the members a portable probang.

Dr. M. also read two short papers; one, an Account of the Organs of Voice in the New Holland Ostrich, and the other on the Structure of the Teeth.

The last paper was by Mr. Walker, on the Nerves and Muscles of the Eye Ball.

*Thursday, 25th August.*—The papers were—First, "A report of the Dublin Committee, appointed by the British Association, on the Motion and Sounds of the Heart," read by Dr. Macartney.

Secondly, "A report of the London Committee on the same subject," read by Dr. Clendinning.

Dr. Symonds read a letter from Dr. Spittal, of Edinburgh, stating that by reason of the death of Professor Turner, and in the absence of one of the members on the continent, the Committee of the Association had not been able to make any report on the same subject, the investigation of which was committed to them at the last meeting of the Association; but that it was their intention to go into the subject.

The third subject introduced was—"On the Gyration of the Heart, by Mr. Greeves." The following is an abstract of the paper:—

1. Muscular fibres can act as levers without a *solid* fulcrum, if there be another set of fibres set at an angle, and contracting simultaneously.
2. A hollow organ may be *dilated* by the construction of such an arrangement of fibres, if in contracting they become more parallel to a plane passing longitudinally along the axis of the organ.
3. That there are two spiral, two longitudinal, and one diagonal set of fibres in the heart, interlacing each other.
4. The ventricles gyrate incessantly to and fro upon their axis.
  - a. In systole, or involution, as the left hand pronates.
  - b. In diastole, or evolution, as the left hand supinates.
5. The double spiral curve of the two great arteries forms a compensating and regulating movement, causing
  6. i. A diminution of friction.
  7. ii. Steadiness and celerity of motion, on the principle of the tilt hammer.
  8. iii. An isochronous action, on the principle of the balance-wheel and spring.
  9. iv. The progression of the whole heart.
10. That the function of the auricle is to maintain the equilibrium of the venous system.
11. The first sound is produced by the sudden tension and sudden change of gyration occasioning vibration of the ventricular walls. The second sound is from flapping of the sigmoid valves.
12. The impulse is partly caused by the progression, partly by the atmospheric pressure, and chiefly by the left ventricle, first *gyrating into the proper position to do so*, carrying the apex against the thorax, with a force equal to the difference of strength between the right and left ventricles.
13. *Bruit de soufflet in the heart*, is the result of increased friction on the pericardium.

The author said he was aware his views on this subject were so very different to those generally entertained, that he appeared, as it were, on his trial before the philosophy of the kingdom, as to whether they were true or erroneous.

Dr. Carson, of Liverpool, after combating some of the propositions of Mr. Greeves, said, he saw nothing to induce him to resort to gyration, when dilatation seemed so natural. It was evident, on grasping the heart of an ox, for instance, that it expanded with great force; and he had heard nothing to induce him to alter his notion of the dilatation of the heart.

Dr. Williams said, he believed the elasticity of the heart was sufficient to account for the phenomenon of its dilatation.

A paper "On the Polarization of Light observed in the Crystalline Lens, by Sir David Brewster," was read by the President; as was also a letter from the same gentleman on the subject of cataract, which, if resisted in its early stages, the author believed might be overcome. This disease, which generally manifested itself between the ages of forty and sixty, when persons begin to require spectacles, Sir David gave a receipt for detecting, which he had done in his own case; and though, perhaps, induced by an impaired state of health, yet, by attention to diet and regimen, and taking care not to study by night, he found in about eight months he was cured. If the affection had not been checked in time, he entertained no doubt it would have ended in cataract.

The last paper read was *On Absorption*, by Dr. Carson, of Liverpool.

*Friday, 26th August.*—Mr. Adams stated the appearances he had observed in Chronic Rheumatism, viz., as in the arm, enlargement of the glenoid cavity and head of the humerus, and loss of the tendon of the biceps. He described several other morbid appearances, and shewed specimens.

Mr. Hetling read a paper "On a New Instrument for the removal of the Ligature of Arteries at pleasure."

Mr. Gordon exhibited some Anatomical Models.

The last paper read, was "On the Chemistry of the Digestive Organs," by Robert D. Thomson, M.D. The author began by drawing attention to the necessity of admitting chemical action as an important agent in digestion, because, inasmuch as every change in the position of the ultimate particles of matter is a chemical or electrical change, so the conversion of food into chyme and its assimilation must fall under this head. Dr. Thomson divided the consideration of the subject into—I. Chemical state of the stomach, first, in health; and, secondly, in disease. II. Chemical state of the mouth and œsophagus, first, in health; and, secondly, in disease. I. First, He remarked, that our most eminent physiologists had completely overlooked the experiments of Dr. Prout and others, which establish the fact, that in health the stomach contains a quantity of free muriatic acid. He referred to the recent experiments of Braconnot, who had found a great quantity of this acid in the stomach, and who had determined by very satisfactory experiments that no lactic acid was present. The author detailed an experiment, in which he had succeeded in converting muscular fibre into a substance exactly resembling chyme, by digesting it in dilute muriatic acid, on the sand-bath, during ten hours, taking care to keep the mixture as nearly as possible at a temperature equal to that of the human body. He, therefore, drew the conclusions:—first, that the stomach, in a state of health, when excited by stimulants, contains a quantity of free muriatic acid; and, second, that dilute muriatic acid is capable of producing by digestion with animal matter, at the temperature of the human body, a substance similar to chyme in its physical properties. From which it may be inferred, that free muriatic acid is an important

auxiliary in the process of digestion. Second, with reference to the state of the stomach in disease. Dr. Thomson observed, that the most common form in which chemical re-agents were affected, was by a redundancy of acid, occasioned by the introduction into that viscus of acid fruits and vegetables, which gave rise to fermentation, and the symptoms of heart-burn, a very familiar complaint. He next proceeded to describe the only other form of disease of the stomach, which was indicated peculiarly by the action of re-agents, by an alkaline state occurring in the disease commonly termed *pyrosis* or *water-brash*. Having investigated this disease very carefully with regard to its chemical nature, he showed that it proceeded from the diseased state of the secretion in the stomach,—alkali having taken the place of the free acid. By chemical analysis he found that the alkali was ammonia, and probably, also, a little soda was present. Having observed this very remarkable and important fact, the practice consequent upon it was evident, and the result proved of the most satisfactory nature; he found that the administration of acid gave immediate relief. If the case was of a chronic nature, he prescribed also anodynes,—as conium and hyoscyamus, in order to act directly upon the nerves, should they have been long subjected to the action of the diseased secretion. The author detailed the particulars of several cases. In one instance, a female had become greatly emaciated, in consequence of the disease having existed daily for three months,—the patient ejecting by the mouth, in the course of the day, not less than a pint of tasteless fluid. Dr. Thomson immediately prescribed for her an acid mixture, and in the course of two days, when he next saw her, the disease had entirely disappeared; nor was she again affected by it. The author stated that he had been unable to detect any general laws, which would seem to regulate this complaint. He had met with it in all constitutions and ages, and equally as abundantly in England as in Scotland. Butter and all oleaginous substances were liable to produce it, as well as the simultaneous use of apples and porter, at least in some individuals.

II. First, The author next proceeded to detail the results of his experiments upon the chemical state of the fluids of the mouth during health, which, in confirmation of the experiments of Donné, of Paris, he had found to be alkaline, and sometimes neutral. He noticed the experiment of Donné, which would appear to prove that the mucous membrane of the alimentary canal (which is alkaline) and the skin (which it is well known is acid) constitute a kind of voltaic pile; for when one of the poles of a delicate galvanometer is placed in contact with the mouth, and the other with the skin, very distinct electric currents are produced, which cause the needle to deflect  $15^{\circ}$ ,  $20^{\circ}$ , and sometimes  $30^{\circ}$ .

Second, the author stated that he had found the mouth indicating an acid re-action whenever inflammation existed in any of the membranes in connexion with it, as in laryngitis, pleuritis, bronchitis, gastritis, and enteritis, and in other diseases of an inflammatory nature. He directed the attention of medical men to this fact, as a most important feature in the diagnosis of such diseases. He stated

that he had extended his observations to many inflammatory diseases, and had found, uniformly, that inflammation of mucous and serous membranes in all parts of the body, is attended by the secretion of free acid. Hence the scientific method of removing this source of irritation in such diseases, viz., by the local application of alkaline solutions, as in erysipelas, inflammation of the urethra, &c. He stated also that he had examined the chemical composition of the membrane deposited in croup, and had found its principal constituent to approach nearer the character of albumen than any other animal substance, which would add some weight to the opinion of Donné, that morbid products derive their origin from the free acid secreted on the surface of the membrane upon which the product is deposited.

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*Additions to the different Sections.*

WE are indebted for the following reports to their respective authors.

*On a method for ascertaining the strength of spirits*, by Mr. William Black. (The author of this paper is well known by his valuable "Practical treatise on Brewing and on Storing of Beer, deduced from forty years experience.")

I believe it has for long been a desideratum with government to find a more scientific and accurate mode of ascertaining the strength of spirits than that now in use. A very slight inattention in the method of using the hydrometer may make a difference of at least five per cent., and when the spirits are adulterated with sugar or salts, that instrument is totally useless. It is a well known fact that when equal quantities of proof spirits and water are mixed together at a temperature of between 50° and 60° (Fahrenheit), the thermometer, if immediately immersed in the mixture, will rise 9½ degrees. I do not, however, think it is so generally known that the thermometer rises more or less, according to the strength of the spirits, and that it does so apparently in very regular progression, when the spirits are between the strengths of 45 per cent. over and 45 per cent. under proof.

When spirits, 45 per cent. over proof, are mixed in equal quantities with water, both being of the same temperature, *i. e.* between 50° and 60°, the thermometer, if immediately immersed in the mixture, will rise 14° degrees; but with the strongest alcohol, also mixed with an equal quantity of water, it will not rise above that temperature; no further concentration therefore takes place, unless more water be added, shewing, I should think, that alcohol can only combine with water in atomic proportions, and that a certain portion of that spirit must remain in the first mixture in an uncombined state.

Every degree on the thermometer appears to indicate a difference in the strength of the spirits of about 10 per cent; thus if we mix equal quantities of spirit, 10 per cent. over proof, and water, both at equal temperatures of about 55°, the thermometer will rise 10½°; with spirit 20° over proof, mixed as above, it will rise 11½°; and so on—one degree for every 10 per cent. over proof, until it reaches about from 40 to 45 over proof, when no further increase is apparent, unless, as I have before stated, more water be added.

The thermometer seems to act in a similar manner with spirits under proof; thus, with spirits ten per cent. under proof, mixed with water as above, it will rise about  $8\frac{1}{2}^{\circ}$ , and one degree less for every 10 per cent. under proof, until we get to  $45^{\circ}$  under proof; after which, although a rise does take place, the indications do not seem to be so regular.

When the spirits are mixed with sugar increasing the specific gravity so as to falsify the hydrometer 20 or 30 per cent. or more, the indications of the thermometer are precisely the same, making allowance for the slight difference in volume caused by the mixture of sugar.

If the mixtures be made at higher temperatures, the indications of the thermometer are proportionally a lesser number of degrees, according to the temperature; I think when between  $70^{\circ}$  and  $80^{\circ}$  nearly 2 degrees less, but the progressions appear to go on regularly as before.

I do not, however, presume to give the above as accurate results, but merely to state that the thermometer appears to indicate a regular progression according to the strength of the spirits, and the temperatures at which they may be mixed with the water.

My only desire at present is to draw the attention of men of science to the subject, who may discover some mode of application which may render it available, and perhaps accurate, in ascertaining the qualities of spirits or acids.

*Abstract of a Paper read before the Members of the British Association, at Bristol, August 26, 1836, entitled, "On some Fallacies involved in the Results relating to the comparative Age of Tertiary Deposits obtained from the Application of the Test recently introduced by Mr. Lyell and M. Deshayes."*

By EDWARD CHARLESWORTH, ESQ., F. G. S.

DURING the author's investigation of the fossiliferous strata above the London clay in Suffolk and Norfolk, some facts have come under his observation, which appear to him to point out sources of error to a considerable extent in the application of the test recently proposed by M. Deshayes and Mr. Lyell, and which is now so generally made use of in the classification of tertiary formations.

The crag has been referred by Mr. Lyell to his older *pliocene* period, on the authority of Deshayes, who identified among the fossil testacea of that deposit 40 per cent. with the existing species. The correctness of this result has been called in question by other eminent conchologists, particularly by Dr. Beck, of Copenhagen, who has examined the crag fossils in the author's collection, and considers that the whole of them are extinct. In this opinion Dr. Beck is supported by Mr. G. B. Sowerby, who states, that he has only met with two or three crag shells, which may, perhaps, be identified with existing species. Professor Agassiz has inspected an extensive series of ichthyological remains, collected from the crag by the author, and pronounces them all to belong to extinct genera or species; while a precisely similar result has attended Dr. Milne Edwards's examination of the corals.

Professor Phillips, in his introduction to geology, has placed the

crag in the *miocene* division; while Dr. Fleming, who, for more than a quarter of a century, has been an indefatigable collector of British shells, considers that the proportion of recent species in the fossils of that formation has been rather *'under* than over rated by Deshayes; and among the corals of the crag he has detected a large proportion of living forms.

The particular one of Mr. Lyell's divisions to which a geologist will refer any given deposit must therefore depend upon his own estimate of the characters which constitute specific distinctions, and which is evidently liable to the greatest possible amount of variation.

The author next enters upon an enquiry respecting the course which should be adopted in obtaining the relations of analogy presented by the fossils of different deposits to one another, or to the races in existence at the present period. The effect of the method now made use of is to class as contemporaneous those deposits which respectively furnish the same per-centage of extinct forms, without the slightest reference to the greater or less degrees of approximation which these forms exhibit, when compared with living types. The conchologists who agree with Dr. Beck cannot, by means of the per-centage test, express the difference in the amount of approximation presented by the testacea of the crag and London clay to those now existing, because they would consider all the fossils of both these formations extinct, and, consequently, refer them both to the *eoene* division.

In this instance, the relations of analogy can only be obtained by a general estimate of the amount of resemblance borne to existing species by the entire series of crag or London clay fossils, taken collectively. This mode of procedure may, at first, appear only a different adaptation of the numerical plan adopted by Mr. Lyell. It will, however, be found an important modification of his principle; for, when applied to the fossils of those formations which, from the presence of living species, can also be subjected to the per-centage test, it will, under some circumstances, furnish results that clearly establish a fallacy in one of the two methods. For instance, the red and coralline crag are supposed by Deshayes to contain the same number of extinct species; and, by the per-centage test, they, therefore, present an equal approximation to the existing organization. But if the shells, which Deshayes thinks he can identify with those now inhabiting the German Ocean, are rejected, and the extinct testacea alone compared with living types, the forms most remote from existing species will be found to occur in that series which has been derived from the coralline crag.

The author then changes his line of argument, and, assuming that there is a general agreement among conchologists as to the characters which should be depended upon in discriminating species, and also, that the per-centage test is the true method of obtaining relations of analogy, he proceeds to inquire whether the association of organic remains in fossiliferous deposits implies their previous contemporaneous existence. The evidence drawn from this source appears to the author to be by no means so conclusive as it has been generally considered; and his opinions have been formed principally from an attention to the causes now in operation upon the earth's surface.

\* “The small part of this island, occupied by the crag formation, is intersected in one spot with several estuaries, which have completely removed this generally superficial fossiliferous stratum, the bed of the estuary being formed in an older formation. Along the banks of the Deben, which flows through a part of the coralline crag, in some spots the fossil shells line the shore in greater numbers than the recent testacea; and, during the period in which this estuary has been formed, prodigious numbers of these fossils must have been swept down into the German Ocean, and there indiscriminately mingled with the *reliquiæ* of existing species of Mollusca. It is not merely the extent of surface at present occupied by these estuaries which has thus been denuded of the crag, but considerable tracts of marsh land formerly connected with them, but from which the water has since been shut out, have also lost this original covering. Within a very short distance of the Deben, another estuary, the Stour, flows through a lacustrine deposit belonging to the *newer pliocene period*; and here, in addition to the shells, is a considerable stratum of mammalian remains, which, at one period, evidently extended as far as the opposite bank of the river, a distance of about a mile and a half or two miles.

“I must now look forward some few thousand years, and anticipate the time when, by the recession of the sea, or the elevation of the land, the deposits forming at the mouths of these estuaries has become accessible, and is made the subject of geological investigation. I must also assume, that the geologists of that remote period have followed the same course of induction that has recently been pursued, and have arrived at similar conclusions respecting the course to be adopted in ascertaining the relative antiquity of tertiary deposits. The age of the formation in question, then, is about to be tested by comparing its organic remains with the then existing species. Of what will these fossils consist, and whence will they originally have been derived? The bones of such animals as are now drifted down the rivers Deben and Stour will be mingled with those of the extinct Mammalia of the *newer pliocene period*. The living species of Mollusca now inhabiting the German Ocean, will be found associated with the extinct Testacea of the *newer pliocene*, *older pliocene*, and, perhaps, even *miocene* epoch. Yet this deposit, in which the organized beings of different geological periods shall be found thus indiscriminately mingled, will be one exhibiting every appearance of regular stratification; a deposit in which a large portion of Testacea will be found naturally grouped, and, in which, there will be the clearest evidence of their having become entombed on the spot which they had long previously inhabited. That the influences of causes now in operation is really producing such an effect as the one now described, admits of almost actual demonstration; for the fossil shells of the crag are thrown up along various parts of the Suffolk coast, several miles from the spots in which they have been carried down.

“It may be said, that these older shells, entering into the new deposits, carry with them evidence of the stratum from which they

\* The portion between inverted commas is given at full length.

have been derived; or that, at all events, their worn appearance would distinguish them from the more recent *Mollusca* with which they are associated. This is so far from being the case, that considerably finer and more perfect specimens of the *Volùta Lamberti* can be picked up on the sea shore, where they have been dashed by the waves upon a shingly beach, than can ever be obtained from the beds of the crag formation itself. In fact, this gradual process of degradation appears, in many instances, to be of all others the most favourable for detaching organic remains from the matrix in which they are embedded; and, with respect to the evidence that might possibly be supposed to arise from a difference in lithological character, it should be remembered, that even if such indications did exist, by the time these new deposits become accessible, every vestige of the crag will have disappeared. There will, consequently, be nothing to excite the slightest suspicion that the crag species are not contemporaneous with all the organic remains associated with them. In adopting this line of argument, I am, of course, supposing that the geologists of a future epoch have the same amount of information respecting the history of the tertiary deposits of those days that we have of our own, and not that a geological record of events has been continued up to that period.

“To a certain amount, then, this admixture of fossil with recent shells, even in regular stratified deposits, cannot be denied; but it may be urged that it takes place only under peculiar circumstances, and to such a limited extent as would never interfere with the accuracy of general inductions founded upon extended research and careful practical observation.

“If, however, we enlarge our field of observation, we shall find that a process has been going forward, attended with similar results, over a tract, the superficial extent of which far exceeds that occupied by the whole of the crag formation. The bed of the ocean, all along the coasts of Norfolk, Suffolk, and Essex, and probably as far as Kent on the one side, and Yorkshire on the other, is strewn with multitudes of the bones of extinct *Mammalia*. These remains have been taken up twenty miles from the shore; and, in dredging for oysters, the fishermen have suffered considerable inconvenience from the number of elephants' bones and teeth which become entangled in their nets. Mr. Woodward supposes that the grinders of at least 500 elephants have been fished up off the oyster-bed at Happisburgh;\* and, from the numbers which I have seen, I have no reason to think this calculation is exaggerated. I do not now propose enquiring whence this prodigious accumulation of fossils has been derived, or to what geological epoch they should be referred: it is sufficient for my present purpose to feel satisfied that they are the remains of beings belonging to a remote era, which are becoming entombed, covered with the *balani* and *zoophytes* that now inhabit the German Ocean. These are facts which, I presume, will not be disputed; and yet so entirely has the operation of existing causes in this respect been overlooked, that Mr. Lyell fully concurs in the assumption that,

\* A village on the Norfolk coast, between Cromer and Winterton.

in undisturbed stratified deposits, the embedded organic remains must necessarily have existed contemporaneously; and upon this evidence, solely, important conclusions have been drawn respecting the bones of elephants, associated with the shells of existing species of Mollusca, in a deposit in Yorkshire.\*

The next point adverted to in the paper is the presence of *secondary* fossils in the *upper* or *red crag*. During the formation of this deposit, causes similar to those now in existence appear to have been in operation; and effects have there been produced which exactly correspond with the author's deductions as to the nature of the formations at this time in progress round some parts of the British coast.

This introduction of secondary shells in the tertiary beds of Norfolk and Suffolk has been detected solely by an attention to lithological characters; and the evidence derived from this source is no longer available when there is reason to suspect an admixture of organic remains belonging *exclusively* to rocks of the supra-cretaceous series.

The species which are *common* to the chalk and red crag are very few when compared with those which are common to the *red crag* and to the subjacent *tertiary* strata. In the latter case, however, we have no means of ascertaining whether those individual species which occur in separate formations existed throughout distinct periods, or, like the fossils of the chalk, were, by the natural process of degradation, removed from their original matrix, to be again entombed with the races of a more recent epoch. Unless this difficult problem is solved, it is clear that the application of the percentage test may be attended with the most fallacious results. To what extent erroneous conclusions may already have been formed, from the neglect of those considerations so obviously necessary in the examination of the crag, must be a subject for future investigation.

The author lastly notices some questions which have already been discussed by Professor Phillips in the *Encyclopædia Metropolitana*.† The most important of these is the physical relation existing between any one fossiliferous deposit, and the locality in which the living types of its fossil species occur.

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## II.—Belfast Museum.

THE eighth public meeting of the Natural History Society was held in the Museum, on Wednesday, the 25th of May; about one hundred and twenty members and visitors being present. After several donations had been presented, one of the Secretaries read the following Report of the Council:—

\* "That these quadrupeds, and the indigenous species of Testacea associated with them, were all contemporary inhabitants of Yorkshire (a fact of the greatest importance in geology), has been established by unequivocal proofs, by the Rev. W. V. Vernon, who caused a pit to be sunk to the depth of more than 200 feet through *undisturbed* strata in which the remains of the mammoth were found embedded together with the shells, in a deposit which had evidently resulted from tranquil waters." (*Lyell's Geology*, vol. i. p. 96. edit. 1.)

† *Vide* Article Geology.

The council of the Society, according to the usual custom, have now to lay before the members a general report of the proceedings of the present session. It is one whose course has been attended by the same unanimity among the members, and the same progressive increase of their number, which have so justly formed a subject of congratulation on former occasions. But it is one which has not been marked by any unusual or remarkable event, such as distinguished the two preceding sessions. In one of these, a debt of nearly £800 had been discharged: in the other, the unfinished portions of the building had been completed. Our history, during the past session has not been of embarrassing circumstances overcome, or of serious difficulties surmounted, but is one of cheerful and prosperous advancement, less eventful, but not less gratifying, furnishing less to record, but not less on which to frame our pleasing recollections of the past, and our happy anticipations of the future. The grounds on which this conclusion rests, it will now be the duty of the council to lay before you, with that brevity which is most befitting when communicating to their fellow-members an abstract of occurrences, of which many of them are already cognizant.

During the past year, an opinion has been gaining ground among many of our townsmen, that an extension of lectures, an extension adapted to the increasing commerce and manufactures of the town, would be highly beneficial. The council of the Society, while they felt unwilling to introduce any changes in the course they had hitherto pursued, and which had been so eminently successful, were, at the same time, most desirous of extending, as widely as possible, the utility of the Museum, and of interesting a still larger number of their townsmen in its prosperity. They, therefore, agreed that for the present session, papers on Natural Philosophy and Statistics should be admissible, in the same manner as those on Zoology, Botany, Mineralogy, and Topography, and that when a sufficient number of persons were enrolled, as desirous of reading exclusively on these subjects, particular nights should be set apart for the purpose. By this arrangement the original constitution of the Society remains unchanged, and preparation has been made for adapting it to the altered situation of the community among which it has been established.

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### III.—*Pharmacy, &c.*

1. *Adulteration of Iodine.*—Stieren has detected several adulterations of iodine, by dissolving the latter in spirit. The impurity remains undissolved, consisting sometimes of iron, silica, and alumina, at other times of iron, containing carbonaceous matter. Buchner has found glance coal.—*Buchner's Repert.*, v. 230.

2. *Volatile Oils.*—Völter and Dann have made a set of experiments to determine the relative produce of oils to the raw material employed. The following is the result. We use the German measures, which approach nearly our own, the Nuremberg pound being equal to .959266 lb. Troy, the lb. or civil pound consisting of 16 ounces. Multiplication by this number gives the equivalent in English.

1. *Oil of Bitter Almonds*.—26 lbs. of almonds pressed cold, and then distilled with water, gave  $10\frac{1}{2}$  lbs. of fat, and 2 ounces of volatile oils.

2. *Ol. Anisi aeth.*—16 lbs. of anise seeds gave 7 ounces of volatile oil, and 10 lbs.  $2\frac{1}{2}$  ozs. of the same, spec. grav. 0.984.

3. *Ol. Anisi stellati*.—10 lbs. of the seeds gave 22 drs. volatile oil.

4. *Ol. Calami aromat.*—14 lbs. of dry roots left 20 ozs. volatile oil; 118 lbs. fresh roots were peeled, and the 45 lbs. of bark left by distillation  $3\frac{1}{2}$  ozs. of oil; the roots, when dried, weighed  $13\frac{1}{2}$  lbs.

5. *Ol. Carvi*.—15 lbs. of the seeds gave 7 ozs. oil; 10 lbs. gave 7 ozs. of spec. grav. .915.

6. *Ol. Caryophyl. aromat.*—1 lb. gave 20 to 21 drs.

7. *Ol. Caryophyl.*— $6\frac{1}{2}$  lbs., when distilled three times, gave  $18\frac{1}{2}$  ozs. oil of spec. grav. 1.232.

8. *Ol. Cerae*.— $1\frac{1}{2}$  lb. *Cer. flav.* gave, by dry distillation, 5 ozs. 5 drs. of oil.

9. *Ol. Coriandri*.—32 lbs. of the seeds gave 2 ozs. 7 drs. oil.

10. *Ol. Cynae sem.*—165 lbs. of the seeds left 14 ozs. 3 drs. oil.

11. *Ol. Cynae*.—5 lbs. seeds of *Cyn. levant.* gave 4 drs. 1 sc. oil;  $\frac{1}{2}$  lb. seeds of *Cyn. lev.* gave 10 drs. *Ext. Cyn. aeth.*;  $2\frac{1}{2}$  lbs. *Sem. cyn. barb.* gave 3 drs. 50 grs. oil; 13 lbs. *Sem. cyn. natur.* left  $2\frac{5}{8}$  lbs of an earthy powder effervescing with acids.

12. *Ol. Foeniculi*.—12 lbs. seeds gave 5 drs. oil. 3 lbs. seeds gave  $14\frac{1}{2}$  drs. oil spec. grav. 0.968.

13. *Ol. Junip. bacc.*—21 lbs. of fresh berries left 26 drs. clear oil.

14. *Ol. Macis*.— $1\frac{1}{2}$  lb. mace gave  $18\frac{1}{2}$  drs. oil of spec. grav. .920.

15. *Ol. Marjoranae*.—82 lbs. left 11 ozs. oil.

16. *Ol. Menth. piperit.*—374 lbs. of the plant gave  $49\frac{1}{2}$  ozs.

17. *Ol. Petroselin.*—4 lbs. seeds afford  $1\frac{1}{2}$  oz. of an oil sinking to the bottom of water.

18. *Ol. Sinap. Sem.*—35 lbs. gave 11 drs. of oil, and by other experiments, 15 lbs. gave 6 drs., and 50 lbs. gave 31 drs. The greatest product was 8 drs. oil from 10 lbs.

19. *Ol. Aether Tanacet.*—20 lbs. of the tops gave 1 oz of oil.

20. *Ol. Valer. aether.*—10 lbs. of the root gave 12 drs. oil, and 22 lbs. gave  $18\frac{1}{2}$  drs. of oil spec. grav. .960.—*Central. blatt.*, June, 1836.

3. *Morphin in Native Green Poppy Heads*.—According to Du Menil, morphin exists in common poppy heads, but in small quantity. He evaporated the expressed juice of the poppy head in a water bath to the consistence of honey, exhausted the residue with spirit of 80 per cent. slightly rendered acid by sulphuric acid, distilled the greenish solution, added water to it, filtered it, neutralized the solution with ammonia, although not completely, precipitated it by a solution of galls, collected the precipitate, washed it, digested it with lime water, dried the mixture in a water bath, pulverized it, digested it with spirit and distilled. The residual solution left behind a very small quantity of a resinous matter (3 lbs. poppy heads gave  $\frac{1}{2}$  gr.), which tasted somewhat bitter, and was coloured scarlet by concentrated nitric acid, and blueish by chloride of iron.—*Ibid.*, August, 1836.

#### IV.—*Easy Method of preparing Spongy Platinum.*

IF native platinum is fused with double its weight of zinc, the resulting alloy reduced to powder, and treated first with dilute sulphuric acid, and then with dilute nitric acid, in order to oxidize and dissolve all the zinc, an insoluble residue is obtained, consisting of platinum in the form of a grayish black powder mixed with an osmiuret of iridium, which not only possesses the same properties as platinum after it has been properly purified by washing with caustic potash and water, but has such an oxidizing power that it converts formic acid into carbonic acid, alcohol into acetic acid, and even the osmium contained in it into osmic acid. This plan was pointed out by Descotils more than 30 years ago; but he was not acquainted with its powers. He did not inquire into the cause of its detonating like gunpowder, and the effect of taking away this property which muriatic acid possesses. Berzelius supposed that it was combined with hydrogen; but Dobereiner's experiments have shewn this opinion to be quite erroneous. If platinum, separated from zinc, is slightly moistened with alcohol, it inflames, or rather becomes incandescent, and disengages osmic acid; but if the alcohol is mixed up into a paste with it, the circumstances are altered, and acetic acid alone is formed. This is the simplest method of purifying the air of a room, and of removing the odour of nicotine.—*Ann. der. Pharmacie, January, 1836.*

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#### V.—*Temperature of Space.*

AT Fort Reliance in N. L.  $62^{\circ} 46\frac{1}{2}$ , W. L.  $109^{\circ} 0' 39''$ , Captain Back observed the spirit of wine thermometer to sink as low as  $-70^{\circ}$  F. Arago concludes from this that the temperature of space must certainly be under  $-70^{\circ} \cdot 6$ . Poisson, however, does not admit this conclusion, for according to him the temperature of the upper stratum of air is considerably lower than that of space.—*Poggen-dorff's Ann., xxxviii. 235.*

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#### VI.—*Acid Beer.*

A PATENT has been taken out by Mr. Stowell, in America, for preventing beer from becoming acid in hot weather, or between the temperatures of  $74^{\circ}$  and  $94^{\circ}$ . To every 174 gallons of liquor we are directed to apply one pound of raisins, in the following manner. "Put the raisins into a linen or cotton bag, and then put the bag containing the raisins into the liquor before fermentation. The liquor may then be let down at  $65^{\circ}$  or as high as  $70^{\circ}$ . The bag containing the raisins must remain in the vat until the process of fermentation has so far advanced as to produce a white appearance or scum all over the surface of the liquor, which will probably take place in about 24 hours. The bag containing the raisins must then be taken out, and the liquor left until fermentation ceases. The degree of heat in the place where the working vat is situated should not exceed  $66^{\circ}$  nor be less than  $60^{\circ}$ ." To prevent distillers wash from becoming acid, two pounds of raisins should be put into 150 gallons

of the wash, the raisins being chopped and put in without a bag, and 1 lb. of hops should be put into the wash vat for every 8 bushels of malt at the time of mashing, and  $\frac{3}{4}$  of a pound of hops for every bushel of malt brewed, to be boiled in the liquor in the copper.—*Journal of the Franklin Institute, Sept., 1836.*

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BOOKS ANNOUNCED OR NEWLY PUBLISHED.

*Just Published,*

*The British Annual and Epitome of the Progress of Science.* Edited by ROBERT D. THOMSON, M.D. The plan of this work is similar to that of the *Annuaire par le bureau des longitudes*, which has gained great popularity in France. The first part of the British Annual is occupied with a calendar for the year 1837. This is followed by Divisions of Time, Tables of the Elements of the Solar System, Positions of Observatories, Heights of Mountains, &c., Complete tables of English, Swedish, French, German, and Portuguese Weights and Measures; principal foreign Commercial Weights and Measures; Tables of the Coins of different Countries, with their Weights; Tables for calculating the Altitude of Mountains from Barometric Observations; Tables of the Specific Gravity and Atomic Weights of Bodies; Universities of England, Scotland, France, Denmark, with the incomes of the Professors; American Colleges; Lists of the office bearers of the different learned societies, with the fees; Statistics of Glasgow, for 1832, by DR. CLELAND, &c.

The second part of the Annual contains, *Recent Progress of Optical Science*, by the Rev. BADEN POWELL, M.A., F.R.S., Savilian Professor of Geometry, Oxford.

*Experiments and Observations on Visible Vibration and Nodal Division*, by C. TOMLINSON, ESQ., &c.

*History of Magnetical Discovery*, by THOMAS STEPHENS DAVIES, F.R.S., L. & E., F.R.A.S., of the Royal Military Academy, Woolwich.

*Recent Progress of Astronomy*, by W. S. B. WOOLHOUSE, ESQ., F.R.A.S., Head Assistant of the Nautical Almanac Establishment.

*Recent Progress of Vegetable Chemistry*, by ROBERT D. THOMSON, M.D.

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THE original edition of the Antiquities of Athens, by the celebrated Stuart, is now in course of publication, so arranged that each Edifice is complete in one Part, with brief explanations of the Engravings, by this means the Student may consult the first authority in any particular order of Grecian Architecture, separate from the rest of a work of twenty-four guineas value, and now very scarce.

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THE Report of Sir David Barry and Dr. Corrie, on the Medical Charities of Ireland, is now published. These gentlemen were appointed by Government, Commissioners for investigating the Management of Hospitals and Asylums.

# Meteorological Journal, -- For October.

Kept at the Manse of the Parish of Abbey St. Bathans, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea.  
By the Rev. JOHN WALLACE.

DATE.	THERMOMETER.			HYGROMETER. (Liasle s.)			BAROMETER. At 32° of Fahrenheit.			Rain inches. Weekly.	Direction of Wind at X. A. M.	REMARKS.
	X. A. M.	III. P. M.	X. P. M.	IX. A. M.	III. P. M.	X. P. M.	IX. A. M.	X. A. M.	X. P. M.			
1835.												
Sa.	41	42½	45½	3	0	0	28-728	28-697	28-426		E. by S.	Brisk wind, morning lowering, from 10 A.M. to 5 P.M. heavy rain, evg. clear.
Su.	40½	41½	45½	6	10	0	28-923	28-253	28-352		W. N. W.	Strong wind, occasional showers, troubled sky, evening nearly calm.
Mo.	46½	43	46½	15	17	0	28-419	28-437	28-455		S. W. by W.	Brisk wind, cumulous clouds abundant on a blue sky, evening overcast, rain.
Tu.	4	47½	49½	6	12	17	28-758	28-775	28-990	1-250	S. W. by W.	Calm, sky overspread with cumuli and cirrocumuli, evening clear.
We.	5	43½	46	4	8	1	29-152	29-171	29-185		S. W. by W.	Calm, sky overspread with cumulous clouds, P.M. showery, evening clear.
Th.	6	48	46	3	5	18	29-277	29-277	29-201		S. W. by W.	Calm, A.M. masses of cumulous clouds abundant, P.M. clear, evg. overcast.
Fr.	7	50½	52	0	2	0	28-880	28-897	28-885		S. E.	Calm, cirrostratus prevalent with soft clouds floating below, in the evg. rain.
Sa.	8	54	53½	5	4	0	28-678	28-678	28-618		S. E.	A.M. brisk wind, light showers occasionally, P.M. wind decreasing, evg. calm.
Su.	9	44½	45½	6	5	1	28-600	28-568	28-525		S. E.	Brisk wind, A.M. cirri on a blue sky, P.M. overcast or lowering, showery.
New M.	10	48	49½	4	3	0	28-436	28-406	28-725		S. E.	Calm, morning clear, mid-day heavy rain, P.M. gradually clearing, evg. clear.
Mo.	11	44½	44	0	0	8	28-781	28-787	28-794	1-313	N. W.	Gentle wind, A.M. heavy rain, P.M. clear sky, evening calm with rain.
We.	12	45½	48	1	3	15	28-091	28-140	28-348		S. W.	Gentle wind, A.M. cirrostratus prevalent, P.M. calm, in the evening heavy rain.
Th.	13	49½	46	1	3	10	29-008	29-064	29-231		S. W.	Boisterous wind, A.M. tendency to rain, P.M. clear, wind much decreased.
Fr.	14	47½	48	16	17	19	29-416	29-401	29-309		N. W. by W.	Gentle wind, cumuli occasionally floating on a blue sky, evening calm.
Sa.	15	38½	43	0	1	0	29-590	29-619	29-633		S. W.	Gentle wind, A.M. cirrostratus prevailing the sky, soft clouds below, P.M. rain.
Su.	16	48	50	2	3	5	29-631	29-631	29-584		S. W.	Calm, A.M. cirri and cirrostratus prevalent, P.M. cloudless.
Mo.	17	44½	47	2	4	2	29-390	29-368	29-292		S. W.	Calm, A.M. cirri abundant on a blue sky, P.M. cirrocum. prevalent, evg. clear.
Su.	18	52½	54	1	2	0	29-335	29-351	29-614	0-209	S. W.	Calm, A.M. cirri abundant on a blue sky, P.M. a heavy shower, evg. cloudy.
We.	19	42½	46	6	15	24	29-535	29-561	29-614		W.	Wind strong and occasionally boisterous, wind rising in the evening.
Th.	20	48½	46	5	10	12	29-891	29-901	29-843		S. W.	Brisk wind, heavy masses of cloud often prevalent, evening cloudless.
Fr.	21	48	49½	0	0	3	29-724	29-730	29-708		S. E.	Gentle breeze, cirri on a blue sky, in the evening rain.
Sa.	22	48	48½	0	0	0	29-845	29-854	29-864		S. E.	Calm, overcast, A.M. occasional drizzle, evening hazy, a lunar halo.
Su.	23	53½	53½	11	10	9	29-716	29-695	29-741		W. N. W.	Very calm, A.M. drizzle, P.M. fog, evening cloudy, wind rising.
Mo.	24	54½	57	9	14	14	29-610	29-632	29-592		S. W.	Gentle wind, sky nearly cloudless, wind rising in the evening.
Tu.	25	51½	52½	0	8	13	29-425	29-409	29-263	0-105	N. W. by W.	Gentle wind, masses of soft cloud passing over a blue sky.
We.	26	51½	52½	13	16	26	28-964	29-029	29-250		W.	Brisk wind, masses of soft clouds floating, P.M. and evg. clouds floating.
Th.	27	37½	37	8	10	9	29-352	29-338	29-584		W. N. W.	Gentle wind, masses of soft clouds floating, occasionally overcast, evg. windy.
Fr.	28	30	31½	0	5	20	29-154	29-209	29-432		N. W. by W.	Boisterous wind, large masses of clouds driving, in evening wind decreased.
Sa.	29	32½	32½	6	8	11	29-614	29-622	29-618		N.	Brisk wind, A.M. large masses of cloud floating, P.M. overcast, snow.
Su.	30	32½	32½	11	11	0	29-646	29-646	29-564		N. W. by W.	A.M. strong wind, hail and sleet, P.M. heavy clouds, occasional hail showers.
Mo.	31	32½	32½	11	11	0	29-646	29-646	29-564		N. W. by W.	Brisk wind, heavy masses of cloud on a disturbed sky.
Means.		45	46	5	7	9	29-162	29-169	29-170	2-932		Calm, cirrostr. polarized N. to S., tendency to cirrocum. & cymoid formations.

Means { Therm. 44°5 } Mean temperature of spring water 46°4  
 for the { Hygr. 5° } Mean point of deposition 41°7  
 Month { Bar. 29.180 in. } Moisture in a cubic inch of air = .00183 grs.

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