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PROCEEDINGS

OF THE

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

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NOTE.

THE object which the Society have in view in publishing their Proceedings is to give an immediate and succinct account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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ERRATA.

Page 96, line 3, for $Q_3 = \frac{2}{3}P_1$ — read $Q_3 = \frac{P_1}{3}$.

Page 96, line 6, for $2Cu(NO_3)_2 + 2N_2O_2$ —
read $3Cu(NO_3)_2 + N_2O_2$.

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 2nd, 1877.

Rev. WILLIAM GASKELL, M.A., in the Chair.

“A Case of Flowering of *Chaemerops Fortunei* (Hook) at Alderley,” by ARTHUR W. WATERS, F.G.S.

The fact of *Chaemerops Fortunei* (Hook) flowering so far north as near Manchester seems to me to be of sufficient interest to be worth mentioning to the Society.

This species, indigenous in Japan, is the most hardy of all the palms. I find that several have flowered in the open air in the neighbourhood of London without any protection; but for it to flower so far north as this is probably very uncommon, though I hear it has done so at York.

The plant in question, in my mother's garden at Alderley Edge, has been there now about 25 years, and has never been covered up in winter. It is about 6ft. high, and has always seemed very healthy; but this summer, for the first time, threw out a racemose male flower. The position of our palm is well sheltered from east and north winds. The garden generally is extremely well protected, so that many of the rarer conifers and other trees and shrubs are scarcely to be surpassed for their fine growth in the neighbourhood of Manchester, thus showing that the palm has grown

under specially favourable natural position, and it would be interesting to know if it has or can flower in the neighbourhood in less favoured spots.

The *Chaemerops* have a special interest from having been found fossil in the Miocene of Europe, and even in very high Arctic latitudes.

Besides the *Fortunei*, the *Ch. excelsa* and *Ch. humilis* are common in England in cultivation. The *excelsa* is only half hardy, and requires protection under glass in winter. The *humilis* is a south European species, but flourishes out of doors at Osborne, though it requires protection in the shape of matting in cold weather.

All the *Chaemerops* grow in temperatures comparatively low when compared with that required by other palms. According to Hooker, the *Chaemerops martiana* grows at 8,000ft. in the West Himalayas, where they are annually covered with snow.

Some of the *Chaemerops Fortunei* in Kew Gardens will be about 10 to 12ft., and at the Royal Gardens at Osborne there was in 1860 (Hook) one 10ft. high, which had blossomed for three years running without any protection.

“Table of Effect of Movement of the Surface of the Globe on the Shifting of the Axis of the Earth,” by ARTHUR W. WATERS, F.G.S.

After reading my last paper I examined further the influence of the position of land and seas upon the shifting of the axis of the earth, and prepared the following table for a paper elsewhere.

The present distribution was taken, and to find the influence which this would have the globe was divided into 2,500 divisions, viz., 5 degrees of Longitude by 5 degrees of Latitude, and then the effect of movements in one quadrant was calculated by means of Professor Haughton's corrected formula.

The result obtained was that if any water is added to the earth ab extra then it will be so distributed over the ocean that the effect is the same as if $\frac{1}{12}$ of this amount had been added in the Southern hemisphere from the Equator to the South Pole, along a line passing through 45° 44' East Long. Thus, any movement, such as a submarine elevation which displaces water, would spread it over the oceanic area, and the result, with the present configuration, would be the same as if about one twelfth of the weight had been added in the Southern hemisphere along this line passing through 45° 44' East, which passes over the Caucasus and through Madagascar.

The following table will show the relative effect of elevations on the position of the pole under different conditions. The two last columns are the most important, as showing the different value of equal elevations in the same Latitude North and South of the Equator in any position near the Longitude mentioned which passes through 45° 44' East Long.

Elevation of land (already raised above the sea).		Effect of submarine elevation only.	Submarine elevation, together with effect caused by displacement of water.	
			In the Southern hemisphere, along the line 45° 44' East (or the opposite Northern hemisphere).	In the Northern hemisphere, along the line 45° 44' East (or the opposite Southern hemisphere).
LAT.				
0° to 5°	·086	·051	·076	·026
5 — 10	·26	·16	·18	·13
10 — 15	·47	·28	·30	·25
15 — 20	·66	·40	·43	·37
20 — 25	·84	·50	·53	·48
25 — 30	1·00	·60	·62	·57
30 — 35	1·16	·69	·72	·67
35 — 40	1·25	·75	·77	·72
40 — 45	1·28	·77	·79	·74

The geographical position of the land and sea will be always changing, and with it the locality in which the distribution of the water will act. Thus it does not seem at all probable that the shifting caused by movements in one direction will be balanced by those in the opposite direction,

and all large movements of the crust consist of minor elevations and subsidences.

For latitudes, 45° to 90° , the tables must be reversed.

As an example of a special case the Caspian area was examined, as it is known that great changes have here taken place in geologically recent times, and that the sea was much more extensive and stood at a higher level. Supposing from this district an amount of water, equal to 200ft. in depth, and of twice the extent of the present Caspian has been removed, this would shift the axis the same as if that weight was added in the Southern hemisphere, and would move the Pole about 166ft. from this cause; but the water thus removed would be spread over the globe so as to move the the Pole, the same as if one-twelfth was added along $45^{\circ} 44'$ E. Long. in the S., thus causing in all a movement of 176ft. (this is supposing the distribution of land and sea remained all the time the same as at present). If there were a continent in the South with such a sea as the Caspian, and similar phenomena took place, then the loss of the water would move the Pole about 166ft. in an opposite direction; but the reapportioning of the water would move it about 10ft. in the same direction as before, so that now, instead of being moved 176ft., the Pole would only be shifted about 156ft.

Ordinary Meeting, October 16th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. MACKERETH, F.R.A.S., stated that he had observed an unusual disturbance of his magnetometer at Eccles on the day preceding the great storm of the 15th, and that similar disturbances often occurred immediately before or during great and violent atmospheric changes.

The PRESIDENT exhibited to the meeting some coal measure plants and other organic remains from Spain. He said that Mr. R. C. W. King, of London, had sent him a collection of minerals and fossils to examine. In looking them over he found scales of a *Palæoniscus*, an *Estheria* which his friend Professor T. Rupert Jones, F.R.S., had recognised as *tenella*, and which he (the President) had found in the upper and middle coal measures of Lancashire; *Sigillaria reniformis*, a *Lepidodendron*, and a *Neuropteris*. They were met with in sinking a shaft in search of coal at Puertollano, near Ciudad Real, in the Province of La Mancha.

The following is a section of the strata in descending order:—

No.		Metre. Cent.
1	Vegetable earth	1·35
2	Saponaceous earth	1·35
3	Decomposed slate	1·95
4	Ditto with thin seam of coal	0·05
5	Slate, solid	3·80
6	Thin seam of coal	0·01
7	Slate with fossils.....	0·20
8	Thin seam of coal	0·02
9	Dark-coloured saponaceous earth	0·20
10	Grey sandstone, hard	2·30
11	Ditto, light coloured	0·31
12	Slate with fossils.....	1·50
13	Grey sandstone, light coloured ..	0·02
14	Slate, solid	0·04
15	Grey sandstone, light coloured	0·04
16	Slate, solid	0·04
17	Grey sandstone, light coloured	0·02
18	Slate, solid	1·63
19	Grey sandstone, hard	0·22
20	Slate, solid	1·94
21	Grey sandstone, hard	0·05
22	Decomposed slate with thin coal	0·20

From the character of the fossil organic remains and the nature of the strata he was led to believe that the coal field of Puertollano was of the same geological age as our true English coal measures.

Mr. M. M. PATTISON MUIR, F.R.S.E., exhibited and gave a description of a modified form of Hofmann's Apparatus for Determining Vapour Densities.

"Note on an Edible Clay from New Zealand," by M. M. PATTISON MUIR, F.R.S.E.

I lately received from my friend Mr. R. E. Day, M.A., a small specimen of a clay which is greedily eaten by the sheep in a certain district in New Zealand.

The clay was brought by Mr. Day from Simon's Pass Station, Mackenzie country, South Island. It there forms a range of low bare hills: the sheep (merino sheep) eat very considerable quantities of the clay without appearing to be any the worse for it. So far as Mr. Day could learn the clay eating is confined to this particular part of the Islands. It is supposed by the shepherds that the clay must contain salt, and that it is to supply the deficiency of this article of food that the sheep resort to the earth. The analysis shows that very probably the shepherds are right, although one would suppose that to consume so much silica and alumina for the sake of the small proportion of salt was hardly an economical proceeding on the part of the sheep.

The alkali was determined by Lawrence Smith's process of fusion with calcium carbonate and ammonium chloride. The water was determined by heating in a stream of dry air and direct weighing of the moisture expelled: the organic matter, by noting the difference between the amount of water thus obtained and the total loss suffered on ignition.

Silica	= 61.25
Alumina	= 17.97

Ferric Oxide	=	5·72
Calcium ditto	=	1·91
Magnesium ditto.....	=	0·87
Alkalies* (as Chlorides)...	=	3·69
Organic Matter	=	1·77
Water	=	7·31
		<hr/>
		100·49
		<hr/>

“On the Decomposition of Calcium Sulphate by Alkaline Chlorides. A Contribution to Agricultural Chemistry,” by M. M. PATTISON MUIR, F.R.S.E.

In one of his papers upon Diffusion,† Graham describes experiments which prove that a solution of potassium or sodium sulphate in lime water, when allowed to diffuse into lime water, yields a diffusate containing potassium or sodium hydrate: but that a solution of potassium or sodium chloride is not decomposed by lime water under the same circumstances. Graham further shows that when solutions of calcium sulphate and potassium or sodium chloride are mixed no decomposition ensues at the ordinary temperature, but that on boiling the mixed liquids for some time sulphate of sodium (or of potassium) is found, and continues to exist in the cold solution, inasmuch as if lime water be added two or three days after the boiling has been carried out, and the mixture be allowed to diffuse into pure water, caustic soda or potash is found in the diffusate. Graham suggests that if solutions of calcium sulphate and sodium or potassium chloride be allowed to react upon one another for a considerable period of time at ordinary temperatures, a decomposition might slowly take place analagous with that

* The alkali consisted almost entirely of soda. The clay contained small quantities of phosphoric acid.

† On the application of Liquid Diffusion to produce Decomposition: *Chem. Soc. J.*, III. 60.

which is quickly produced when the mixed solutions are heated. If such a decomposition indeed take place, we shall have, says Graham, a reaction, which, occurring in the soil, may lead to the ultimate production of those alkaline carbonates required by plants for their nutrition. The steps in the process would be these: — decomposition of the alkaline chloride by calcium sulphate with production of alkaline sulphate; decomposition of the alkaline sulphate by lime, added to or originally present in the soil, with production of alkaline hydrate; transformation of the alkaline hydrate into carbonate by the action of carbonates in the soil, or of carbonic acid in the water and air.

The investigations of Graham and others have shown that the two last stages are readily accomplished.

2. In order to determine whether the first stage in the series of transformations is or is not attainable, I mixed solutions of calcium sulphate and sodium chloride, and allowed the liquids to remain at the ordinary temperature of the air (15° to 18°) for several weeks. If decomposition had taken place the liquids would contain calcium sulphate, sodium chloride, calcium chloride, and sodium sulphate: the first of these salts is insoluble in ordinary alcohol: hence the addition of alcohol to the mixture would throw down the whole of the calcium sulphate existing as such, without otherwise influencing the state of equilibrium of the various salts in the liquid. In order to determine the amount of calcium sulphate which had undergone decomposition it was therefore only necessary to wash the precipitate produced by adding alcohol, with dilute spirit, and to ignite and weigh it.

The following are my results:

(a.) 100 cc. of CaSO_4 solution = 191.0 mgms. CaSO_4 mixed with
10 cc. of NaCl solution = 233.3 mgms. NaCl .

Amount of CaSO_4 in solution after 28 days = 167.0 mgms.

Hence amount of Na_2SO_4 found = 25 mgms.

(b.) 100 cc. CaSO_4 solution mixed with 50 cc. NaCl solution
= 1166.5 mgms. NaCl .

Amount of CaSO_4 in solution after 28 days = 103.0 mgrms.

Hence amount of Na_2SO_4 produced = 93.9 mgrms.

(c.) 100 cc. CaSO_4 solution mixed with 100 cc. NaCl solution
= 2333 mgrms. NaCl .

Amount of CaSO_4 in solution after 28 days = 64.0 mgrms.

Hence amount of Na_2SO_4 produced = 132.7 mgrms.

3. It is very evident then that Graham's supposition is correct. Without the aid rendered by diffusion time alone is sufficient to bring about a decomposition of calcium sulphate in solution by sodium chloride, also in solution. If this decomposition is then realised in the Laboratory, there can be little doubt that under the more favourable conditions offered by the soil, the decomposition will play a somewhat important part in the nutrition of plant life.

4. The numbers which I have obtained show that the extent of the decomposition under consideration is influenced not only by the time during which the salts are allowed to remain in contact, but also by the mass of sodium chloride employed. The amount of chemical action is almost directly proportional to the mass of the sodium chloride employed, the amount of calcium sulphate remaining constant.

"On some Thionates," by H. BAKER, Student in the Owens College. Communicated by Professor C. SCHORLEMMER, F.R.S.

Having been lately working on these salts, I offer the following observations:—

Barium Dithionate. $\text{BaS}_2\text{O}_6 \cdot 2\text{Aq}$. According to Heeren* this salt is soluble in 1.1 parts of boiling water; but he does not give the boiling point of the saturated solution; this I have observed to be 102° , and its solubility at this

*Pogg. Ann. 7, 172.

temperature to be 1 in 0.994 parts of water. It is remarkable that the saturated solution of such a soluble salt should boil at so low a temperature. The sp. gr. of the crystals of this salt at 13°.5 is 4.536.

Lead Dithionate. $\text{PbS}_2\text{O}_6\text{4Aq.}$ "Is very easily soluble in water," Watts. I find its solubility at 20°.5 to be 1 in 0.869 parts of water. Its sp. gr. at 11° is 3.259.

Calcium Dithionate. $\text{CaS}_2\text{O}_6\text{4Aq.}$ The crystals have a sp. gr. of 2.176 at 11°.

Nickel Dithionate. $\text{NiS}_2\text{O}_6\text{6Aq.}$ One part of this salt dissolves in 0.897 parts of water at 12°.

Magnesium Dithionate. $\text{MgS}_2\text{O}_6\text{6Aq.}$ According to Watts' Dictionary "forms six-sided tables, very soluble in water;" but Gmelin says it forms ill-defined six-sided prisms.

I obtained it in oblique prisms; and found its solubility at 17° to be 1 in 0.692 parts of water.

Sodium Dithionate. $\text{Na}_2\text{S}_2\text{O}_6\text{2Aq.}$ Its sp. gr. at 11° is 2.175. Watts says, "it crystallizes by spontaneous evaporation in large transparent right-rhombic prisms." Gmelin gives several measurements, but they are not sufficient to calculate all the forms from. My measurements show the crystals to be rhombic, and the axes to be $\bar{a} : \bar{b} : c = 0.9922 : 1.0000 : 0.5981$; the forms occurring are ∞P , $\bar{P}\infty$, P , $\bar{P}\frac{1}{2}$, $\infty \bar{P}\infty$, and the type is long prismatic, through predominance of ∞P . In these and the following angular measurements the interfacial angles are denoted as in "Kopp's Krystallographie," viz. :—

A = the angle of a pyramid, over a vertical edge in the brachy-diagonal.

B = the angle of a pyramid over a vertical edge in the macro-diagonal.

C = the angle of a pyramid over a lateral edge.

W = the acute angle in the vertical prism, and the lateral angle in a dome.

Form.	Angle.	Calculated.	Found.	Gmelin.
P	A	125°45'	125°40'	125°48'
	B	125°18'	125°29'	125°18'
	C	81°11'	————	80°18'
$\bar{P}\frac{1}{2}$	A	88°39'	88°34'	————
	B	137°43'	137°40'	————
∞P	W	89°33'	89°25'	89°22' & 89°36'
$\bar{P}\infty$	W	62° 9'	62° 3'	62°12'

Silver Dithionate— $\text{Ag}_2\text{S}_2\text{O}_6\cdot 2\text{Aq}$.—forms, according to Watts, right-rhombic prisms, isomorphous with sodium dithionate. Gmelin gives some measurements. I find the axes to be $\check{a}:\bar{b}:c=0.9884:1.0:0.5811$, and the forms $\bar{P}\infty$, P, ∞P , $\bar{P}\frac{1}{2}$, $\infty\bar{P}\infty$, $\infty\check{P}\infty$; the type is very short prismatic.

Form.	Angle.	Calculated.	Found.	Gmelin.
P	A	126°47'	126°51'	127°
	B	126° 6'	125°55'	126°
	C	79° 9'	79° 8'	79°10'
$\bar{P}\frac{1}{2}$	A	89°54'	89°58'	90°12'
	C	104°58'	104°52'	————
∞P	W	89°20'	89°22'	89° 8'
$\bar{P}\infty$	W	60°54'	61° 1'	————

Silver Sodium Dithionate— $(\text{AgNa})_2\text{S}_2\text{O}_6\cdot 2\text{Aq}$ —is, according to Watts, “apparently isomorphous with the component salts.” I find it to be rhombic with the axes $\check{a}:\bar{b}:c=0.9813:1.0000:0.5856$, and the forms $\bar{P}\infty$, P, $\infty\check{P}\infty$, ∞P , $\infty\bar{P}\infty$; the dome is developed so much as to make the crystals horizontally prismatic, the prism is only slightly developed, and the pyramid $\bar{P}\frac{1}{2}$ does not occur. Watts also says, it “exhibits very distinct cleavage;” this cleavage plane I find to be ∞P , and I also find that the sodium and the silver dithionates exhibit very distinctly this cleavage.

Form.	Angle.	Calculated.	Found.
P	B	125°30'	125°23'
	C	79°48'	79°46'
∞P	W	88°55'	89° 1'
$\bar{P}\infty$	W	61°39'	61°41'

This sample contains % Ag. 32.0
 $\text{AgNaS}_2\text{O}_6\cdot 2\text{Aq}$ requires % Ag. 33.00

Potassium Trithionate. $K_2S_3O_6$. The following are the statements regarding the crystalline form of this salt:—*Gmelin* gives two (1) “Slender four-sided obliquely truncated prisms (Plessy T. für Pract. Chem. 33, 348),” (2) “Right rhombic prisms with dihedral summits resting on the acute lateral edges (Provostaye N. Ann. Chim. Phys. 3, 354).” According to *Watts* it forms four-sided prisms, bevelled with two faces, and according to *Miller* it crystallizes in the rhombic system, prismatic type with dihedral summits. I obtained it in rhombic needles of ∞P , with the acute edges modified by another prism $\infty \check{P}2$; $\infty \bar{P}\infty$, and $\infty \check{P}\infty$ are also present; they are terminated by a brachy dome. Some of the crystals taper like a sewing needle, I suppose through the presence of a very acute pyramid. I find the axes to be $\check{a} : \bar{b} : c = 0.3586 : 1.0000 : 0.4204$, and the observed forms are ∞P , $\infty \check{P}2$, $\infty \bar{P}\infty$, $\infty \check{P}\infty$, $\bar{P}\infty$.

Form.	Angle.	Found.	Calculated.
∞P	W	39°31'	39°28'
$\infty \check{P}2$	W	71°12'	71°18'
$\check{P}\infty$	W	45°37'	45°37'

This salt is prepared by acting on a saturated solution of Potassium thiosulphate with SO_2 . Now, as it is not stated that sodium thiosulphate will not similarly yield sodium trithionate, nor any reason given why it should not, I attempted to prepare sodium trithionate in this manner, but only obtained crystals of sodium thiosulphate. Rathke (Journ. pr. Chem. 95, 13) prepares sodium trithionate as a white powder by decomposing Potassium trithionate with sodium tartarate.

Ordinary Meeting, October 30th, 1877.

Dr. R. ANGUS SMITH, F.R.S., Vice-President, in the Chair.

Professor OSBORNE REYNOLDS, F.R.S., showed and explained some models illustrative of the manner in which waves enter upon the undisturbed surface of water.

Professor W. BOYD DAWKINS, F.R.S., exhibited and explained the results of the Creswell Caves Exploration of 1876.

Ordinary Meeting, November 13th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

“Note on Envelopes and Singular Solutions,” by Sir JAMES COCKLE, F.R.S., Corresponding Member of the Society.

1. Leibnitz (1694) showed how to find the curve formed by the continuous intersection of an infinity of curves included, on making the parameter vary, in one and the same equation. This gives rise to a new equation by which a value of the parameter is expressed as a function of the coordinates. And this value substituted in the proposed equation gives at once a finite equation for the required curve. Applying this method to the question of finding the curve whereof the normals have a given relation to the portions of the axis of abscissæ intercepted between the origin and the normals, Leibnitz considers this curve as formed by the continuous intersection of an infinity of circles having their centres on the axis. Then the radii of

the circles become the normals to the curve, and the given relation is supposed to exist between the radii and the abscissæ which correspond to the centres of the circles. Taking the relation to be this, viz., that the square of the normal is proportional to the abscissa of the centre, he found the curve of intersection and solved the problem (Lagrange, *Leçons* 1806, pp. 263, 264). But Leibnitz did not remark that his solution does not admit of an arbitrary constant, while it is evident that the problem leads naturally to a differential equation, whereof the integral cannot be completed without the introduction of an arbitrary constant (*ib.* 265). Nor did John Bernoulli, discussing the same problem in his own way, remark that it belongs essentially to the inverse method of tangents, and that, consequently, the general solution depends on an integration which ought necessarily to introduce an arbitrary constant (*ib.* 272). Neither Leibnitz or John Bernoulli seem to have noticed the sort of contradiction which their solutions, arrived at by different methods, offered to the very principles of the differential calculus (*ib.* 274, 275).

2. Taylor (1716) is perhaps the first who deduced a singular solution directly from the derived equation, and who recognised the singularity. He obtained it by differentiating the differential equation (*ib.* 276). Clairaut (1739) was led to a differential equation whereof he obtained two different solutions by means of differentiation (*ib.* 278). He noticed the singularity, and states generally that there are differential equations which have two different solutions, whereof one may be obtained without using the integral calculus (*ib.* 279). Lagrange showed that, in general, every derived equation is susceptible of a form such that the derivee of this form has two factors, whereof one corresponds to the complete primitive, and the other gives at once the singular solution, if there be one (*ib.* 211, 214, 216, et seq., and compare 180.)

3. Euler (Mechanics) found different examples of these duplex solutions, and even gave rules for discovering them in certain cases. Some years after (in his Memoir of 1756) he expressly occupied himself with the subject (*ib.* 282). He was content to exhibit this duplicity of solutions as a paradox; inasmuch as the equation which contains an arbitrary constant, and which we ought therefore to regard as the complete integral, does not include the other finite equation. This last is, however, equally a solution of the differential equation, contrary, as it seemed, to the principles of the calculus (*ib.* 287).

4. It remained to discover the connection between these singular solutions and the complete integrals, as well as between the curves represented by the one and the other, and to refer the whole theory of these different solutions to the first principles of the calculus. This was done by Lagrange (*ib.* 290; 178).

5. It is the characteristic of singular solutions that they appertain to the curves formed by the continuous intersection of the curves represented by the complete primitive, when the arbitrary constant, a particular value of which distinguishes a particular primitive, is made to vary continuously (*ib.* 268, 269).

BRISBANE, QUEENSLAND, AUSTRALIA,
SEPTEMBER 7, 1877.

On the Formation of Hailstones, Raindrops, and Snowflakes, by Prof. O. REYNOLDS, F.R.S.

My present communication forms a continuation of the paper I read before this Society on the 31st of October, 1876, "On the manner in which Raindrops and Hailstones are formed."

To the contents of this paper I shall have to refer continually—hence in order to render what I have to say intelligible it may be well for me to recapitulate some of the leading points in my former paper. The chief purpose of

the paper was to explain the manner in which the minute cloud particles aggregated so as to form raindrops and hailstones.

Aggregation resulting from the more rapid descent of the larger particles.

I commenced by pointing out that as the suspended particles of water or ice which constitute a cloud are all descending with velocities which increase with their size, the larger particles will descend faster than the others, and will consequently overtake those immediately beneath them; with these they will combine so as to form still larger particles which will move with greater velocity and more quickly overtaking the particles in front of them, will add to their size at an increasing rate—and I then proceeded to consider how far this was a sufficient, as well as a necessary, cause of the phenomena of hail and rain. One of the most important points on which my arguments were based was

The Shape and Structure of ordinary Hailstones.

On close observation I had found what had previously been noticed by other observers, that the shape of an ordinary hailstone is not what at first sight it appears to be. They are not spheres more or less imperfect, but more or less imperfect cones or pyramids with rounded bases like the sections of spheres; the conical surface being striated, the striæ radiating from the vertex of the cone.

In texture the hailstones have the appearance of being an aggregation of minute particles of ice fitting closely together but without any crystallization, such as that seen in the snow-flake, while, on careful observation, it is seen that they are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have been broken off in their descent.

As I explained, it seemed to me that this form and structure was exactly such as would result from the manner of

aggregation, which I had discovered when a particle which ultimately forms the vertex of the cone starts on its downward career and encounters other particles; these adhere to its lower face. The mass, therefore, grows in thickness downwards, and as some of the particles strike the face so close to the edge that they overhang, the lower face continually grows broader, and a conical form is given to the mass above.

When a particle first starts it moves slowly, and the force with which it meets the other particles is slight, and consequently its texture is loose, but as it increases in size and velocity it strikes the particles which it overtakes with greater force and so drives them into a more compact mass.

Assuming that the temperature at which hailstones are formed is not greatly below 32° the particles must actually freeze together. For the effect of squeezing two pieces of ice together at or near the temperature of 32° is to cause them to thaw at those points where the pressure is greatest, at which points they freeze again as soon as the pressure is removed.

In illustration of the force with which the particles strike the face of the hailstone, I instanced the action of the particles of sand in Mr. Tilghman's sand blast used for cutting glass and other hard materials.

I also reverted to the possibility of making

Artificial Hailstones,

by blowing a stream of frozen fog against a small object, making as it were the cloud to rise up and meet the stone instead of the stone falling through the cloud.

I had not, however, then overcome the difficulty of obtaining such a stream of frozen fog, but I gave two sketches of plaster stones, which as far as their shape and the striated appearance of their surface were concerned, closely resembled hailstones, and which plaster stones had

been obtained by blowing some finely divided plaster of Paris against small splinters of wood by means of a jet of steam.

In the discussion which followed my paper, Dr. Crompton suggested

The Ether Spray,

such as is used in surgery as a means of obtaining a frozen fog. And shortly after the meeting I tried this ether spray, using an instrument such as surgeons use. But although I found that the spray would freeze anything such as a small tube of water, I could get no deposit of ice particles on the outside of any object. I varied the form of the apparatus, but with no better success, and for the time I abandoned the attempt.

What the cause of this failure was I do not precisely know, but I attribute it to some excess of alcohol in the ether then used, which was not methylated ether. That this might have been the cause occurred to me about two months ago. I then determined to try again, and combine a spray of water with that of ether. I now obtained the lightest ether which Messrs. Mottershead & Co. could supply. The specific gravity of this was $\cdot 717$, and it was made from methylated spirit.

With this, somewhat to my surprise, I at once obtained a deposit of ice even without the water spray, and with the same apparatus I had previously used. It was not, however, until I used the combined spray of water and ether that I obtained anything resembling a hailstone in appearance. But the first time I used this combination I obtained a small but well-shaped hailstone on the end of a match, which I held pointed towards the spray.

The next time I tried, however, on another day, I did not succeed so well with the water as without it.

When using the water spray the deposit of ice was wet or half melted, while without the water I obtained a hail-

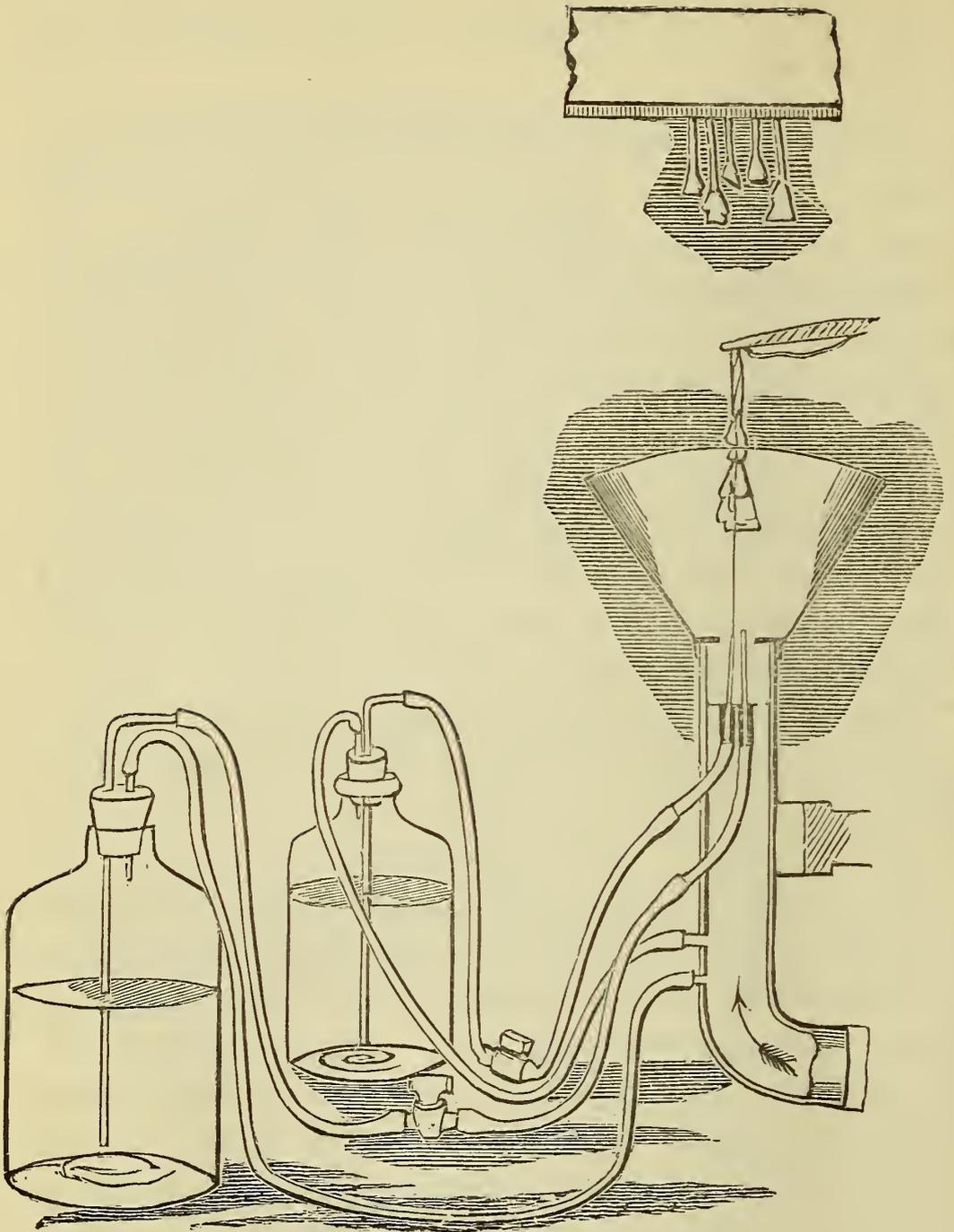
stone in much the same manner as I had obtained before with the water.

This difference in the results on the two occasions was at once explained by the different states of the air, for on the first occasion it had been cold and dry, whereas on the second it was warm and saturated. With the dry air the ether spray reduced the temperature so far below 32° that the particles of ice did not freeze together—the force of impact was not sufficient to cause them to thaw in the first instance, and hence the water spray was necessary to keep this temperature from falling too low—whereas with the warm saturated air the ether did not reduce the temperature of the air and the vapour it contained much below 32° , and consequently when the water spray was added the water was only partially frozen.

I subsequently improved the apparatus so as to be able to regulate the supply of water and ether to the condition of the air.

The Apparatus.

This is shown in the accompanying sketch. It consists of a brass tube half an inch in diameter, one end of which is connected with bellows capable of maintaining a constant pressure of about 18 inches of water, on the other end of the tube is a cap over the end of which is a flat plate or diaphragm having a central opening $\frac{1}{8}$ of an inch in diameter which forms the aperture for the blast. Entering through the sides of the main brass tube are two small brass tubes which reach to within $\frac{1}{2}$ an inch of the plate and into the ends of which are sealed fine glass capillary tubes, the glass being very thin; these protrude just through the middle of the aperture, the one about $\frac{1}{16}$ of an inch and the other $\frac{1}{32}$. Through these tubes the water and ether are separately introduced into the blast to form the spray, and it is mainly on the adjustment of these tubes that the efficiency of the apparatus depends. It is essential that



the ether tube should be slightly the longest, otherwise the ends become stopped with ice, and I find it better that the ether tube should be somewhat larger than the water tube. The bore of the tubes must be very small, but this is not sufficient, for unless the glass is very thin the spray will not be finely divided. Both the ether and water are forced through the tubes from bottles by connecting the interiors of these bottles with the bellows, and the quantities of ether

and water are regulated either by raising and lowering the bottles or by means of the cocks in the pipes.

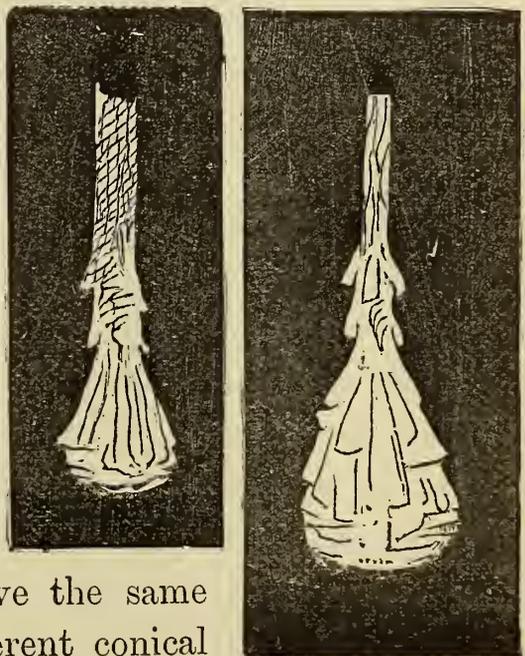
The tube is fixed in an ordinary retort-stand, so that the blast is vertical. If then a small splinter of wood is held downwards pointing into the spray, a lump of ice forms on the end of the splinter, and this lump has all the appearance of the hailstones. It is quite white and opaque, it is conical in form and has a rounded base and striated surface.

In this way I have formed stones from half to three-quarters of an inch in diameter. When, however, the stones are growing large it is necessary to move this splinter so as to expose in succession all parts of the face of the stone to the more direct action of the spray.

When using this apparatus in a warm room I have found it best to fix a pad of blotting paper over the jet at a height of 10 or 12 inches. The surface of this pad is cooled by the spray and prevents radiation from the ceiling, which otherwise tends to melt the top of the stone. For a similar reason I have found it well to surround the blast with a wide cylinder or inverted cone of paper, which keeps off radiation without interfering with the action of the jet.

By sticking several splinters of wood pointing downwards into the pad, a number of stones may be made at once.

In the accompanying sketch a medium sized stone as well as one of the largest stones are shown attached to the splinters of wood. The surface of the cone, where continuous, is truly conical or rather pyramidal, but this surface is broken, as it were by steps, and a very marked fact is that all the continuous surfaces have the same vertex, and hence the different conical



surfaces to which they belong, have not the same vertical angle, the surface being exactly such as would be acquired by the fragments of a sphere so constituted that the fracture tended to follow radial lines.

Owing to the radiation of the surfaces from a common vertex and the steps which occur between the vertex and the base, the angle of the conical surface of the stone is greater near the vertex than near the base. Thus the smaller stones appear less elongated than those which are larger.

The fact that in the sketches of actual stones, which I gave in my last paper, I showed the steps as less pronounced and the angles larger than they are in the artificial stones, is probably owing in some measure to my having formed my ideas from the observation of favourable specimens chosen from amongst those which fell. The larger angles were probably also, in part, owing to the smaller size of the actual hailstones, which were not much more than one fourth of an inch across. But I think that it is important to notice that the somewhat imperfect way in which the outside layers in the surface of the artificial stones are continued, may be owing to the narrowness of the jet of air which, on striking the stone, tends to diverge laterally rather than to flow upwards past the sides of the stone, as it would do if the jet were broader, or as the air must do when the stone is falling through it.

The rate at which stones can be formed depends on the amount of water which can be introduced into the spray, the larger stones taking from one to two minutes. At first sight this may seem to be somewhat slow, but the following estimate tends to show that the artificial are probably formed quicker than the actual stones.

The speed of the jet of air at the point at which the stones are formed is nearly equal to that at which the larger stones would fall through the air. This is shown by

the fact that if a large stone becomes accidentally detached from its splinter of wood it rather falls than rises, but when this happens with smaller stones they are driven up by the force of the blast.

I find that the speed of the blast varies from 150 to 200 feet per second, *i.e.*, from one to two miles a minute. The larger stones, therefore, traverse from one to three miles of frozen spray. So that if we imagine a cloud as dense as the spray, it would have to be from one to three miles thick in order that the stones might, in falling through it, attain the size of the artificial stones, and considering that the stones would only gradually acquire a speed equal to that of the blast, the time occupied in falling through the cloud would in all probability be very considerable, at least from five to ten minutes after the stone had acquired a sensible size.

As regards the proportion which the density of spray bears to that of a cloud, a comparison may be made from the fact that when working in saturated air at a temperature of 60° or 70° F., the condensation of vapour supplied sufficient ice to form the spray; and since it is probable that the dense summer clouds, from which hail is formed, result from the cooling of air from temperatures nearly if not quite equal to this, there is probably no great difference in the density of the clouds and the spray. I shall now endeavour to put the apparatus in operation, and exhibit the production of some of these artificial stones.

Snow Crystals.

I have not yet had an opportunity of examining the texture of these artificial stones under the microscope, but to all appearance they consist of an aggregation of small spherical particles of ice; and it seems worthy of notice, that while nothing like a grown crystal appears ever to be produced in the ether spray, the moment the blast is stopped the end of the ether tube becomes covered with ice, which often assumes the form of snow crystals.

This appears to indicate the character of the difference between those conditions which result in snow and those which result in hail.

When the cloud-particles are formed at or above the temperature of 32° , and then freezes, owing to cooling by expansion or otherwise, the particles as they freeze retain their spherical form. This is what happened in the spray.

On the other hand, when saturated air at a temperature below 32° is still further cooled, the deposition of the vapour will be upon ice, and will take the form of snow crystals.

The aggregation of the snow crystals into flakes is, as I pointed out in my previous paper, accounted for by the larger crystals overtaking the smaller crystals in their descent, and the still more rapid descent of the flakes as they increase in size.

As regards the formation of rain-drops, I have nothing to add to what was contained in my last paper. The same explanation obviously applies to both hail and rain; and any doubt which may have been left by the less direct arguments in my former paper will, I venture to think, have been removed by the verification of my predictions in the production of artificial hailstones so closely resembling in all particulars those formed by nature. And, in conclusion, I would thank Dr. Crompton for the suggestion of the means by which I have been able to produce these stones.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 8th, 1877.

Prof. BOYD DAWKINS, F.R.S., President of the Section,
in the Chair.

The President gave an address in which he brought before the notice of the Section the three discoveries in Palæontology which stand out beyond the rest in the record of the advance of knowledge in the last session. The evidence offered by the Creswell caves as to the progress of man in the palæolithic age, the full proof by Prof. Marsh of the intermediate position of the cretaceous birds between the reptiles on the one hand and living struthious birds on the other, and the filling in of the gap in the pedigree of the European horse by Dr. Forsyth Major's discovery of *equus stenorus* in the pleiocene of Italy.

He also touched on the problem of evolution, and held that the argument against that doctrine based on the persistence of simple forms such as the invertebrates generally was worthless, because simple structure implies simple needs and simple environment. The change has ever been simplest in the most highly organised. The argument founded on our inability to tell the how or the why also is worthless, because we know the how or why of scarcely anything around us. And lastly that based on the suddenness of the appearance of new forms, as for example the Tertiary flora and Tertiary mammalia was equally worthless, because it depended on the perfection of the geological record and on the absurd idea that we are now in possession of every part of the record. How imperfect the record is may be gathered from the fact that since the Meiocene age the

mountains of the west of Scotland have lost $\frac{1}{2}$ of their height. The general conclusion at which he arrived was that evolution is a good working hypothesis, apart from the consideration of the how and the why, and that it is sufficient to prove that certain forms have been evolved from others unlike them, such as the horse from the hipparion, without being able to do it in all cases. In conclusion, the attention of the Section was called to the very unsatisfactory condition of museums in this country, and to the equally unsatisfactory position of natural history in the general scheme of education. The President looked forward to the time when there will be a museum in Manchester which will be a centre around which all the Mechanics' Institutions and natural history societies in the district will group themselves. To hasten this it will be necessary for every individual to use his efforts to influence those immediately around him.

Mr. HURST exhibited a collection of Mediterranean marine algæ; principally from the island of Rhodes, and from the neighbourhood of Alexandria: and a large specimen of an Egyptian cotton plant, from the Ashmouni districts.

Mr. WATERS, F.G.S., exhibited some microscopical sections of Bryozoa, and explained their anatomy: especially dwelling upon the genus *Flustra*, or horn wracks.

General Meeting, November 27th, 1877.

Dr. R. ANGUS SMITH, F.R.S., Vice-President, in the Chair.

Mr. Wilfred Becker, B.A., of Withington Road, Whalley Range, and Mr. Samuel Moore, of Bamford, Derbyshire, were elected Ordinary Members of the Society.

Ordinary Meeting, November 27th, 1877.

Dr. R. ANGUS SMITH, F.R.S., Vice-President, in the Chair.

Mr. G. S. WOOLLEY presented to the Society a photographic portrait of Dr. Dalton, enlarged from a Daguerreotype taken by the late Mr. John Parry about the year 1841; and also a photograph of the first prospectus of Dalton's School.

On the motion of Mr. BAILEY, seconded by the Rev. WM. GASKELL, the thanks of the Society were unanimously voted to Mr. Woolley for his valuable donations.

“On the Origin of some Ores of Copper” (Part I.), by CHARLES A. BURGHARDT, Ph.D., The Owens College.

To endeavour to trace the various ores of copper to one fundamental source, and to study the relationships probably existing between the copper compound and its accompanying minerals, has appeared to me a subject of great interest, in fact, one which has engaged my attention for some time. The commonest occurring ore of copper is undoubtedly

cuprite (Cu_2O) either independently, or more often associated with malachite, azurite, brown iron ore, and native copper. Scarcely a specimen of copper ore can be obtained (with the exception of the sulphides) which does not exhibit more or less cuprite in intimate intermixture with the other ore; whence it must be inferred that all, or almost all the ores of copper have been formed by aqueous and not by plutonic action, as cuprous oxide is undoubtedly prepared with greater facility by the wet method (that is from cuprous or cupric solutions) at a comparatively low temperature than by the dry method at a very high temperature. I will briefly state in which way cuprite (that is the crystallised cuprous oxide) has been obtained by different authors, firstly by the dry method and secondly by the wet method.

PREPARATION OF CUPRITE.

Dry Methods of Preparation of Cuprite.

1. A mixture of 24 parts of cupric sulphate and 29 parts of copper turnings heated to redness in a well closed crucible furnishes eventually a reddish-brown crystalline mass which is cuprite.

2. A mixture of 100 parts of cupric sulphate and 57 parts of soda crystals is heated until all the water of crystallisation is driven out, the residue powdered finely, and 37 parts of copper turnings are added, and the whole mixture heated in a crucible to a white heat for half an hour. On washing out the cooled mass with water, cuprite of a very fine colour is obtained. (Wöhler and Liebig, Pogg. Ann. XXI. 581.)

3. Crystals of this substance are often observed in the slags obtained in the smelting of copper ores.

It will readily be observed from the above that an intense heat is requisite for the formation of cuprite by the dry method.

Wet Methods of Preparation of Cuprite.

1. A solution of equal parts of cupric sulphate and grape sugar is mixed with sufficient caustic soda solution to dissolve all the resulting precipitate and then gently heated, cuprous oxide then separates out in the form of a crystalline powder. (Mitscherlich Journal Pract. Chem. XIX. 430.)

2. Bequerel (Compt. Rend. XLIV. 308) describes a beautiful method which is applicable for the preparation of many minerals. He filled a test tube with a neutral solution of cupric nitrate, placing a little cupric oxide and a clean strip of copper plate at the bottom, closed the tube airtight and left it to itself for many months. In this way he succeeded in obtaining small shining cubes of cuprite.

3. A. Knop (Jahrb. f. Min. 1861, 508) states that if a mixture of solutions of cupric sulphate and ferrous sulphate be treated by an alkaline carbonate, carbonic acid gas is evolved and a mixture of ferric hydrate and hydrated cuprous oxide is precipitated, the latter becoming crystalline after the expiration of some time.

Of the three methods given above that of Knop undoubtedly furnishes one of the most probable processes by which many large deposits of the so-called tile ore and copper pitchblende have arisen; but there are again many other deposits of cuprite which may easily have been formed in other ways. I will not discuss at the present time the various opinions upon the formation of copper ores expressed by some very eminent and competent authorities, but I may mention that Dr. Ferdinand Wiebel, in a lengthy and exhaustive treatise (*Das Gediegen Kupfer und Roth Kupfererz*, Hamburg, 1864) concludes that the native metal and cuprite have in *all* cases been formed from a cupric sulphate solution (obtained by the oxidation of copper pyrites) through its reduction by a solution of ferrous sulphate (also derived from the oxidation of copper pyrites). This statement is doubtless correct in the case of some

formations, but there are many others where it is questionable. Gustav Bischoff points out in his "Chemical and Physical Geology" that "It is certain that the contents of a lode are of later date than the adjoining rock, so that it can be shown that these contents originate from the adjoining rock, and if it can be ascertained what compounds of the metal exist in that rock, the previously existing minerals may be distinguished from those of later date; but the determination of this point is attended with great difficulties and is generally impossible." It is a well ascertained fact that all the ores of copper are found mostly in crystalline rocks or metamorphic rocks derived from them (some ores however being found in newer formations); and Struve (Ueber die Nachbildung der naturlichen Heilquellen, Heft 2, 17) has proved in the most conclusive manner that granite, porphyry, phonolite, gneiss, basalt, clay slate, trachyte, &c., contained a more or less appreciable amount of sodium chloride, and it is also well known that nearly every spring water flowing from rocks contains sodium chloride or a chloride of one of the alkaline earths (magnesium chloride). Further, it has been shown by Delesse (Jahrb. f. Min. 1862, 605) that all rocks are *saturated* with water, and it is also a well ascertained fact that iron pyrites (FeS_2) is disseminated throughout *all* crystalline rocks. Having these facts before me, I made an experiment in order to ascertain what reactions would occur on heating iron pyrites and a solution of cupric chloride together in a sealed tube at a moderately high temperature. Small pieces of pure iron pyrites were placed in a glass tube, then covered with a moderately strong solution of pure cupric chloride, and the tube sealed up and heated for 14 days at a temperature varying from 135° — 210° C., but the greater part of the time the temperature oscillated between 150° and 170° . On the seventh day the colour of the cupric chloride had become considerably lighter, and a small deposit of violet-red

crystalline cuprite was observed adhering to the sides of the glass tube, whilst on allowing the tube to cool it was observed that a very considerable amount of cuprous chloride had crystallised out in beautiful transparent colourless tetrahedrons. On the 14th day the tube was opened, as no further changes had taken place, and the contents examined and found to consist principally of cuprous chloride, with a small quantity of unaltered cupric chloride; there being at the same time a good amount of ferric sulphate and cupric sulphate present. The deposit on the glass tube was cuprite. I was unable at the time to make any quantitative determination of the various substances formed in this reaction, and am therefore not prepared to say what the reaction was precisely. Of course the iron pyrites was considerably attacked, but there was no separation of free sulphur; free hydrochloric acid was also present, but not in sufficient quantity to dissolve the small quantity of cuprite formed on the tube. The insoluble crystallised cuprous chloride was carefully washed in distilled water in order to free it from the substances already mentioned, then placed in another glass tube, covered with distilled water, and the tube sealed up and heated for seven days in an air-bath at temperatures varying from 160° — 180° . On the second day a very marked amount of a bright-red substance had already formed on the sides of the tube, and here and there minute green spots of a substance somewhat resembling atacamite ($\text{CuCl}_2 \cdot 3\text{CuHO} + \chi\text{H}_2\text{O}$) in colour. This tube was heated at the temperatures given for about two weeks, when it was observed that although the red deposit did not increase in quantity, the cuprous chloride was slowly undergoing decomposition, a black powder having separated out. On opening the tube a smell of hydrochloric acid was perceived. The liquid portion in the tube was poured out, and the solid portion (which principally consisted of unaltered

cuprous chloride crystals with a little of the dark-coloured powder) well washed with water. By levigation it was easy to separate the powder from the crystals and to submit it to a further examination. In order to free it completely from cuprous chloride it was heated in a beaker with a solution of ammonium sulphate in water, which dissolved out all the cuprous chloride and left the powder intact; and the latter, on being dissolved in nitric acid, did not give any reaction for chlorine, whilst the presence of copper was very evident: hence there is scarcely any doubt about this substance being cupric oxide. The liquid portion contained free hydrochloric acid and cupric chloride.

When the red deposit first commenced to form upon the sides of the tube a small quantity of violet-red crystalline cuprite was undoubtedly also present, but disappeared on the experiment being continued. I dissolved the red deposit in nitric acid and detected the presence of a large quantity of chlorine in the solution, also that of copper. Therefore I conclude that this red substance is the so-called cuprous oxychloride described by Wöhler (*Ann. Chem. Pharm.* CXXX. 376). On exposing the substance to the air it takes up oxygen, and acquires a beautiful apple-green colour, becoming a cupric oxychloride which has probably the same composition as Atacamite.

Chalcotrichite.—Being desirous of ascertaining what the action of a moderately strong solution of sodium chloride in water would be upon pure artificially-prepared cuprous oxide when heated with it in a sealed tube, I employed the following method, viz.: The tube was heated in an air-bath for five days at a temperature varying from 150°—180°, when small traces of a green substance (probably atacamite) were observed, the cuprous oxide had become dark-brown in colour, and on the sides of the tube there were small beautiful radiating tufts of an orange-red coloured substance closely resembling the prismatic variety of cuprite called

chalcotrichite. Continuing to heat the tube, on the eighteenth day it was opened, when it was found that no further formation of the prismatic substance had taken place, but there was a beautiful deposit of highly crystalline violet-red coloured cuprite on the sides of the tube. The fibrous substance was dissolved in nitric acid (after having been boiled in distilled water for several hours and carefully washed) and tested for chlorine, which was found to be entirely absent, whilst copper was present in large amount: hence there can be no doubt that this prismatic substance was the rare mineral chalcotrichite. From this experiment it is evident that cuprous chloride was formed at first by the reaction between the sodium chloride and cuprous oxide, and this cuprous chloride was decomposed into cuprous oxide again and deposited on the sides of the tube as above described. I shall refer to the importance of the occurrence of chalcotrichite in conjunction with cuprite when I come to consider the origin of all the copper ores in a future paper, and will therefore describe another series of experiments having for their object the direct formation of malachite.

Malachite.—Great difficulty was experienced in carrying out these experiments for any length of time, owing to the frequent bursting of the tubes, due, no doubt, to their being somewhat weakened by the formation of silicates upon the surface of the glass. Pure artificially prepared cuprous oxide was placed in a glass tube and covered with distilled water, saturated with perfectly pure carbonic acid gas, and then sealed up and heated in the air-bath at a temperature varying from 150° — 175° . After heating fifteen days, the tube was opened and allowed to stand exposed to the air, when a thin film of malachite was observed coating the small clumps of suboxide here and there. The action of oxygen was found to be absolutely essential in the production of malachite by this method, there being no sign of any such

formation in the tube before it was opened. Thus there can be no doubt that the greater part of the malachite formations have been produced by the action of water containing carbonic acid gas in solution upon cuprous oxide, as the malachite of the Gumeschewskoi Mine, near Ekaterinenburg, in Russia, is found deposited in a dark-coloured clay which penetrates a small limestone ridge, the whole formation finally resting upon chloritic slate. I have examined numerous specimens of malachite from all parts of the world, and have always observed strong evidence of the action of water upon them, there being numerous cavities always present, which exhibit deposits of cuprous oxide, or hydrated ferric oxide in their interiors, and occasionally amorphous silica and calcite. Gustav Rose (*Mineralogische-geognostische Reise nach dem Ural, dem Altai und dem Kaspischen Meere*, 1837), was of opinion after examining the copper formations in the Ural and Altai districts that the cuprite there was formed by direct oxydation of the metal; and, secondly, that the large malachite deposits were formed from the cuprite. This opinion probably well expresses the actual process which eventually resulted in the formation of malachite, as the presence of the clay surrounding the ore, the limestone formation and the hydrated ferric oxide and hornstone (amorphous silica) shows that the former neighbouring crystalline rocks must have been submitted to the powerful action of water, which in all probability contained carbonic acid gas. Rose does not give (so far as I am aware) any experimental proofs of his theory of the formation of the great copper deposits of the districts above mentioned. I considered it therefore necessary to endeavour to determine experimentally the accuracy of his statement. For this purpose I placed perfectly clean polished strips of chemically-pure copper sheet in a glass tube, and covered them with distilled water saturated with carbonic acid gas; the tube was then sealed up and heated in a water-bath at 100° for

some time. In three days I observed a decided formation of a film of cuprous oxide (which was crystalline) upon the metal; on the seventh day there were small particles here and there of a green substance, which was undoubtedly a basic copper carbonate—very probably malachite. On continuing the heating of this tube for a few days longer at the same temperature, a considerable amount of cupric oxide was formed in scales, arising, doubtless, from the decomposition of the malachite, as it is a well-known fact that malachite on being boiled in water, decomposes into cupric oxide, carbonic acid gas, and water. In order to see what effect pure distilled water would have upon metallic copper under similar circumstances, I placed a large strip of clean polished sheet-copper in a tube, covered it with distilled water and sealed the tube up, heating it in the water-bath as in the previous experiment. I was astonished to find on the third day that the copper was strongly coated with *cupric* oxide, so that it is evident that pure water has a greater chemical action as an oxidising agent than water containing carbonic acid gas. Having thus obtained the results described, I may be allowed to draw some conclusions from them, as regards the primary copper ore, and the eventual formation of the other ores from it, briefly as follows, viz. :—

1. In all probability the crystalline rocks contain disseminated throughout their mass extremely minute quantities of metallic copper (*when perfectly fresh*); Bischoff mentioning many rocks in which small quantities of cupric oxide have been detected, where its presence would never have been expected.

2. By the action of water, or a solution of carbonic acid gas in water, the metallic copper particles were converted *in situ* into one of the three following substances, viz., cuprous oxide, cupric oxide, and malachite.

3. By the action of water partially charged with sodium chloride (derived from the surrounding rock masses),

cuprous chloride and cupric chloride were formed and carried down into the fissures below, where, by the action of a heat which need not have exceeded 160° , the *cuprous chloride* (for the cupric chloride would be completely reduced to cuprous chloride by the action of the iron pyrites which is universally present in all crystalline and metamorphic rocks) was decomposed into cuprite and cupric oxide and deposited in the fissures or lodes. The ferric sulphate simultaneously formed in the reduction of the cupric chloride would eventually decompose into hydrated ferric acid and accompany the cuprite in the lodes, whilst the free hydrochloric acid formed in this process would readily attack the neighbouring rocks, thus causing the deposition of gelatinous silica in the lodes.

It will now be my next endeavour to study the chemical composition of all the rocks occurring in the immediate neighbourhood of copper mines, and particularly the evidences of decomposition of these rocks at the place of contact with the metalliferous lode, in order to ascertain whether the above conclusions are correct. In a future communication I hope to deal with the statement made by great authorities on this subject, that the sulphides of copper are the primary and oldest compounds of that metal.

“On the Construction of a Room or Series of Rooms free from Germ Life, proposed for use in the performance of Surgical Operations,” illustrated by a Model Room and Apparatus, by WILLIAM THOMSON, F.R.S.E.

My object in bringing this paper before the Society is to show what I consider to be a valuable application of a well-known principle, viz.: that of filtering from the ordinary air the innumerable spores which are constantly found floating about in it, and so to arrange a room or series of rooms, in which the air may be rendered optically pure. The practicability of producing, and afterwards retaining

in a room or rooms of ordinary dimensions, an optically pure atmosphere, was suggested to me by the results of a large number of experiments which I have carried on during the last seven or eight years, but more especially the results obtained from a series of experiments which I commenced about the beginning of this year, on the cultivation of different fungi, the spores of most of which are found in large numbers, floating about in our atmosphere.

Flour paste suggested itself as the best soil I could obtain on which to grow the different fungi. When flour paste is exposed to the air for a few days, as is well known, fungus makes its appearance, and soon completely covers its surface, and if a number of flour pastes be made and exposed in different places, it will be found that different kinds of fungus spring up, in each showing a splendid variety of different colours, and shades of the same colour. It was with a view to isolate and study these different fungi that one of my last series of experiments was commenced, and it became necessary to look for a means of sowing the seeds of each fungus on a soil which should originally be free from spore life, and afterwards should be effectually protected from the falling of other spores from the air on to the soil, and further, should, if possible, give a free and open supply of air to the microscopic plants.

After a few experiments I found that this could be done effectually by placing flour paste in an ordinary glass shade turned upside down, covering the mouth of the shade with a thin layer of cotton wool placed between two pieces of cloth, the three layers being fixed round the edges of the glass by an elastic band. The paste, when boiled in the vessel thus closed, was afterwards found to remain free from fungus life for any length of time. Having thus been successful by such a rough arrangement in preventing the entrance of fungus spores, I next arranged to try the same experiment on a rougher and larger scale, and consequently

had the rough model made which I show here this evening. Each side of this box or room is made of thin layers of cotton wool sewn between two pieces of cloth, with a glass in front, which serves equally as door and window. A conical-shaped piece made of stout paper is pasted to a square frame at its base, which fits against one end of the room. An ordinary glass funnel is placed in this cone with its stem passing through the apex which is firmly tied round it, and an india-rubber tube communicating with a pair of bellows is put on the end of the stem; the large end of the funnel is covered with a thin layer of cotton wool placed between two pieces of cloth, and the whole fixed round the funnel edge by means of an elastic band. When the window at the other end of the room is opened and the bellows worked, a current of air passes through a layer of cotton wool placed between two pieces of cloth on the mouth of the funnel and then through that at the end of the room, moves bodily along the room and escapes at the window, little or no special currents or draughts being apparently produced. This I observed by previously filling the room with fumes. The force of the air, by passing through the two layers of cotton wool, was evidently equally distributed throughout the whole area of the room. My first experiment with this arrangement was made by simply working the bellows for about twenty minutes to endeavour to drive the impure air, with its floating particles and spores, out of the room by passing into it the filtered air from the bellows; then boiling one-half of some flour paste in a basin inside the room for a minute or two, and lastly, closing the window with the glass, the other half of the paste being boiled in a similar basin and left outside exposed to the ordinary or contaminated air. After a few days the difference between the two pastes was very marked, the surface of the one left outside the room was completely covered with fungus, whereas, only 4 or 5 little specks of

fungus were observed to be growing on the surface of the one inside the house, which soon developed into 4 or 5 little buttons of fungus. I considered that the spores which had fallen on this flour paste must have been sticking to the sides or roof of the room, and to eliminate this assumed cause of error, I drew out the supporting rods of the room and crushed it up and boiled it in water for half an hour, then allowed it to dry and repeated the same experiments with the two basins of flour paste, the bellows being first worked and the glass then taken out and laid down in the ordinary or contaminated air, then wiped with a clean cloth and put in. In a few days afterwards, the paste outside the house was completely covered with fungus, whilst the one inside was absolutely free from it, and it remained free for nearly a month, when one little speck was observed on it which soon developed into a little button of fungus. This showed that for three weeks after the paste was placed in the room no spore had fallen on it. The only cause of failure I could see was the glass, upon which some spores must have adhered, consequently I had a bag made and sewn on above the window, into which the glass could be pulled in opening the window. When this was done the house was again crushed up and boiled in water, again placed in position, the paste boiled, the window closed, and in this third trial the experiment was perfect. No fungus was developed on the paste inside the house, and after some months it dried up to a hard mass. Thus these thin sheets of cotton wool forming a room of 10,032 cubic inches capacity, *i.e.*, capable of holding 36 gallons of air, was sufficient to prevent the entrance of spores into it, and thus to preserve the flour paste from decomposition during several months, till it had completely dried up.

These results, I think, conclusively shew that ordinary large rooms may be constructed and ventilated with filtered air, by means of fans, so that flour paste, taken as a test

standard, would remain in them free from fungus life, and I believe that such a room or series of rooms might be of great advantage in surgery, perhaps as a means of preventing spores from entering the wounds of patients, and so doing away with or lessening the onus put upon the antiseptic treatment, or in giving a better chance of success in serious surgical operations. It is, I understand, believed by many medical men that pus and blood poisoning may be generated in a wound, from the blood corpuscles or bioplasts in the body, and that such developments have nothing to do with the germs of the outside air, and that the antiseptic treatment is employed to assist their development, but no one, so far as I know, has ever proved this beyond doubt, and such an arrangement of a room or rooms as I shall suggest further on would, I believe, be one mode of experimenting, whereby the whole or a great part of a serious obstacle to such researches may be eliminated—but in passing, I should like to mention one slight incident in reference to the development of pus in wounds which would tend to show that certain germs floating in the air are capable either directly or indirectly of developing pus. Some years ago, when working with a solution of albumen, which had been exposed to the air for some time, and which was then putrid, and swarming with bacteriæ, I accidentally cut my finger, and some of the putrid liquid entered the wound. I washed it out as well as possible, and thought nothing more of it. Next morning it was slightly swollen, and had gathered. I opened it with a knife, and microscopically examined the matter, expecting to find bacteriæ, but to my surprise no bacteriæ could be observed, and all that could be seen was simply pus cells.

The arrangements which I should suggest on the large scale are, a long room or series of rooms in a line, at one end of which should be fitted a fan behind a good filter of cotton wool, a long pipe with a series of bunsen burners set

along it, so that when all were lit a line or sheet of flame would be produced which might be gently passed along and made to play on every part of the floor, walls, and roof of the room, beginning near the end at which the fan works and going gradually towards the door; by this means any spores adhering to the walls would be destroyed, and no air could pass back to pollute the walls or floor which had been thus purified. A stove might be arranged at the door end of the room, by which cotton garments to cover the ordinary clothes of the surgeons and attendants might be heated to a temperature presumably sufficient to destroy or paralyse the vitality of any spores which might have been adhering to them, and where the knives and other appliances used might be previously heated, and where water used in washing the wounds might be previously heated under pressure. With such an arrangement at an hospital it seems to me that one interesting mode of investigation into some most important subjects might be commenced.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 5th, 1877.

CHARLES BAILEY, Esq., in the Chair.

MR. HURST exhibited some specimens of dried plants from Alexandria, mostly of the Genus *Silene*.

Mr. ROGERS exhibited some specimens of the rare British Moss *Bartramidula Wilsoni* (B. & S.) from the neighbourhood of Barmouth. It had not been noticed for forty years.

Several other rare mosses were noticed: the more uncommon being *Bartramia rigida*, and *Hednigidium imberbe*. Specimens of these were likewise exhibited, with about 25 specimens of Phanerogamic plants, *e.g.*, *Dianthus deltoides*, *Saponaria*, *Koniga*, *Veronica hybrida*, *Mentha* 4 specimens, etc.

MR. J. COSMO MELVILL exhibited the new shell of the Genus *Rostellaria* (*R. Martinii*, Marrat) from the coral reefs of Cebu, one of the Philippine Islands.

He likewise gave a résumé of the position and natural affinities of the Genus; and produced examples of five other species of *Rostellaria* from his private collection for comparison. The new species swells the number of known recent species up to nine, including two of the Subgenus *Rimella*, perhaps referable to *Strombus*. Its nearest ally is *R. curvirostris*, (Lum.) from which it differs by its greater size, its lightness, and above all by the channelled suture not being so well developed. It is very strange that so well worked a coast as that of Cebu should at so late an epoch have produced so imposing a novelty, as this is much the largest specimen of the Genus. But two specimens are known; the other having been presented by Mrs. Price Martin to the Liverpool Museum. No other species of *Rostellaria* proper is known to inhabit the Philippine Isles. Though both the species of the Subgenus *Rimella* are native there. *R. curvirostris* is from the Red Sea, the *R. rectirostris* (Sov.) = *fusus* (L.) being from Celebes.

Ordinary Meeting, December 11th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

“Note on the Daguerreotype Portrait taken of the late Dr. Dalton,” by J. B. DANCER, F.R.A.S.

I was not present at the meeting of this Society on the 27th November, but have read in the printed Proceedings of that meeting that a photographic portrait of the late Dr. Dalton was presented to the Society; and that this photograph was enlarged from a Daguerreotype, taken by the late Mr. John Parry, about the year 1841. I have a doubt as to the correctness of the latter statement, and I will briefly give my reasons for that doubt. I worked at the Daguerreotype from the first announcement of its discovery, and when I came to reside in Manchester, in June, 1841, I still continued to amuse myself and friends with the process. The late Mr. John Parry was an acquaintance of mine, and he was not experimenting in the Daguerreotype at that period; if he had been, I think I should have been aware of it, and especially so if he had taken any photograph of Dr. Dalton. Mr. Beard of London (the gentleman who purchased the patent of the Daguerreotype process for England) opened a photographic gallery on November 18, 1841, in rooms over the Manchester Exchange; and the late Mr. Nicklin was the principal photographer, and through my acquaintance with that gentleman Dr Dalton received an invitation to sit for a photograph. The Dr. did not give his consent willingly, and his intimate friend the late Mr. Peter Clare had much difficulty in getting him down to the gallery. When in the photographic room the Dr. intimated that he would not sit for more than one picture, but Mr. Nicklin contrived to take three portraits. Mr. Peter Clare had one of these; Mr. John Dale

the chemist (who had been a pupil of the Doctor's) had another; and I became possessed of the third. Dr. Dalton died in August, 1844, and to the best of my belief the three Daguerreotypes named are the only photographs of the Dr. taken from life. Mr. John Dale has had his Daguerreotype of Dr. Dalton copied and enlarged, and in all probability the retouched photograph at the Society's room has originated from some of these copies. Anything relating to the late Dr. Dalton will, I am sure, interest the members of this Society. I do not hesitate, therefore, to bring this short account of his photographs before the Society. My Daguerreotype of Dr. Dalton is here for the inspection of the members; it has, unfortunately, been roughly handled, through lending it to artists, engravers, sculptors, and seal engravers, and the impression now is faint compared to when it was taken. Among the Daguerreotypes will be found one of the late Mr. Nicklin—it is the centre portrait of the three. This gentleman was one of the photographers sent out by government to the Crimea. He was unfortunately drowned when the *Rip van Winkle* foundered in the Black Sea. There is also a Daguerreotype of the late Mr. John Parry, which I took about 1848.

Since writing the foregoing, Mr. G. S. Woolley has shown me a small copy of Dr. Dalton's photograph, and on comparing it with my own copy of Mr. Dale's Daguerreotype I find it to be identical. My conjectures have proved correct.

“Note on Metallic Niobium and a new Niobium Chloride,” by Professor H. E. ROSCOE, F.R.S., &c.

The history of the investigation of the two closely related metals Niobium and Tantalum is of interest as showing how step by step the difficulties and uncertainties which surround the examination of analogous and rare substances such as these are overcome; and I may therefore be allowed shortly to state the growth of our knowledge on this subject.

In the year 1801 Hatchett discovered a new metal in the

mineral columbite from Massachusetts, and to this he gave the name of Columbium. Ekeberg a year later found a similar new metal in a rare Swedish mineral, and to it he gave the name Tantalum. Wollaston in 1809 came to the conclusion that these two metals were identical. Berzelius next examined the metal contained in the Swedish tantalite, and afterwards Wöhler found these metals in several other minerals. Heinrich Rose next investigated this subject, but in spite of persistent effort the results of his experiments left the matter in a still less satisfactory condition, inasmuch as he first came to the conclusion that two more new metals were contained in these minerals, to which he gave the names of Niobium and Pelopium, whilst at a later period he decided that both niobic and pelopic acids were different oxides of the same metal, for which he proposed the names niobic and hyponiobic acids.

Then again in 1860 v. Kobell thought that he had found a fourth new metal, which he called Dianium, in the the same minerals, and Hermann believed that he had discovered in them a fifth metal, to which he gave the name Ilmenium. It is to Marignac and Bloomstrand that we owe a deliverance from this state of contradiction and uncertainty. They independently proved that only two metals in reality exist—Niobium and Tantalum—and that these are present in varying proportion in most of the minerals in question. They showed that the compounds derived from Rose's niobic oxide contain mixtures of niobic and tantalic oxides, whilst his hyponiobic oxide is pure niobic oxide.

Bloomstrand arrived at this result by a series of analyses of the chlorides. He proved that the white hyponiobic chloride of Rose is in fact an oxychloride, whilst the yellow niobic chloride contains no oxygen.

Marignac, on the other hand, came to a similar conclusion by an investigation of the fluorides of niobium and tantalum, and pointed out that the oxides are pentoxides Nb_2O_5

and Ta_2O_5 , whilst the niobium chloride and oxychloride are represented respectively by the formulæ $NbCl_5$ and $NbOCl_3$.

All the other supposed metals turned out to be either simple mixtures of these two in varying proportions, or such a mixture containing in addition some other metal, such as tungsten, titanium, or iron.

Troost determined the vapour densities of niobium pentachloride and oxychloride, obtaining the numbers 9.6 for the first and 7.87 for the second, agreeing closely with the calculated densities when the atomic weight of niobium is taken to be 94.

Some time ago I obtained several crystals of Greenland columbite which possessed a specific gravity of 5.389 and which contains, as Marignac has shown, little or no tantalum. From this material I prepared niobium pentoxide (Nb_2O_5), niobium pentachloride ($NbCl_5$), and niobium oxychloride ($NbOCl_3$) by well known methods.

I have likewise prepared metallic niobium and a new chloride of niobium. The results of these experiments are contained in the following communication.

The following results of analysis of the pentachloride and oxychloride serve to show the purity of the material.

<i>Pentachloride.</i>		<i>Oxychloride</i>	
Calculated.	Found.	Calculated.	Found.
Niobium.....34.62	34.38	Niobium.....43.42	42.93
Chlorine.....65.38	65.55	Chlorine.....49.19	49.34
—————	—————	Oxygen (Diff.) 7.39	7.73
100.00	99.93	—————	—————
		100.00	100.00

Metallic Niobium is difficult to prepare, and precautions similar to those employed for the preparation of metallic vanadium must be taken. It is obtained as a bright steel grey shining metallic crust by passing the vapour of the pentachloride mixed with hydrogen repeatedly through a red-hot glass tube. The metallic crust then deposited on the side of the tube is next strongly ignited in a porcelain tube in a current of hydrogen until no further evolution of hydrochloric acid occurs.

In order to avoid as completely as possible access of traces of oxygen, the very hygroscopic pentachloride was prepared in a narrow tube, and this was transferred bodily into the wider tube in which the preliminary heating with hydrogen takes place.

The metal thus obtained was analysed by placing a weighed quantity in a porcelain boat and heating it in a glass tube in a current of air. The metal takes fire and burns brightly to the pentoxide, a small trace of oxychloride being usually deposited, existing as impurity in the metal. In one instance when the metal was allowed to oxidise very slowly, the formation of an indigo-blue-coloured lower oxide was first observed; this afterwards passed into the yellow pentoxide on further heating.

The following analyses were made with metal obtained in two distinct operations :

I. Weight of Metallic Niobium taken 0.3963 gram.
 ————— Niobium pentoxide obtained... 0.5480 „

This corresponds to 0.3810 of metal; or the substance contains 96.14 per cent of niobium.

II. Weight of Metallic Niobium taken 0.3900 gram.
 ————— Niobium pentoxide obtained... 0.5403 „

This corresponds to 0.3790 of metal; or the substance contains 97.18 per cent of niobium.

That the substance thus prepared is not, as has been supposed, a hydride, is shown by the fact that the second of the above specimens on analysis was found to contain only 0.27 per cent of hydrogen.

Metallic niobium possesses a steel-grey colour and bright metallic lustre. It is remarkable that it is not attacked in the cold by hydrochloric or nitric acids, either dilute or concentrated, or even by *aqua regia*. Strong sulphuric acid, on the other hand, dissolves the metal quickly, yielding a colourless solution. The metal does not take fire in chlorine gas at the ordinary temperature, but it does so when gently heated, with formation of pentachloride and a trace

of oxychloride, owing to the unavoidable presence of a small quantity of oxygen in the metal experimented with.

The specific gravity of metallic niobium at 15.5° is 7.06 as a mean of two well agreeing determinations made with half a gram of substance.

Niobium Trichloride NbCl_3 . When the vapour of niobium pentachloride is passed through a heated glass tube it is slowly decomposed, a black deposit of trichloride being formed on the sides of the tube. Niobium trichloride is generally obtained as a black crystalline crust which possesses an almost metallic lustre, and closely resembles in appearance that of a film of sublimed iodine. It is sometimes deposited in long needle-shaped crystals which exhibit a dichroic lustre. The trichloride is non-volatile, it is not deliquescent, nor is it decomposed by water or ammonia, but when brought into contact with dilute nitric acid it is at once converted into niobic and hydrochloric acids. Heated in the air it emits dense white fumes.

For the purpose of analysis the trichloride, from several different preparations, was, in some cases, decomposed by nitric acid directly, and in others first fused with sodium carbonate and the fused mass treated with nitric acid.

		No. 1.	No. 2.	No. 3.
Substance taken... ..		0.2781 ...	0.4406 ...	0.5344
Nb_2O obtained		0.1930 ...	0.3000 ...	0.3611
AgCl		0.5681 ...	0.9333 ...	1.1218
	Calculated.	No. 1.	No. 2.	No. 3.
Niobium Nb	46.88 ...	46.68 ...	47.76 ...	47.42
Chlorine Cl_3	53.12 ...	50.53 ...	52.40 ...	51.93
	<hr/>	<hr/>	<hr/>	<hr/>
	100.00	99.21	100.16	99.35

When the vapour of niobium oxychloride is passed over red-hot charcoal in an atmosphere of carbon dioxide no change occurs, and the pentachloride is obtained when the carbon dioxide is replaced by chlorine.

A remarkable reaction occurs when niobium trichloride is heated in a current of carbon dioxide; this gas is decomposed, a white sublimate, consisting of oxychloride, is

obtained, and the corresponding quantity of carbon monoxide is formed.

That this is the case was ascertained by analysing the oxychloride and by collecting the gas over caustic potash and measuring the volume of insoluble gas which came over. This gas was shown to burn with a blue flame, and the quantity was found in two experiments to correspond closely with the theoretical amount. The following is the composition of the oxychloride thus prepared:—

	Found.
Niobium	43·48
Chlorine	47·03
Oxygen	9·49
	<hr style="width: 10%; margin: 0 auto;"/>
	100·00

A decomposition of carbon dioxide by a metallic chloride has hitherto not been observed. Phosphorus trichloride absorbs free oxygen on distillation in the air, with formation of oxychloride, but it does not decompose carbon dioxide.

“On the Retention of Saline Impurities by Hydrated Ferric Oxide,” by Mr. CHARLES FREDERICK CROSS, Dalton Scholar in the Owens College. Communicated by Professor H. E. ROSCOE, F.R.S.

It is a fact well-known to all chemists that ferric oxide, when precipitated in the form of hydrate from solutions which contain either organic matter or the fixed alkalis, carries down a certain quantity of these substances in adhesive union.

That this adhesive action is exerted upon *saline* bodies in solution, although an obvious inference, I have found nowhere stated to be the case. The importance of the analytical bearings of such a property, if it do exist, led me to undertake a quantitative investigation of the matter, the results of which I proceed briefly to detail.

I. Two grams fine iron wire were dissolved in HCl, the solution oxidized with HNO₃, the excess of acid driven off

by evaporation, distilled water added, and the volume of this solution of ferric chloride made up to about 50 c.c.

The weight of this solution was found to be 56.0955 gram. 1 gram. therefore contains .0356 gram. Fe as Fe_2Cl_6 .

To verify this calculation: 2.529 gram. solution were precipitated by ammonia, and the iron estimated as oxide in the usual way.

	Found.	Calculated.	Difference.
Fe.	.0896	.0901	.0005.

A series of determinations was then made in which the oxide was precipitated by ammonia in presence of sodium chloride, which was selected as a typical saline compound. The results are tabulated below:—

	Weight of Solution taken.	Weight of Fe. contained.	Weight NaCl added.	Weight Fe. from Fe_2O_3 obtained.	Error.	NOTES.
a.	2.556	0.0911	2.000	0.0938	.0027	Wash Water 350 cc. pptd. in <i>boiling</i> solution.
b.	2.574	0.0917	2.000	0.094	.0023	Wash Water 250 cc. pptd. in <i>boiling</i> solution.
c.	2.579	0.0919	1.000	0.0945	.0026	Wash Water 125 cc. pptd. in <i>boiling</i> solution.
d.	2.542	0.0906	2.000	0.0952	.0046	Wash Water 175 cc. NaCl added after pptn.
e.	2.480	0.0884	2.000	0.0941	.0057	Wash Water 175 cc. pptd. in <i>cold</i> solution.
f.	2.587	0.0922	2.000	0.0994	.0072	Wash Water 375 cc. pptd. in <i>cold</i> solution.

In the above determinations no more than the ordinary precautions were adopted, *i.e.*, the precipitate was washed with boiling distilled water from 15 to 30 times until the washings no longer gave an alkaline reaction.

In some cases the precipitate of oxide was redissolved after weighing, in HCl, and the iron precipitated as sulphide. The filtrate from the latter was evaporated and the residue ignited—in every case a considerable fixed residue of sodium chloride was obtained.

A similar series of determinations was then made with ferrous ammonium sulphate; weighed quantities were oxidised with nitric acid and ferric oxide precipitated by ammonia in presence of sodium chloride. The results

obtained were identical with those already cited. In two of these determinations caustic potash was substituted for ammonia: the results obtained deserve mention:—

II.	Wt. of $\text{Fe}(\text{NH}_4)_2$ $2\text{SO}_4 \cdot 6\text{H}_2\text{O}$.	Per centage Fe theoretical.	Wt. of KOH&NaCl added.	Per centage Fe from Fe_2O_3 obtained.	Error. per cent.	NOTE.
<i>g.</i>	·706	14·28	Grms. { 5 KOH 5 NaCl	17·93	3·65	Wash water=950 cc. pptd. in <i>boiling</i> so- lution
<i>h.</i>	·720	18·76	4·48	Wash water=400 cc. pptd. in <i>boiling</i> so- lution.

The precipitates were redissolved in concentrated HCl after weighing. They were observed to effervesce violently on treatment with the acid, and on evaporating the solution the alkaline chloride crystallised out in considerable quantities.

I instance these as indicating the extent of adhesion existing between ferric oxide and the fixed alkalis, and the futility of the practice, which I believe is not unusual, of weighing a precipitate of the oxide from which alumina has been removed by means of potash, without previously redissolving and again precipitating.

Determinations were then made in which special precautions were taken with the view of ascertaining the limit of the error due to the retention of alkaline salt. A weighed quantity of pure iron was taken for each experiment. The results are appended:—

III.	Wt. Fe taken.	NaCl added.	Wt Fe calc. from Fe_2O_3 obtained.	Error.	NOTE.
<i>k.</i>	·1669	0·000	·1666	—0·0003	} Washed persistently with boiling water till washing abso- lutely free from Chlorine.
<i>l.</i>	·2088	1·000	·2096	+0·0008	
<i>m.</i>	·1485	1·000	·1498	+0·0013	

The impurity present in the precipitate in *m* was isolated in the manner described above and weighed. It was found = ·0023 grm. Multiplying this by 0·7, the fraction expressing the ratio $\text{Fe}_2:\text{Fe}_2\text{O}_3$, we obtain the number 0·0016, which it will be seen is in close agreement with the error obtained (calculated as Fe).

In the following experiment pure iron was brought into solution as Fe_2Cl_6 , and precipitated by ammonia alone.

The precipitate was thoroughly washed till the washings were absolutely free from chlorine, and transferred, together with the filter paper, to a retort containing a solution of caustic soda, which had been freed from ammonia by prolonged boiling. The retort was connected with a condenser, the contents of the retort boiled, and the distillate "nesslerised" in successive portions. The result is appended :

	Wt. Fe. taken.	NH ₃ estimated in distillate.	Calc. as NH ₄ Cl.	NOTE.
n.	·2265	·00054	·0017	500 cc. boiling distilled water employed in washing the ppt. of oxide.

Determinations were then made in which other alkaline salts were substituted for sodium chloride :

IV.	Wt. Fe. taken.	Salt added. gm.	Wt. Fe. found.	Error.	NOTES.
p.	·160	LiCl. 0·25	·1603	·0003	The isolated impurity was examined with the spectroscope, which showed strong Li line.
q.	·2165	Na ₂ SO ₄	·2191	·0026	Washings gave H ₂ SO ₄ reaction after all HCl removed. 500 cc. employed.
r.	·206	KNO ₃	·2097	·0037	Wash water=325 cc. ppt. in boiling solution.
s.	·182	KNO ₃	·1866	·0046	Wash water=650 cc. pptd. in cold solution.

Having thus sufficiently investigated the adhesive action of ferric hydrate upon salts of the alkalis in solution, determinations were made in which salts of the alkaline earths were substituted for the former. For these experiments it was necessary to employ ammonia free from carbonate, which was prepared by agitating the ordinary solution with lime in a closed flask and filtering just prior to use. Care was taken to add the reagent in but very slight excess. The following results were obtained :

V.	Wt. Fe. taken.	Salt added.	Wt. Fe. found.	Error.	NOTES.
t.	·173	CaCl ₂	·1786	·0056	Pptd. in boiling solution. Wash water =330 cc.
v.	·192	CaCl ₂	·2052	·0132	Pptd. in cold solution. Wash water = 650cc.

Similar results were obtained in presence of barium chloride. I may mention that by employing an ordinary laboratory solution of ammonia the error on ·152gram. Fe (precipitated in *hot* solution) amounted to ·0072, and on ·2313 (precipitated in *cold* solution) to ·0196 gram. These

results were confirmed by isolating the impurities and determining the calcium by precipitation as oxalate. The precipitates effervesced considerably on treatment with the acid.

The above experiments suffice to indicate the magnitude of the obvious error which attends the common practice of separating iron and similar oxides from the alkaline earths by precipitation with excess of alkali.

Since iron is very frequently separated from other metals in the form of the basic acetate, the purity of this compound, *i.e.* of the oxide which results from the washing and ignition of the same, was made the subject of the following determinations :

VI.	Wt. Fe. taken.	Salt added.	Wt. Fe. found.	Error.	NOTES.
<i>w.</i>	·1995	NaCl	·2007	·0012	} In all cases solution Fe_2Cl_6 nearly neutralised with $(\text{NH}_4)_2\text{CO}_3$ and pptd. by $\text{NaC}_2\text{H}_3\text{O}_2$ in excess.
<i>x.</i>	·2106	NaCl	·2122	·0016	
<i>y.</i>	·180	CaCl_2	·1825	·0025	} Only a trace of Ca could be detected in the isolated impurity.
<i>z.</i>	·1987	CaCl_2	·2000	·0013	

Here as before it is seen that alkaline salt is retained by the iron oxide, but the separation of the latter from the alkaline earths is practically complete.

The general conclusions to be drawn from the above results are :

(1). That ferric oxide, when precipitated in presence of salts of the fixed alkalis, removes a certain quantity of the latter from solutions, this quantity being within certain limits independent of the amount of alkaline salt present in solution, but varying with the temperature of the solution at the time of precipitation, being much greater in the case of cold than of hot solutions. (I, IV.)

(2). That the error due to the retention of alkaline salt may, by persistent washing with boiling water, be reduced to a minimum but not completely eliminated. (III.)

(3). That the error is very much greater when the oxide is precipitated by excess of alkali in presence of salts of the

alkaline earths; but that the latter may be effectually separated from iron by precipitating this metal in the form of the basic acetate.

(4). That ferrous sulphide does not exert this adhesive action upon salts in solution, and is therefore, where the circumstances of the analysis permit, the best form in which to separate iron from the other metals which accompany it.

In conclusion, I would briefly call attention to the researches of Reichardt and Blumtritt* upon the adhesive action of iron oxide upon gases, as affording indirect evidence in favour of the conclusions at which I have arrived from a consideration of the results of my investigation.

The former chemist found that 100 grm. of ferric hydrate recently prepared by precipitation with ammonia, from a solution of ferric chloride, yielded, under suitable conditions, 375.54 cc. of a gaseous mixture (O, N and CO₂)—that the same weight of hydrate previously washed with hot water yielded 250.82 cc, and that 100 grm. of the oxide after gentle ignition, were found to retain 40 cc. of gas. These results, together with the composition of the gaseous mixtures in each case, is set forth below :

Wt. of Substce.	Condition of oxide experimented upon.	Vol. of gaseous mixture in cc.	Ratio of vol. of substance in that of gas.	Per centage composition of gas by vol.	Wt. of CO ₂ = i.e. per centage.
100	{ By precipitating Fe ₂ Cl ₆ with NH ₄ OH and drying in the air. { After washing with hot water. { After gentle ignition.	375.54	1:3	N O CO ₂ 26.29—3.85—69.86	.51
"		250.82	1:3.5	18.9—0.91—80.19.	.39
"		42.69	1:0.58	65.32—10.54—24.14	.02

These facts not only explain the fact of the retention of the fixed alkalis and alkaline earths in the form of carbonates, which I have stated to be the case, but compel the inference, apart from experimental verification, that the adhesive action of ferric hydrate upon saline bodies in solution would be equally characteristic.

* *Journ. Prakt. Chem.*, ii, 440—467 (1866).

Ordinary Meeting, December 26th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

“Notice of a large Boulder Stone at Old Trafford, Manchester,” by E. W. BINNEY, F.R.S., F.G.S.

Of late years considerable attention has been directed to the large stones found in the till or brick clay about Manchester. Many years since, in vol. viii., Second Series, p. 278 of the Society’s Memoirs, a description was given of some of the largest which had been then observed—one of them, found near Mr. Buckley’s sand delf at Collyhurst, reached the weight of about 5 tons. Since then a much larger specimen, estimated at nearly 20 tons, has been placed in the public park at Oldham. Probably many others have been met with and escaped my notice.

In the past summer Mr. William Worthington, contractor, of this city, met me in the street and showed me what he termed a piece of a remarkable stone which he had met with in making a main sewer under Seymour-grove, Old Trafford. After looking at it I said, I wish you had taken the stone out for me. He replied, that getting it to the surface would have been no easy task, as it weighed over 50 tons. I went down to Seymour-grove to look for myself, and found plenty of the fragments of the stone, but the specimen itself I did not see, as it had been covered up. The stone was a fine grained gritstone, which Lancashire borers would call a burr. It was remarkably hard, tough and sharp, and its outside was smoothed and scored with parallel striæ as shown by the specimen exhibited to the meeting. Rocks resembling it are sometimes met with in

the lower coal measures of Lancashire, but I never saw one so hard, tough, and sharp. Its exterior resembled the skin on the outside of a fine grained sandstone found near a fault and which had been there subjected to great friction, but in the case of the boulder this hardness extended right through the specimen. No traces of bedding or lines of stratification were seen, nor was there evidence of any joints in it. The whole appeared as one compact mass. In breaking it I found black patches which might have been caused by the carbonaceous matter of fossil plants, and thin veins of sparry matter, some of them branching out. Professor W. Thomson, F.R.S.E., of the Royal Institution, has kindly examined the stone for me and finds its specific gravity to be 2.654, water at 60° Fah. = 1.

Mr. Worthington has been so kind as to furnish me with the following information :

The Ash, Whalley Range,
Nov. 8th, 1877.

DEAR SIR,

It is impossible for me to give dimensions of the large boulder we cut through in tunnelling for the sewer in Seymour Grove, Old Trafford. I can only form a rough idea of the weight of it. It took two of our men a week to blast through it. We brought to the surface stone which I estimate about fifteen tons. After the opening was cut through it I went down the shaft to examine the same, and found a large mass of stone left below our level and on the westerly side. I should think that we did not get more than one-third of the stone out. There are eight or ten tons of it now lying at my house, which I intend to use for rockery work. Any other information you may require I shall be glad to give you.

Yours truly,

WM. WORTHINGTON.

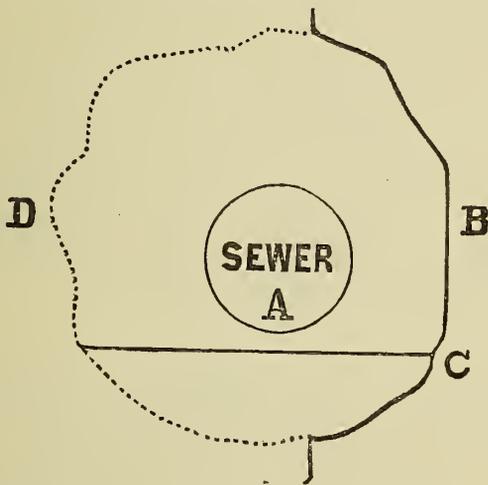
To Mr. E. W. Binney.

Manchester, Nov. 14, 1877.

Large Boulder in Seymour Grove.

DEAR SIR,

I find that we did take out the full depth of the stone, which was something like three feet below our level. The easterly side was also taken off, leaving the bulk of the stone left in on the western side and the roof of the tunnel. The length blasted through horizontally was about ten feet, and the hole driven through it 4ft. 6in. \times 3ft. 6in. The section is something like the following:—



To Mr. E. W. Binney.

EXPLANATION.

A—Excavation through the stone for the sewer.

B—Side of the stone cut away.

C—Level of the excavation.

D—The bulk of the stone left in the westerly side and roof of the tunnel, as shown by the jagged line.

The deposit of till in which the stone was found is about twenty feet in thickness, and rests on the pebble beds of the Trias. It is free from many small stones, but a few boulders of moderate size have been met with in making the sewer. The height of the spot where the stone was found is ninety feet above the sea level, the direction of the sewer magnetic north and south, and the longitudinal axis of the

stone appeared to be at a right angle to the excavation. The place where it occurred was about a quarter of a mile to the south of the Old Trafford Railway Station, opposite to some dark-coloured palings on the right-hand side of Seymour Grove.

It is hard to say where the stone came from, but it has all the appearance of a fine gritstone of the lower coal measures, and great friction may have generated sufficient heat to alter its structure in a similar manner to what has been done on the sides of great faults and slides where two bodies have been rubbed against each other.

As to its dimensions, we have no data to go upon, except the opinion of Mr. Worthington, an experienced judge of bulks, and a stone merchant, who, when he first mentioned the specimen to me, said that he estimated its weight at over fifty tons. The depth from the surface to the bottom of the sewer was thirteen feet.

“On the Geometrical Representation of the Equation of the Second Degree,” by CHARLES CHAMBERS, F.R.S., Superintendent of the Colaba Observatory, Bombay. Communicated by J. A. BENNION, F.R.A.S., A.C.P.S.

For every value from $-\infty$ to $+\infty$ of one of the variables in an equation of the second degree between two variables, there are corresponding pairs of values of the other variable; and for every value from $-\infty$ to $+\infty$ of the second variable there are corresponding pairs of values of the first variable. The corresponding pairs of values are of two classes, viz. first those which are not, and secondly those which are, affected by the symbol $\sqrt{-1}$. The Cartesian method gives a perfectly clear geometrical representation of that part of the equation for which the values of both the variables are real, but discards as unintelligible that part for which either of those values is imaginary. In the simplest case—of rectangular coordinates—the unit adopted

in the Cartesian system, as the analogue of the unit of the algebraical equation, is a unit length of a mathematical straight line, with a convention as to signs which we need not describe. For a reason that will presently appear, we here make the otherwise simple observation, that if we substitute—in the Cartesian method—the idea of pairs of coincident ordinates and take the product of their values instead of the square of a single ordinate, we only add a convention that leaves the analogy between the algebra and the geometry in question untouched. We further remark that this analogy will still be perfect, if we conceive of the two coincident ordinates as tubes instead of mathematical straight lines, providing the tubes be of infinitesimal transverse section, which may be of circular form and (say) of radius R , and providing further that the numerical values of the tubes are the quantities of curved surface which they possess, the convention as to signs being unaltered: the unit of curved surface must obviously be $2\pi R$. Instead, however, of the curve being described by the motion of the central point of the extremity of the ordinate, let it be described by a diameter, through that point, perpendicular to the plane of the coordinates: it will still be of infinitesimal depth. Further let us take, as the fundamental ideal of the tubular ordinate, the one whose height is R . It may be varied in value by elongation in a positive or negative direction, its diameter remaining constant; or it may be varied by dilatation into larger and larger circles, its height remaining constant: in the latter case the ordinate, which may be called circular to distinguish it from the tubular ordinate, will be of positive sign if it be on the positive side of the coordinate plane, which is perpendicular to the tubular ordinate, and of negative sign if on the opposite side of that plane. Elongation to unit length of the tube, and dilatation to unit radius of the circle, alike make the quantity of curved surface $2\pi R$, which is the unit of this

magnitude. Adopting this latter unit as the analogue of the algebraical unit, we shall thus have two distinct but connected systems of variation of the magnitude—that of elongation yielding tubular ordinates analagous to and co-extensive with the ordinates of the Cartesian geometry; and that of dilatation yielding the circular ordinates, the nature of which we now proceed to consider.

Just as in our modified Cartesian system, the ordinate is erected with the axis of the tube at the extremity of the corresponding coordinate, and with a length determined by the equation; so here the circular ordinate must be described (on the plane perpendicular to the tubular ordinate) with its centre at the extremity of the corresponding coordinate, and its radius the function of the latter coordinate given by the equation. Also as the curve, in the Cartesian system, separates the region of greater ordinates than its own at any point from the region of lesser ordinates, so here the curve must be such as will do the same; in other words, it must be the envelope of the series of circular ordinates. The reason will now appear why we substituted the product of a pair of coincident tubular ordinates for the square of a single ordinate: one of the pair we suppose to be produced by the elongation of a fundamental tube, of infinitesimal height and radius, on the positive side of the perpendicular coordinate plane, and the other by elongation of a like tube on the negative side of that plane. The equal dilation of these tubes will produce, not coincident, but adjacent circular ordinates, and these of opposite sign, and of which the product will therefore be a negative square. The curve will be described by the line of intersection of contiguous circular ordinates, that line being of infinitesimal length $2R$, half on the positive side and half on the negative side of the plane of the circles.

Once again, we introduce an arbitrary convention that is only additional to, not in contravention of, those of the

Cartesian geometry. In whichever manner the pair of fundamental tubes enlarges, whether by elongation or dilatation, we regard the product of the pair of ordinates produced as the counterpart of the square of the corresponding variable in the algebraical equation. Thus, if γ be the magnitude, regardless of sign, of the geometrical ordinate, then, when $y^2 = a^2$, we may for the algebraical y^2 substitute $(+\gamma)(+\gamma)$ or $(-\gamma)(-\gamma)$, and we obtain the result $\gamma = \pm a$; and, when $y^2 = -a^2$, we obtain $(+\gamma)(-\gamma) = -a^2$, whence $\gamma = a$. In this way we avoid altogether the introduction of the algebraical symbol $\sqrt{-1}$ as affecting the geometrical representation of the equation. Although in the case of circular ordinates the value $\gamma = a$ is single, it is obvious that two points in the curve—that is, in the envelope of a series of circles whose centres are on a straight line—have the same ordinate and abscissa, one point, namely, on each side of the straight line.

The writer is prepared to have what he has advanced so far regarded as rather curious than instructive, unless it can be shown that the system he describes leads to interesting results. In what follows it will be shown that such results are not wanting.

We now write down a series of equations and construct a figure to represent them; and we then proceed to comment on the equations and curves, and on their points of interest in connection with what has gone before.

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1 \dots\dots\dots(\alpha)$$

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = -1 \dots\dots\dots(\alpha')$$

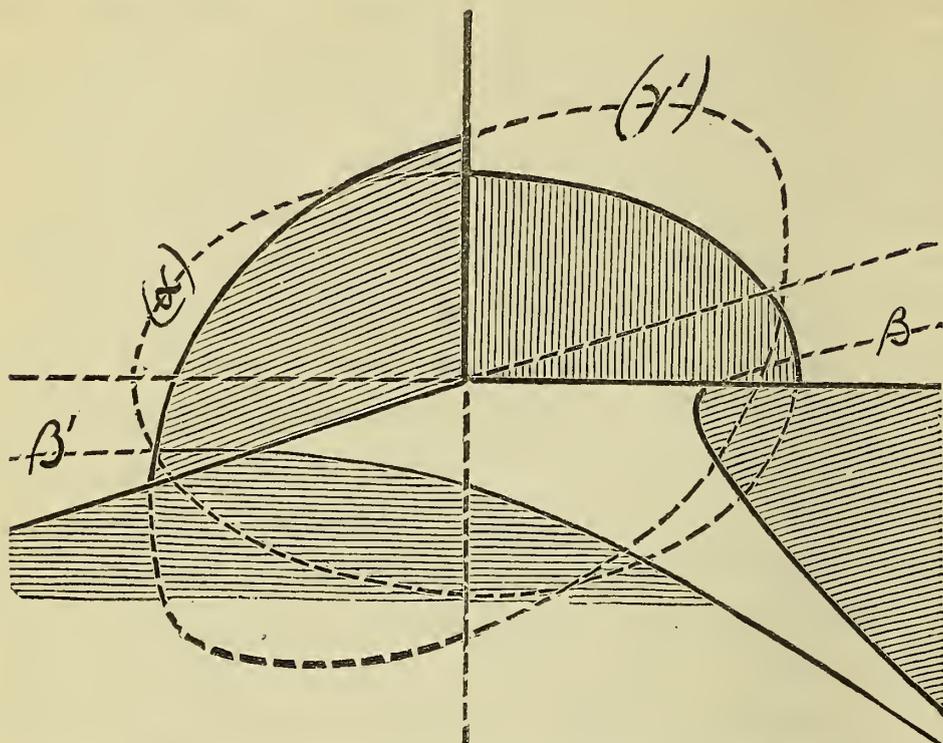
$$\frac{x^2}{a^2 - b^2} - \frac{z^2}{b^2} = 1 \dots\dots(\beta)$$

$$\frac{x^2}{a^2 - b^2} - \frac{z^2}{b^2} = -1 \dots\dots\dots(\beta')$$

$$-\frac{y^2}{a^2 - b^2} - \frac{z^2}{a^2} = 1 \dots\dots(\gamma)$$

$$-\frac{y^2}{a^2 - b^2} - \frac{z^2}{a^2} = -1 \dots\dots\dots(\gamma')$$

Of equation (a) we have the following cases, a^2 and b^2 being always positive numbers :—



Case 1. When x^2 and y^2 are both positive numbers :— the ordinates are both tubular, and the equation is represented, as in the Cartesian system, by the ellipse (a) whose semi-axes are a and b .

Case 2. When y^2 alone is negative, that is, when x^2 is positive and greater than a^2 :—the x ordinates are tubular and the y ordinates circular, and the equation is represented by an envelope on the xz plane, the Cartesian equation to which we shall presently show to be (β), viz :—That of the hyperbola (β). This hyperbola has its vertices in the foci of the ellipse (a), and its foci therefore at the vertices of the ellipse, and its transverse axis is of the same magnitude as that of the ellipse.

Case 3. When x^2 alone is negative, that is, when y^2 is positive and greater than b^2 : the y ordinates are tubular and the x ordinates circular : the envelope of the latter has (γ) for its Cartesian equation, and is an impossible curve or in other words, there is no intersection of the circular ordinates and therefore no envelope.

Case 4. When both x^2 and y^2 are negative: the x and y ordinates are both circular, and each pair being compounded (in the manner of the compounding of moments of rotation) forms a single circular ordinate, the plane of which is different from that of every other compound ordinate; hence, there can be no intersection of compound ordinates, and these can have no envelope.

The three equations (α), (β), (γ) possess this remarkable reciprocal property: if any one of them be taken as the equation to be represented, it is itself the Cartesian equation of the corresponding curve when x^2 and y^2 are both positive, and the two others are the Cartesian equations of the respective envelopes when either x^2 or y^2 is alone negative: and thus the ellipse (α) and the hyperbola (β) are the representative curves of all the three equations.

The three similar equations (α'), (β'), (γ'), in which the only difference from the former three is in the reversed sign of unity on the right-hand sides—possess, of course, the same reciprocal property; but here the impossible curve is on the xy plane, that on the xz plane is the conjugate hyperbola to the former one, and is on the same plane with it and has therefore the same asymptotes, whilst the curve on the yz plane is an ellipse whose semi-axes are a and $(a^2 - b^2)^{\frac{1}{2}}$.

When $a = b$, that is, (α) is a circle, the three other real curves coincide with the z axes.

To find the Cartesian equation to the envelope of the circular ordinates in the case No. 2, in which y^2 alone is negative, we have for the positive square of the type radius of the circles $\frac{b^2}{a^2}(x^2 - a^2)$, and for the equation to a type circle

$$(\xi = x)^2 + \zeta^2 = \frac{b^2}{a^2}(x^2 - a^2) \dots r \dots (1)$$

where x is the variable parameter: hence, taking the first derivative,

$$-(\xi - x) = \frac{b^2}{a^2}x \dots \dots \dots (2)$$

and eliminating x , by means of (1) and (2), we obtain

$$\frac{\xi^2}{a^2 - b^2} - \frac{\zeta^2}{b^2} = 1 \text{ or } \frac{x^2}{a^2 - b^2} = \frac{z^2}{b^2} = 1$$

which is the equation (β) written above. Similarly, by dealing with the impossible ordinates to the y abscissæ, we obtain the equation (γ).

The same three equations (α), (β), (γ), have the reciprocal property that has been described, when, instead of the axes being rectangular, the primitive equation is referred to a pair of conjugate diameters as axes, providing, however, that the tubular ordinates be of elliptical transverse section such that their section by the plane of the two other coordinates is circular.

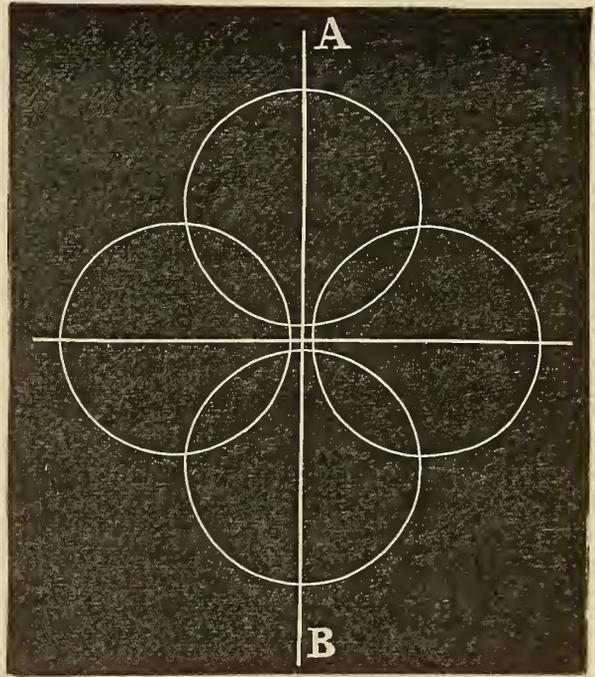
The extension of the method so as to represent an equation between three variables, referred to rectangular axes, presents no difficulties.

It will be observed that the envelope of the circular ordinates is the same central conic, that we should obtain by taking, in the proper plane, ordinates affected with the sign $\sqrt{-1}$ as normals instead of ordinates and measured from the extremities of the respective abscissæ which yield them.

One interesting consequence of taking account of both the representative curves of an equation, is that it affords a geometrical illustration of the four-point intersection of pairs of conics in cases which are unintelligible without it: in the case of similar and similarly situated concentric ellipses, for example, the absence of any points of intersection on the ellipses is compensated for by the touching of the pair of supplementary hyperbolas on the coincident asymptotes.*

* The writer is under the impression that he will find that, in the general case of elimination between the equations of a pair of conics, if

Years ago, the writer became acquainted with the method in analytical geometry which regards a plane and a straight line—not as the pure abstractions of the mathematician, but—as small portions of the surface of a sphere and of the periphery of a circle respectively in the limits in which the radii of the sphere and circle become infinite; and, whilst noting that these definitions were open to serious objections (chiefly as giving sides to a plane and a straight line of an unsymmetrical character) he was much struck by the extent to which they enlarged the field in which geometrical interpretation might be consistently applied to algebraical forms of expression. He became strongly impressed with the notion that fruitful speculation would lie in the direction indicated—of regarding a plane and a straight line as limits of some kind, and that kind such as would, if possible, cause them, at infinity of distance from the parts considered, to deviate symmetrically from the positions due to the abstract conceptions of a plane and a straight line. The rotation of the system of circles shown on the margin about the straight line AB as an axis will yield examples of a straight line and a



one or two pairs of impossible values are given for the co-ordinates of the points of intersection, then as many pairs of real values will be given by elimination between the equations of the supplementary pair of conics which lies in the plane that contains the two centres; but he has yet to apply the analytical test of the correctness of this impression. The nature of this test will be to find whether for each case in which the primitive pair of equations gives x^2 or y^2 negative, the supplementary pair gives x^2 or y^2 positive, and *vice versa*.

plane of the symmetrical kind required, providing that the opposite pairs of equal circles are at the infinitesimal distance R from the central point of the figure, and that their radii are infinities of a second order in comparison with R ; for, with these conditions, finite quantities, which are infinite in comparison with R , will be infinitesimal in comparison with the radii of the circles. Let a sphere of finite radius be described with its centre at the central point of the figure, then the part of the tube of which AB is the axis that is cut off by this sphere has the characteristics of our tubular ordinates; and the part of the same sphere cut off by the space between the upper and lower circles forms circles of the nature of our circular ordinates. When the sphere becomes infinite and of radius greater than the diameter of the circles of the figure, the infinite ordinates—tubular and circular, positive and negative—will all go out into space altogether without the system, in obvious analogy with the sudden passage, in certain otherwise continuous variations, of $+\infty$ and $-\infty$ into each other.

Ordinary Meeting, January 8th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The PRESIDENT said that on the 31st of December last, at about 5.30 p.m., he was walking from Ainsworth to Bury, and he observed a large meteor which started from the star Pollux, and after making a slight curve upwards disappeared near the star Beta in Taurus. It seemed larger than the planet Venus, then brightly shining, and was of a yellowish-white colour, and disappeared without breaking. It left a luminous track of its entire course for a short time after it had passed.

“Note on the Decomposition of Water by Iron-pyrites,”
by C. A. BURGHARDT, Ph.D.

I have recently endeavoured to ascertain what part iron-pyrites plays in the formation of the natural sulphides of other metals, such as copper-glance, copper-pyrites, &c. These and other sulphides are supposed to arise through the reduction of the corresponding *sulphates* of those metals by organic matter. Bischoff, in his “Chemical and Physical Geology,” vol. iii., p. 554, says, “The production of sulphides presupposes the existence of sulphates; and the elimination of sulphur from sulphates, either in combination with hydrogen, or with the metals of the alkalies, or alkaline earths, presupposes the existence of organic life, since that

effect would be produced only by carbonaceous substances. The influence of organic substances in the production of sulphides is further shown by the fossil remains, consisting of iron-pyrites, blende, copper-pyrites, variegated copper-pyrites, sulphide of copper and cinnabar." This theory appeared to me to be open to serious objections, as it would be very difficult to show the presence of organic matter in many crystalline rocks sufficient to produce the large sulphide formations often occurring in them through the reduction of sulphates. I therefore desired to ascertain whether sulphuretted-hydrogen could be produced by simpler means, and in accordance with the occurrence of sulphides in rocks *free from organic matter*. With this view I placed some pure finely-powdered iron-pyrites into a glass tube, covered it with perfectly pure distilled water (previously well boiled in order to drive out any air dissolved in it), placed the tube and its contents in a vessel containing boiling water, heated it gently for a short time to drive out any air which might have been contained in the tube, and sealed the latter up. After heating the above in an air-bath for four days, at a temperature of about 120° C., the tube was opened, when a very strong smell of sulphuretted hydrogen was instantly perceived, and proved to be present by its chemical reactions. The liquid contents of the tube were analysed, and found to consist of ferric sulphate, ferrous sulphate, and free sulphuric acid, whilst there was a slight deposit of ferric oxide upon the sides of the glass tube. This experiment certainly proves that sulphuretted hydrogen can be produced in the interior of the earth *without* the presence of organic matter, as all rocks (both crystalline and sedimentary) contain more

or less iron-pyrites disseminated throughout them, and are also saturated with water. I hope soon to determine quantitatively the reaction which occurs in the above experiment.

Mr. R. D. DARBISHIRE, F.G.S., exhibited a picture, which he had received from a Japanese gentleman, as one of remarkable excellence and beauty and value in Japan. It represented the Buddhist Trinity seated amidst a court of personages in attitudes of veneration on a complicated open-air platform arrangement, in a sort of park, surrounded with gardens and small edifices or "temples," severally occupied by groups of saints (? or images) and attendants (or worshippers). Above this part of the design was a white region, with groups of personages floating on gorgeous clouds, amidst flying musical instruments and flowers; and above that a deep blue "firmament on high," with similar occupants and enrichments; every floating thing in each space being adorned with waving streamers of various colours.

On the whole platform and park were crowded groups of persons, many distinguished with a saintly nimbus round the head, and some with a flaming one, enveloping the figure from head to foot; the groups apparently representing Buddha and attendants, or various scenes of adoration, or trance, or mere tranquil enjoyment, in houses, gardens, or the water.

On each side of this principal illustration and below it were about fifty separate vignettes of much the same general character as those of the groups first mentioned.

The whole design included many scores of figures, much

varied architecture, and a variety of rich conventional decoration, and was brilliantly painted in very fine colours, with a lavish use of gilding for backgrounds and defining lines, with great minuteness and refined taste, upon a piece of very fine white silk, apparently faced with some paper or other substance of the kind, and mounted on a strip of rich brocade, exhibiting in circles, as usual, some Japanese crest.

The picture was stated to represent "What one would see in a Great Buddhist Temple, or in Heaven;" and to have come from a great gentleman's private house.

Mr. D. hoped hereafter to communicate a more detailed description of this very curious picture.

"On the Microscopic Conditions of a Slab from the Mountain Limestone of Bolland," by Professor W. C. WILLIAMSON, F.R.S.

In November, 1845, I laid before the Literary and Philosophical Society of Manchester my memoir "On some Microscopic Objects found in the Mud of the Levant and other Deposits; with remarks on the mode of formation of calcareous and infusorial siliceous rocks;" which memoir was published in vol. viii. of the second series of the Society's Transactions. In that memoir I sought to demonstrate two things:—1st, That not only was Chalk made up of microscopic organisms, chiefly Foraminifera, as had recently been demonstrated by Ehrenberg, but that the fact was equally true and explanatory of the origin of all lime-stones except a few freshwater Travertins. 2nd, That some other extensive deposits, of submarine origin, in which no Foraminifera could now be detected, were not in the state in which

they were originally accumulated. I concluded that Foraminifera had doubtless been present in them also, but that their calcareous shells had been dissolved out of them, and that this disappearance had been effected through the agency of water containing carbonic acid, at an early stage of the formation of these deposits. As is well known, this latter theory has been reproduced as a new one by some of the naturalists of the Challenger expedition, who have applied it to the explanation of phenomena of a substantially similar nature to those which I endeavoured to account for, in the same way, more than thirty years previously.

I am indebted for the slab of limestone forming the subject of this communication, to my friends the Messrs. Pattison, the marble merchants of Oxford-street, Manchester. This slab appears to illustrate in an exquisite manner both the theories to which I have just referred. It is a specimen of the Bolland limestone, which, when sawn through, was found to contain a large concamerated Nautiloid shell more than 12 inches in diameter, which appears to me to have been a true Nautilus, though the section has not passed exactly through its centre so as to reveal any portion of its siphuncle. In the various parts of this slab we find the calcareous material exhibiting different conditions. Throughout the greater part of its substance we have evidence that it has originated in an accumulation of minute calcareous organisms—especially Foraminifera—but most of these are disintegrated and display vague outlines, a condition which I presume has resulted from the action of the carbonic acid already alluded to.

Scattered through the slab are numerous dark-coloured

patches of a substance apparently identical with what the late Dr. Mantell designated Molluskite, and which he believed to be the remains of the soft animal substance of marine organisms. In many of these patches the Foraminiferous shells are better preserved than is the case with the rest of the matrix enclosing the large fossil shell. It appears as if this Molluskite had partially protected the calcareous Foraminifera from the solvent action which had disintegrated most of those forming the rest of the deposit.

But the most interesting features of the specimen are seen within the chambers of the Nautiloid shell. The Foraminiferous ooze has entered freely through the large, open mouth of the terminal chamber in which the animal resided and filled the entire cavity of that chamber. There is no doubt whatever as to the original identity in the character of the ooze thus enclosed within the shell and that which constitutes its investing matrix, though they now appear very different. The latter portion was freely permeated by water containing the solvent carbonic acid; hence the more or less complete disintegration of its Foraminiferous shells. But in the limestone enclosed within the large terminal chamber of the Nautiloid shell, almost every Foraminifer is preserved in the most exquisite perfection. This is especially the case in the deeper part of the chamber, most remote from the mouth, as also in the instances of one or two of the more internal closed chambers, into which the mud has obtained entrance through small accidental fractures in the outer shellwall. It appears obvious to me that the thick calcareous shell of the Nautilus has protected the enclosed shells of the Foraminifera from the action of the

solvent acid. I repeat, that there is no room whatever for doubting that the Foraminiferous ooze both contained within and surrounding the Nautiloid shell were originally in identical states. Microscopic observation makes this sufficiently plain. The differences now observable between them have arisen from changes which have taken place subsequent to their primary accumulation, and which changes have been due to differences of position; the one portion has been protected by the thick calcareous Nautiloid shell which would rob the water percolating through it of all its solvent carbonic acid, and thus preserve the contained Protozoa from destruction, and which protection would continue so long as any portion of the Nautiloid shellwall remained undissolved. The other, being unprotected, would be exposed to the full action of the solvent, which would percolate readily amongst the loosely aggregated, microscopic organisms and speedily act upon their fragile shells.

But there is a yet further feature in this interesting specimen requiring notice. The *closed* chambers of the Nautiloid shell are all filled with clear, crystalline, calcareous spar. The acidulated water, acting upon the calcareous Foraminifera of the ooze, has become converted into a more or less saturated solution of bicarbonate of lime. This has passed by percolation through the shell of the Nautilus into its hollow chambers. Finding there suitable cavities, it has gradually filled them up with a crystalline formation of calcareous spar, and which of course exhibits no traces of the minute organisms from which the calcareous matter was primarily derived. A similar crystallisation has filled up

the smaller interspaces between the Foraminiferous atoms both enclosed within, and external to the Nautilus, rendering the limestone capable of receiving a high polish.

If these explanations are as correct as I believe them to be, we have here the entire history of the origin of a limestone rock—from the first accumulation of the Foraminiferous ooze, as seen in the interior of the first large chamber of the Nautilus, to the deposition, in an inorganic mineral form, of the crystallized carbonate of lime within the closed chambers of the Nautilus; all being illustrated within the area of a slab of limestone little more than a foot in diameter.

Ordinary Meeting, January 22nd, 1878.

R. ANGUS SMITH, Ph.D., F.R.S., &c., Vice-President, in the
Chair.

“On the Cubic Integral $u = \int_{\beta}^{\alpha} \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}$ by

ROBERT RAWSON, Esq., Assoc. I.N.A., Hon. Mem. Manchester Literary and Philosophical Society; Mem. of the London Math. Society.

1. As far as I know the cubic integral has received but little attention from mathematicians: the reason for this may be that it has been regarded by them as a particular case of the quartic integral.

This, no doubt, is to a certain extent true; still, the quartic integral is readily reduced to the cubic (see Art. 13) and there are some advantages in considering the cubic integral first, in its natural order, especially so in the reduction of it to an elementary integral of a fractional modulus (n) and amplitude (θ).

The quartic integral here alluded to is usually called elliptic integral. I cannot hope, however, to induce mathematicians to abandon this unnatural terminology, viz. *elliptical integrals* for the more natural one of *quartic integrals*, as suggested by Professor Cayley (see Salmon's Higher Algebra, page 83).

In this paper I have not entered into the great question of the comparison of cubic integrals—my effort here has been of a more limited kind, viz., to reduce the cubic integral to the elementary integral

$$u = \int_0^{\theta} \frac{d\theta}{\sqrt{1 + n \cos. m\theta}}$$

Where (n) is a proper fraction, and (m) any positive whole number. I believe this elementary integral is better adapted

to the purpose of tabulation than is the elementary integral used by Legendre and others, viz.:

$$\int_0^\theta \frac{d\theta}{\sqrt{1 - k^2 \sin^2 \theta}}$$

to which the former integral can be readily reduced. In tracing the curve whose polar equation is $r^2 = 2: \sqrt{1 + n \cos m\theta}$ by polar coordinates r, θ the periodicity of cubic integrals is readily perceived.

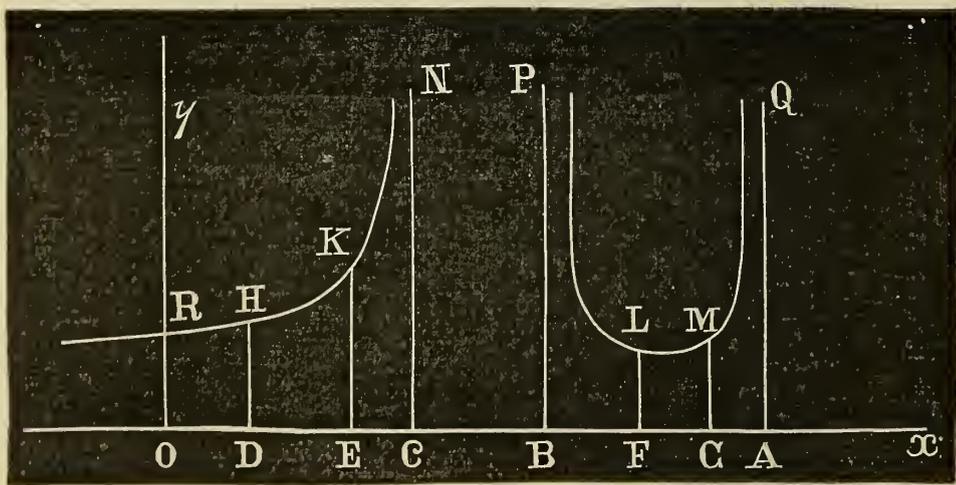
And here I must express my surprise that this method of tracing the cubic integral has not been made use of in tracing elliptic functions. That this has not been done I infer from the circumstance of not finding it in Cayley's recent Treatise on Elliptic Functions.

2. Let a, b, c , be positive, and taken in the order of magnitude, that is $a > b > c$.

If the roots are not positive they can be readily made so by the linear transformation $x = A + z^2$.

With a view, therefore, of fixing palpably the values of the limits a, β with respect to the roots a, b, c , it will be necessary to trace, by the usual rectangular coordinate method, the curve whose equation is

$$y = \frac{1}{\sqrt{(a-x)(b-x)(c-x)}} \dots \dots \dots (1)$$



Let Ox, Oy be rectangular coordinates origin at O .

When $x=0$, then, $OR \sqrt{abc} = 1$. The negative branch of the curve extends to infinity with a continually diminishing

value of the positive ordinate. The positive branch extends only as far as A, where $OA = a$. All values of the ordinate (y) beyond this point are impossible. The ordinate (y) is impossible also between B and C, where $OB = b$ and $OC = c$, since the denominator of (1) is then negative.

- When, $x = a = OA \quad \therefore y = \infty$
- $x = b = OB \quad \therefore y = \infty$
- $x = c = OC \quad \therefore y = \infty$
- $x < a$ and $> b \quad \therefore y$ is real and positive.
- $x < b$ and $> c \quad \therefore y$ is impossible.
- $x < c \quad \therefore y$ is real and positive.

In this geometrical representation of the cubic integral there are two cases to consider, viz. :

1. When the limits a and β are both less than c .
2. When the limits a and β lie between A and B.

In case (1) the cubic integral is correctly represented by the area HDEK, where $OE = a$ and $OD = \beta$.

In case (2) the cubic integral is geometrically expressed by the area LFGM, where $OG = a$ and $OF = \beta$.

3. Transformation of the cubic integral (u) into another cubic integral by relation—

$$x = \frac{2c - a + az}{1 + z} = a - \frac{2(a - c)}{1 + z} \dots\dots\dots(2)$$

From (2), $dx = \frac{2(a - c)}{(1 + z)^2} dz$, $a - x = \frac{2(a - c)}{1 + z}$

$$b - x = \frac{a + b - 2c}{1 + z} \left\{ 1 - \frac{(a - b)z}{a + b - 2c} \right\}, \quad c - x = \frac{a - c}{1 + z}(1 - z)$$

The limits become as follows :—

$$\begin{aligned} \text{When, } x = a \quad \therefore z &= \frac{a + a - 2c}{a - a} \\ \text{,, } x = \beta \quad \therefore z &= \frac{a + \beta - 2c}{a - \beta} \end{aligned}$$

Substitute these values in the cubic integral (u) and it becomes

$$u = \sqrt{\frac{2}{a + b - 2c}} \int \frac{\frac{a + a - 2c}{a - a}}{\frac{a + \beta - 2c}{a - \beta} \sqrt{(1 - z^2) \left(1 - \frac{(a - b)z}{a + b - 2c} \right)}} dz \dots\dots\dots(3)$$

The value of $\sqrt{a+b-2c}$ is real because $a+b-2c$ is positive.

The fraction $a-b$ upon $a+b-2c$ lies between 1 and zero, it is zero when $a=b$ and 1 when $b=c$. At each of these limits the cubic integral is soluble.

4. Transformation of the cubic integral (3) by the relation

$$-z = \cos.m\theta \dots\dots\dots(4)$$

Where (m) is any whole positive number.

From (4) $dz = m\sin.m\theta d\theta = m\sqrt{1-z^2}.d\theta$

Substitute these values in (3) and it becomes

$$u = m \sqrt{\frac{2}{a+b-2c}} \int_{\frac{1}{m} \cos^{-1} \left(\frac{2c-a-\alpha}{a-\alpha} \right)}^{\frac{1}{m} \cos^{-1} \left(\frac{2c-a-\beta}{a-\beta} \right)} \sqrt{1 + \frac{a-b}{a+b-2c} \cos.m\theta} \frac{d\theta}{\dots} \quad (5)$$

Let u_1 represent the integral (5) between the limits $\alpha=c$ and $\beta = -\infty$, then

$$u_1 = \left. \begin{aligned} &\int_{-\infty}^c \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}} \\ &= m \sqrt{\frac{2}{a+b-2c}} \int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1 + \frac{a-b}{a+b-2a} \cos.m\theta}} \end{aligned} \right\} \dots\dots\dots (6)$$

5. The integral in equation (5) applies only to case (1) in Art. 2; the second case requires a slightly different transformation, as follows:—

Transform the cubic integral (u) into another cubic integral by the relation

$$x = \frac{a+b}{2} + \frac{(a-b)z}{2} \dots\dots\dots (7)$$

From (7), $dx = \frac{a-b}{2}.dz$, $a-x = \frac{a-b}{2}(1-z)$

$$-(b-x) = \frac{a-b}{2}(1+z), \quad -(c-x) = \frac{a+b-2c}{2} \left(1 + \frac{(a-b)z}{a+b-2c} \right)$$

The limits become

$$\begin{aligned} \text{when } x = a \therefore z &= \frac{2a - a - b}{a - b} \\ x = \beta \therefore z &= \frac{2\beta - a - b}{a - b} \end{aligned}$$

Substitute these values in the cubic integral (u), then

$$u = \sqrt{\frac{2}{a + b - 2c}} \int_{\frac{2\beta - a - b}{a - b}}^{\frac{2a - a - b}{a - b}} \frac{dz}{\sqrt{(1 - z^2) \left(1 + \frac{(a - b)z}{a + b - 2c}\right)}} \dots\dots\dots (8)$$

6. Transformation of the cubic integral (8) by the relation

$$z = \cos.m\theta \dots\dots\dots (9)$$

where (m) is any whole positive number.

From (8) $dz = -m \sin.m\theta d\theta = -m \sqrt{1 - z^2}.d\theta$, then (8) becomes

$$u = m \sqrt{\frac{2}{a + b - 2c}} \int_{\frac{1}{m} \cos.^{-1} \left(\frac{2\beta - a - b}{a - b} \right)}^{\frac{1}{m} \cos.^{-1} \left(\frac{2a - a - b}{a - b} \right)} \frac{d\theta}{\sqrt{1 + \frac{a - b}{a + b - 2c} \cdot \cos.m\theta}} \dots\dots\dots (10)$$

Let u_2 represent the integral (10) between the limits $a=a$ and $\beta=b$, then

$$\left. \begin{aligned} u_2 &= \int_b^a \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}} \\ &= m \sqrt{\frac{2}{a + b - 2c}} \int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1 + \frac{a - b}{a + b - 2c} \cos.m\theta}} \end{aligned} \right\} \dots\dots\dots (11)$$

By comparing (6) and (11) we obtain

$$\int_b^a \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}} = \int_{-\infty}^c \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}} \dots (12)$$

This result is neat and gives an answer to question 5508 *Educational Times* for December, 1877.

7. Since $2a < 2a$, then $2a - a - b < a - b$; therefore $2a - a - b$ upon $a - b$ and $2\beta - a - b$ upon $a - b$ are always proper

fractions whatever may be the values of the real positive roots a, b, c .

The limits of both (5) and (10), integrals which represent the cases 1 and 2 in Art. 2 respectively, are angles θ_1 and θ_2 which lie between zero and (π) .

Hence, in both cases referred to in Art. 2, the following integral will apply :

$$u = m \sqrt{\frac{2}{a+b-2c}} \int_{\theta_2}^{\theta_1} \frac{d\theta}{\sqrt{1+n\cos.m\theta}} \dots\dots\dots(13)$$

Where $n = \frac{a-b}{a+b-2c}$ $\cos.\theta_1 = \frac{2c-a-a}{a-a}$ or $\frac{2\beta-a-b}{a-b}$
 and $\cos\theta_2 = \frac{2c-a-\beta}{a-\beta}$ or $\frac{2a-a-b}{a-b}$

8. It appears, therefore, that if the elementary integral

$$\int_0^\theta \frac{d\theta}{\sqrt{1+n\cos.m\theta}} \dots\dots\dots(14)$$

is calculated, for any given value of the modulus (n), from $\theta = \text{zero}$ to $\theta = \frac{\pi}{m}$ the result will be a complete solution (arithmetical) of the cubic integral in the case when all the roots are real. The following investigation will show that the same elementary integral will apply equally well when two of the roots are unreal.

9. In the case of the cubic integral having only one real root it assumes the form

$$v = \int_\beta^a \frac{dx}{\sqrt{(a-x)\{(x-b)^2+c^2\}}} \dots\dots\dots(15)$$

where a, b, c are independent constants. The limits a and β cannot be greater than (a) .

Transform the cubic integral (15) by the relation

$$x = \frac{A - B \cos.\frac{m\theta}{2}}{1 - \cos.\frac{m\theta}{2}} \dots\dots\dots(16)$$

where, $A = a + \sqrt{(a-b)^2+c^2}$; $B = a - \sqrt{(a-b)^2+c^2}$

From (16) $dx = m\sqrt{(a-b)^2 + c^2} \frac{\sqrt{1 - \cos.\frac{m\theta}{2}}}{\left(1 - \cos.\frac{m\theta}{2}\right)} d\theta$

$$a - x = \sqrt{(a-b)^2 + c^2} \cdot \frac{1 + \cos.\frac{m\theta}{2}}{1 - \cos.\frac{m\theta}{2}}$$

$$(x-b)^2 + c^2 = \frac{\sqrt{(a-b)^2 + c^2} \{3\sqrt{(a-b)^2 + c^2} + b - a\}}{\left(1 - \cos.\frac{m\theta}{2}\right)^2}$$

$$\left\{ 1 + \frac{(\sqrt{(a-b)^2 + c^2} + a - b)\text{Cos.}m\theta}{3\sqrt{(a-b)^2 + c^2} + b - a} \right\}$$

Substitute these values in (15) observing the change in the limits, then

$$v = \frac{m}{\sqrt{3\sqrt{(a-b)^2 + c^2} + b - a}} \int \frac{\frac{2}{m} \cos.^{-1} \left(\frac{a - a - \sqrt{(a-b)^2 + c^2}}{a - a + \sqrt{(a-b)^2 + c^2}} \right)}{\sqrt{1 + \frac{\sqrt{(a-b)^2 + c^2} + a - b}{3\sqrt{(a-b)^2 + c^2} + b - a} \cdot \cos.m\theta}} \dots\dots\dots(17)$$

It is readily seen that $3\sqrt{(a-b)^2 + c^2} + b - a$ is a positive quantity whatever may be the values of a, b, c . The coefficient of $\cos.m\theta$ is a proper fraction, therefore, equation (17) may be written as follows :

$$v = \frac{m}{\sqrt{3\sqrt{(a-b)^2 + c^2} + b - a}} \int_{\frac{\theta_2}{m}}^{\frac{\theta_1}{m}} \frac{d\theta}{\sqrt{1 + n\cos.m\theta}} \dots\dots\dots(18)$$

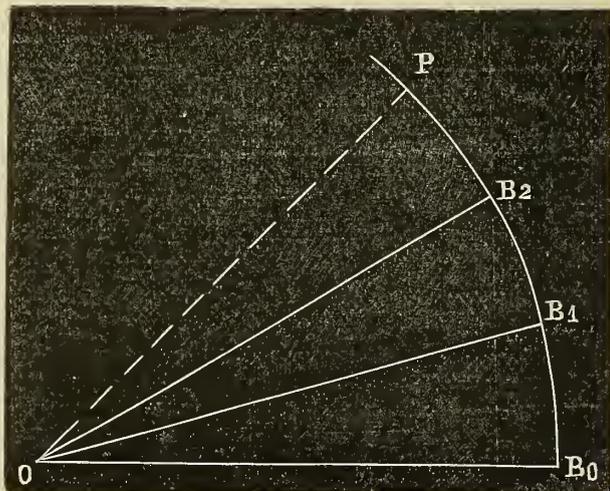
where $n = \frac{\sqrt{(a-b)^2 + c^2} + a - b}{3\sqrt{(a-b)^2 + c^2} + b - a}$, $\text{Cos.}\frac{\theta_1}{2} = \frac{a - a - \sqrt{(a-b)^2 + c^2}}{a - a + \sqrt{(a-b)^2 + c^2}}$

and $\text{Cos.}\frac{\theta}{2} = \frac{a - b - \sqrt{(a-b)^2 + c^2}}{a - b + \sqrt{(a-b)^2 + c^2}}$

10. The comparison of the cubic elementary integral, viz.

$$\int_0^\theta \frac{d\theta}{\sqrt{1+n\cos.m\theta}} \dots\dots\dots (19)$$

with elliptic functions of the first order will be considered hereafter. It will be necessary now to show the march of the integral (19) with respect to polar coordinates in the usual way.



Let B_0P be the polar curve traced by the equation

$$r = \frac{\sqrt{2}}{\{1+n\cos.m\theta\}^{\frac{1}{4}}}$$

where $r=OP$ and $\theta=B_0OP$.

To trace the curve B_0P

$$\begin{aligned} \text{When, } \theta = \text{zero} & \dots\dots\dots \therefore OB_0 = \frac{\sqrt{2}}{(1+n)^{\frac{1}{4}}} \\ \text{,, } \theta = \frac{\pi}{m} = B_0OB_1 & \dots\dots\dots \therefore OB_1 = \frac{\sqrt{2}}{(1-n)^{\frac{1}{4}}} \\ \text{,, } \theta = \frac{2\pi}{m} = B_0OB_2 & \dots\dots\dots \therefore OB_2 = \frac{\sqrt{2}}{(1+n)^{\frac{1}{4}}} \\ \text{,, } \theta = \frac{3\pi}{m} = B_0OB_3 & \dots\dots\dots \therefore OB_3 = \frac{\sqrt{2}}{(1-n)^{\frac{1}{4}}} \\ & \&c. \qquad \qquad \qquad \&c. \end{aligned}$$

It is readily seen that the polar radii from OB_2 to OB_4 are exactly the same as the polar radii included in the angle B^0OB_2 , because $\cos.m\left(\frac{2\pi}{m} + \phi\right) = \cos.m\phi$.

The polar radii on each side of OB_1 are equal at the same angular amplitude, because $\cos.m\left(\phi + \frac{\pi}{m}\right) = \text{Cos}.m\left(\frac{\pi}{m} - \phi\right)$

Therefore, the polar area B_0OB_1 is equal to the polar area B_1OB_2 . From the above considerations the principle of the

periodicity of cubic integrals is readily perceived, and it follows that

$$p \int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1 + n \cos. m\theta}} = \int_0^{\frac{p\pi}{m}} \frac{d\theta}{\sqrt{1 + n \cos. m\theta}}$$

Where, p is any whole number. If the functional notation be used, then

$$pf\left(n, \frac{\pi}{m}\right) = f\left(n, \frac{p\pi}{m}\right) \dots \dots \dots (20)$$

From the figure it will be seen that

$$f\left\{n, \left(\frac{2p\pi}{m} + \phi\right)\right\} - f(n, \phi) = 2f\left(n, \frac{\pi}{m}\right) \dots \dots \dots (21)$$

$$f\left\{n, \left(\frac{2p\pi}{m} - \phi\right)\right\} + f(n, \phi) = 2f\left(n, \frac{\pi}{m}\right) \dots \dots \dots (22)$$

$$\therefore f\left\{n, \left(\frac{2p\pi}{m} - \phi\right)\right\} + 2f(n, \phi) = f\left\{n, \left(\frac{2p\pi}{m} + \phi\right)\right\} \dots (23)$$

where ϕ is less than $\frac{\pi}{m}$.

11. Approximate formula for calculating the cubic integral—

Put $\theta_1 = \frac{\pi}{m}$, $r_0, r_1, r_2, \&c.$, the polar radii corresponding to the angles zero, $\frac{\pi}{pm}, \frac{2\pi}{pm}, \&c.$

It can be readily shown, that

$$\int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1 + n \cos. m\theta}} = \frac{\pi}{6mp} \left\{ r_0^2 + r_p^2 + 4(r_1^2 + r_3^2 + \dots + r_{p-1}^2) + 2(r_2^2 + \dots + r_{p-2}^2) \right\} \quad (24)$$

where (p) is any whole number.

This formula is obtained by supposing the curve B_0P to coincide with the curve whose equation is $r^2 = a_0 + a_1\theta + a_2\theta^2$. The values of $r_0, r_1, \&c.$, are as follows:—

$$\begin{array}{ll} r_0^2 = \frac{2}{\sqrt{1+n}} & r_1^2 = \frac{2}{\sqrt{1+n \cos. \frac{\pi}{p}}} \\ r_2^2 = \frac{2}{\sqrt{1+n \cos. \frac{2\pi}{p}}} & r_3^2 = \frac{2}{\sqrt{1+n \cos. \frac{3\pi}{p}}} \\ \vdots & \vdots \\ r_{p-1}^2 = \frac{2}{\sqrt{1+n \cos. \frac{\pi}{p}}} & r^2 = \frac{2}{\sqrt{1-n}} \end{array}$$

The larger p is the more accurate is formula (24).

12. The reduction of the quartic integral, usually called elliptical integral, to the cubic integral is remarkably easy.

To effect this let the quartic integral be as follows :

$$u = \int_{\beta}^{\alpha} \frac{dy}{\sqrt{(e-y)(f-y)(g-y)(h-y)}} \dots\dots\dots (25)$$

Where, e, f, g, h are in the order of magnitude.

Transform the integral (25) by the relation

$$y = \frac{p+ex}{q+x} = e + \frac{p-eq}{q+x} \dots\dots\dots (26)$$

From (26) $dy = \frac{eq-p}{(q+x)^2} \cdot dx$; $e-y = \frac{eq-p}{q+x}$

$$f-y = \frac{e-f}{q+x} \left(\frac{fq-p}{e-f} - x \right); \quad g-y = \frac{e-g}{q+x} \left(\frac{gq-p}{e-g} - x \right)$$

$$h-y = \frac{e-h}{q+x} \left(\frac{hq-p}{e-h} - x \right)$$

Substitute these values in (25), observing the limits, then

$$u = \sqrt{\frac{eq-p}{(e-f)(e-g)(e-h)}} \int_{\frac{\beta q-p}{e-\beta}}^{\frac{\alpha q-p}{e-\alpha}} \frac{dx}{\sqrt{\left(\frac{fq-p}{e-f}-x\right)\left(\frac{gq-p}{e-g}-x\right)\left(\frac{hq-p}{e-h}-x\right)}} \dots\dots\dots (27)$$

This integral only requires that $eq > p$ in order to obtain its arithmetical value.

If $p = 0$, then (27) becomes.

$$u = \sqrt{\frac{eq}{(e-f)(e-g)(e-h)}} \int_{\frac{\beta q}{e-\beta}}^{\frac{\alpha q}{e-\alpha}} \frac{dx}{\sqrt{\left(\frac{fq}{e-f}-x\right)\left(\frac{gq}{e-g}-x\right)\left(\frac{hq}{e-h}-x\right)}} \dots\dots\dots (28)$$

which is a cubic integral whose roots can be compared with (1). If the roots g, h of eq. (25) are impossible, then equation (28) becomes a particular case of the equation considered in art. (9).

If all the roots in eq. 25 are impossible, then it can be readily reduced to the form

$$u = \int_{\beta}^{\alpha} \frac{dy}{\sqrt{(1+y^2)(1+a^2y^2)}} \dots\dots\dots (29)$$

Transform (29) by the relation

$$y^2 = \frac{1 - \cos.m\theta}{1 + \cos.m\theta} \dots\dots\dots(30)$$

$$\therefore u = \frac{m}{\sqrt{2(1+a^2)}} \int \frac{\frac{1}{m} \cos.^{-1} \left(\frac{1-a^2}{1+a^2} \right)}{\frac{1}{m} \cos.^{-1} \left(\frac{1-\beta^2}{1+\beta^2} \right)} \frac{d\theta}{\sqrt{1 + \frac{1-a^2}{1+a^2} \cos.m\theta}} \dots\dots\dots(31)$$

The equation (31) is, therefore, of the form given in art. (7).

“Notice of a Fossil Plant found at Laxey, in the Isle of Man” by E. W. BINNEY, F.R.S., F.G.S., President.

Many years ago the late Sir William Logan drew attention to the occurrence of fossil plants in the Devonian strata of Canada, and Professor J. W. Dawson, F.R.S., in the Quarterly Journal of the Geological Society, in vols. XV. and XVIII., described and figured some of these specimens. Amongst them was a plant which he designated *Psilophytum*. Dr. S. S. Scoville has since discovered the remains of plants in the Lower Silurians at Longstreet Creek, near Lebanon, Ohio, which Professor Newberry considered as the casts of some large fucoids or marine plants. Count Saporta has found the branch of a fern in the Silurian schists or slates of Angers, France. Professor Leo Lesquereux, to whom we owe so much for his labours in investigating the fossil plants of the United States, in a paper read before the American Philosophical Society, October 10th, 1877, has described and figured a plant from the Lower Heldeberg Sandstone, Michigan, under the name of *Psilophytum cornutum*.

In a paper read by myself before this society on the 26th December, 1876, I stated that after some years' search I had not been able to find the *Palæochorda major* mentioned by Professors Harkness and Nicholson as occurring in the Manx schists in such a state of preservation as to be certain of its

true nature, but I had a fucoid in my possession found by Mr. Grindlay in the drift near Laxey.

As Mr. Lesquereux's specimen so much resembles the one found at Laxey I shall give his description at length.

“*Psilophytum cornutum*, *sp. nov.* Plate 1, *fig. 1.*”

“Stem thick, dichotomous, divisions variable in distance, the terminal ones short, pointed nearly equal in size and length, surface slightly rugose and irregularly striate.

“The branches in the lower part are thick comparatively to their length, three or four millemetres, irregularly striate when decorticated, or merely punctuate upon the thin bark, with small projecting dots resembling the basilar remains of scales or small decayed leaves; lateral branches short, narrowed to a sharp point; the upper or terminal ones about equal in length, appearing like a pair of pointed horns.”

The species is only comparable to some of the fragments not specified but figured by Prof. J. W. Dawson (Geol. Survey of Canada, Fossil Plants of the Devonian and Upper Silurian formations, *figs. 243, 244.* The author remarks “that these fragments are probably originating in the Upper Silurian of Gaspé; that as they are found in the lower part of the limestone which underlies the Devonian Gaspé Sandstone and become more abundant in the upper beds, this suffices to indicate the existence of the neighbouring land, probably composed of Silurian rocks and supporting vegetation.”

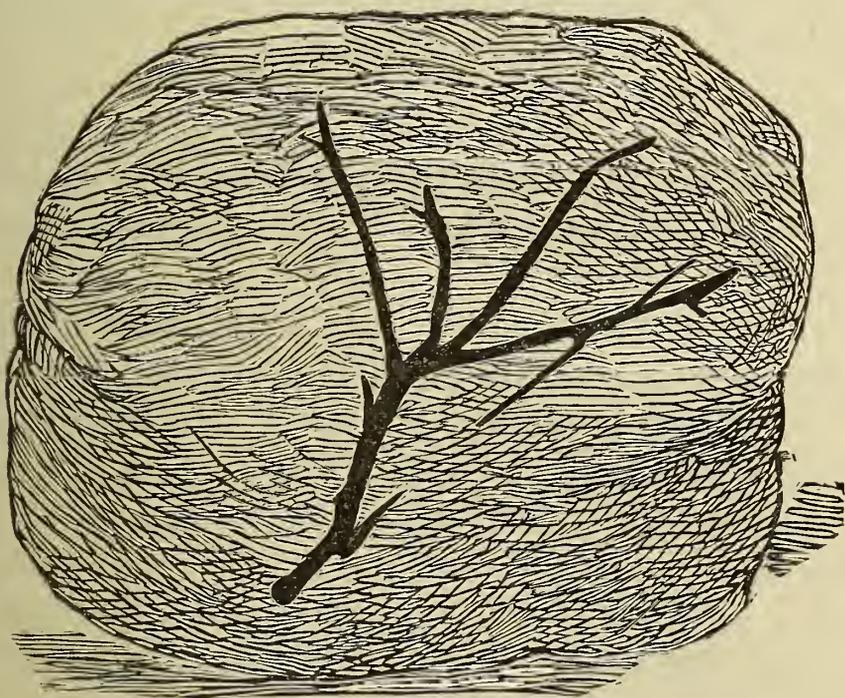
From the preservation of its branches even to the smallest subdivisions, the specimens here represent part of a plant embedded in the place of its growth. The matrice is a piece of very hard calcareous shale seven to eight millimetres thick, bearing on one side irregular undulations like ripple marks, without any trace of organic remains, and on the other the fragments of plants as figured here. The branch in (*a*) represents a different species, and indeed a marine or rather a brackish plant, closely related to the

species of the present genus *Chorda*, Stack. This fragment seems to have been mixed in the tide pools with fresh water or land plants growing there. For another thick specimen of the same locality and compound, bears a profusion of marine mollusks, and has only branches of this as yet undescribed marine species; *Calamophycus septus*.

Habitat Lower Heldeberg Sandstone, Michigan, discovered and communicated by Dr. Carl Rominger (State Geologist).

On comparing my Manx specimen, which was found on the surface in a field at Laxey, with that figured and described by Prof. Lesquereux; it agrees with the latter in every respect, except that striæ and scales are not observable on the stem. The stem is thick, dichotomous; divisions variable in distance, the terminal ones short, pointed nearly equal in size and length, surface nearly smooth. The branches in the lower part are thick comparatively to their length. The surface of the stem appears to be smooth and affords no evidence of striæ or scales.

The wood cut below represents the specimen a little over the natural size.



The stone in which the plant is embedded is a fine grained grit of a grey colour, and the specimen itself is of a

yellow tint, as if coloured by oxide of iron; it runs nearly at right angles to the bedding of the stone, and appears as if standing in the same position as it had grown. The stone is a rolled one but it is evidently from the Manx schists found in the vicinity. These, according to Professors Harkness and Nicholson, are of the age of the Skiddaw slates, but the rock in which the fossil occurs may be of older date, as some of the lower portions of the series have not yet been clearly determined; so here we have evidence of a plant in the lowest part of the Silurian formation, or even lower. By diligent search, the rock in which the specimen occurs may probably be found *in situ* in the upper part of the Laxey valley. The great resemblance, if not identity, of the Manx with the American specimen is very remarkable, and shews the similarity of conditions then prevailing in distant parts of the globe. The specimen might have been called *Psilophytum cornutum*, if any marking on the surface of the stem had been observed, but as these appear to be absent it is proposed to call it *Psilophytum Monense*. As to the nature of the water in which it grew there is no evidence from organic remains, but its characters resemble those of a fucoid more than a land plant.

“On Ptolemy’s Geography, with reference to the Coast from Caernarvon to Cumberland,” by THOMAS GLAZEBROOK RYLANDS, Esq. Communicated by Dr. R. ANGUS SMITH, FR.S., &c.

(The author said, This paper was prepared as “A Chip from my Workshop,” at the request of a friend who knew that I had been engaged for some years investigating Ptolemy’s Geography.)

The special subject of it was selected because it was on the one hand a bit of coast in the far north-west of Ptolemy’s habitable world, respecting which he had no unusual information or interest. It therefore supplies a fair test of his accuracy. On the other hand it is the portion of coast best known to ourselves, and it has been the subject of considerable discussion.

A brief review of what the authorities have taught us shows that we have really up to the present time no reliable data upon which to base an opinion.

After examining the statements of Camden, Horsley, the Whitakers, Ormerod, and others, the measures of the true earth and of Ptolemy's world were given, and, it was shown that, so far as the subject of the present paper was concerned, the proportion of six to five might be safely adopted. This was the ratio of the scales adopted by Dr. Whitaker in the earliest years of the century, and by others since.

Commencing at Ianganorum prom: the positions and the errors of each of the stations in succession were examined and determined, with the following results:—

Tisobius fl :	Traeth Mawr.
Ianganorum prom :	Brach y pwll.
Mona.	Isle of Man.
Seteia Est. :	Dee.
Belisama Est. :	Mersey.
Setantiorum portus :	Ribble.
Moricambe Est. :	Morecambe.
Ituna Est. :	Solway.

The application of the name of Tisobius to the Conway was shown to have resulted from the same editorial error which has cut off the promontory of Caernarvonshire in the whole of the editions of the Geography.

The extraordinary width of the Wirrall, and the consequent differences of opinion as to the true name of the Belisama, was proved to have resulted from the fact that the Seteia is placed too low, and not the Belisama too high. This error being corrected, all the stations are consistent to Morecambe.

The contraction of the coast between Moricambe and Ituna was explained and verified by calculation.

The absence of Anglesea from the Geography of Ptolemy, and the fact that his Mona is the Isle of Man "pure and simple" were elucidated and the paper concluded with some remarks upon the value of the several editions of Ptolemy's work.

The erroneous estimates of the size of the world in the early centuries was compared to our own estimates of the solar parallax. The adoption of the mean degree of five hundred stadia implied neither carelessness nor ignorance.

This is not the only correction required, but it is the only one necessary for the elucidation of the "bit of coast" under consideration. To deal with the general subject much more is required. This caution is necessary, as the numbers employed on the present occasion could not be *generally* applied alone.

This is but a "Chip from the Workshop." The whole subject is preparing for publication. In the process of the work it has been found that the so-called blunders attributed to Ptolemy are, almost all of them, capable of satisfactory explanation when his modes of thought and work are understood.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 3rd, 1877.

A. BROTHERS, F.R.A.S., in the Chair.

Mr. JOHN BOYD exhibited specimens of the Chiego or "Jigger" Flea (*Pulex penetrans*, L.) from Demerara, and explained its method of attacking the feet, thereby causing them to swell, and unless speedily extricated, being the means of danger, sometimes so great as to render amputation necessary. Magnified representations of the mouth maxillary palpi, mandibles, and proboscis of this species and likewise those of *P. irritans* and *P. felis* were shown, and the various distinctions between the species pointed out. The maxillæ are in *P. penetrans* quite rudimentary; and in other respects it is remarkable for its smallness, the size and strength of its lancets, and its peculiar habit of nidification.

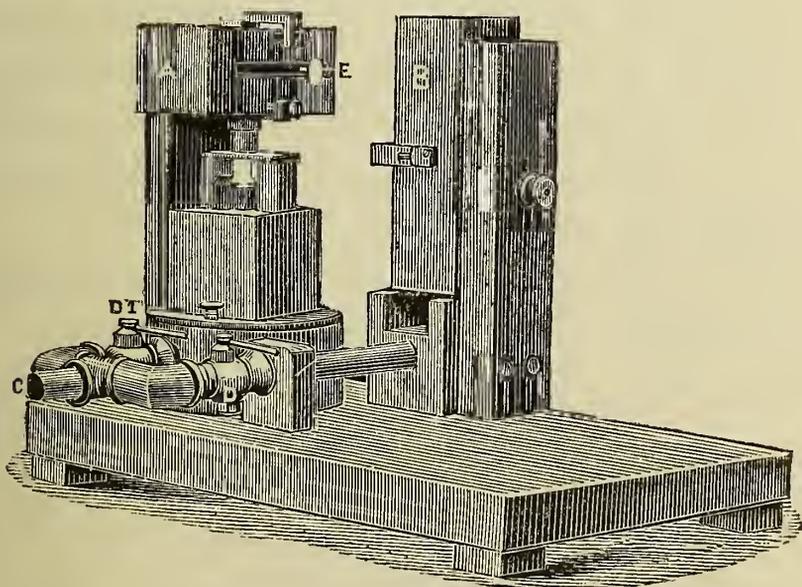
Ordinary Meeting, February 5th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

MR. THOMAS HARRISON, F.C.S., exhibited an apparatus devised by Dr. Dixon Mann to obviate the inconvenience attending the use of tuning forks in the projection of Lissajous' figures on the screen; afterwards he reproduced the figures mechanically, by means of his ingenious optical adaptation to Tisley's Harmonograph for showing its movements on the screen.

“On an Improved Method of Projecting Lissajous' Figures on the Screen,” by J. DIXON MANN, L.K.Q.C.P.

As is well known, the vibrations of tuning forks when used for the production of Lissajous' figures are kept up either by the constant application of the violin bow, or by the aid of an electro-magnet; the former method requiring the presence of two assistants, and the latter adding materially to the complexity of the apparatus and not unfrequently failing to produce the desired result. The difficulty is overcome in the present apparatus by the substitution of harmonium reeds for the tuning forks, the entire instrument being easily controlled by one operator.



The apparatus consists of a base board on which are planted the two reed boxes A and B. The box A is placed horizontally in such a manner as to be capable of slight rotation in the horizontal plane, and also of adjustment in height, by means of the support to which it is attached being provided with a slot and set screw. The box B is permanently attached to the base board in the vertical position. The boxes are so placed that a pencil of light falling directly on E would be reflected to B about one inch from its top; they are furnished with clamping screws for the attachment of the reeds. The boxes are entirely open on the sides facing each other, their margins being covered with soft leather on which the reed plates bed, making a sufficiently air-tight joint. Wind is supplied through the brass tube C which gives off a branch to each box, a stop-cock DD' being inserted in each branch. The reeds are similar to those used in the construction of harmoniums; they are mounted on brass plates which fit the reed boxes. The tongue of each reed is furnished, at its free end, with a small reflector of microscopic covering-glass (E) silvered by Liebig's process, a piece of cork or pith being interposed between the tongue and the reflector, so as to free the latter from the frame of the reed; the reeds are then tuned in the usual manner. It is not necessary that the reeds should be in absolute tune, as, within certain limits, their relative vibrations can be adjusted by means of the stop-cocks, an advantage of great value, believed to be solely possessed by this apparatus.

The reed in the vertical box gives the fundamental ratio of vibrations from which the intervals are built up. Two fundamental reeds are used interchangeably, one giving the double or eight feet C of musicians, the other, being an octave lower in pitch, adds an octave to the intervals obtained from the first fundamental: thus, the third with the first fundamental becomes the tenth with the sub-fundamental.

The horizontal box is furnished with a set of reeds giving all the intervals up to the twelfth, including the unison. The horizontal reeds are changed for the production of the different figures, the fundamental reed being retained.

It was found, when the wind was allowed to pass directly from the bellows to the reed boxes, that the tongues of the reeds were simply blown past the openings in the reed plates and there sustained, no vibration taking place. To obviate this a free space of half an inch was allowed between the supply pipe and the reed box, so as to afford a cushion of air capable of yielding to the elasticity of the tongue. The supply pipe is contracted at its termination to about one-third the size of the hole in the reed box through which the wind enters.

The apparatus is used as follows:—The base board being firmly clamped to a rigid table, one of the fundamental reeds is clamped in front of the box B; another reed, giving the desired interval, is similarly clamped to the box A; an elastic tube, about half an inch in diameter, is attached at one end to the pipe C, and at the other to an acoustic bellows. A fine pencil of light is now thrown on the mirror E, which is then adjusted by rotation of the box A until the light strikes the mirror of the vertical reed, from whence it is reflected on to a screen of tracing paper placed a short distance away; a condenser, interposed between the lantern and the mirror E, focuses the spot of light on the screen. On the bellows being put in motion the figure appears, and can be brought to a perfect stand in any phase of development, looped or cusped, by careful manipulation of one or other of the cocks. The figures are more distinctly seen on the side of the screen away from the apparatus, the room being in total darkness. It is essential to the perfect production of the figures, especially of those resulting from the more discordant intervals, that the entire apparatus should be as rigid as possible, and free from any

vibration other than that produced by the tongues of the reeds, and also that the wind supply should be perfectly steady.

NOTE.—My attention having been drawn by Mr. Baxendell to a paper in the Proceedings of the Royal Society, April 12th, 1877, by Professor McLeod and Lieut. Clarke, “On some figures exhibiting the motion of vibrating bodies, &c.,” I think it right to state that my apparatus was constructed in its present complete form in January, 1872, and exhibited to a number of scientific gentlemen in February, 1872; and further, independently of this fact, that the apparatus of Professor McLeod and Lieut. Clarke (so far as is conveyed by the description in the above-mentioned paper) does not affect the originality of my *schema*.—J. D. M.

Ordinary Meeting, February, 19th, 1878.

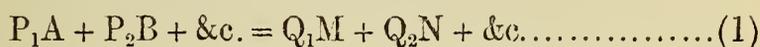
E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

“Note on a Method for Determining the Coefficients in Chemical Equations,” by JAMES BOTTOMLEY, D.Sc.

When two or more chemical compounds act upon one another so as to produce new compounds, provided we know the composition of each, the result may be expressed by a chemical equation. Sometimes, in drawing out the equation, we have some trouble to determine the coefficients to be used with the compounds on each side.

As far as I am aware, the methods used are purely tentative. It occurred to me that in cases where the composition of all the products is known, we might apply the method of indeterminate coefficients as follows:—Let S be the symbol of an element and $a_1 b_1$, &c., the number of atoms of that element occurring in the various compounds entering into the equation.

S_2 and $a_2, b_2, \&c.$, have the same meanings with reference to another element. The letters $a_1, b_1, a_2, b_2, \&c.$, may have any positive integral value including 0. Then a chemical compound may be denoted by $S_1 a_1 S_2 a_2 \&c.$, which for brevity may be denoted by A. So B will denote a compound of the form $S_1 b_1 S_2 b_2 \&c.$, and so on for other compounds. Hence the chemical equation may be written in the form



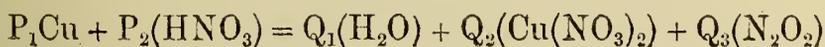
$P_1, P_2, Q_1, Q_2, \&c.$, being undetermined. Then equating the terms containing $S_1, S_2, \&c.$, we have equations of the form

$$\begin{aligned} S_1(P_1a_1 + P_2b_1 + \&c.) &= S_1(Q_1m_1 + Q_2n_1 + \&c.) \\ S_2(P_1a_2 + P_2b_2 + \&c.) &= S_2(Q_1m_2 + Q_2n_2 + \&c.) \\ \dots \dots \dots &= \dots \dots \dots \\ S_n(P_1a_n + P_2b_n + \&c.) &= S_n(Q_1m_n + Q_2n_n + \&c.) \end{aligned}$$

From the above equations the letters $S_1, S_2, \&c.$, might have been omitted, but are retained to show that there will be as many equations as elements. Hence if the number of elements be equal to the number of compounds we shall have sufficient equations to determine $P_1, P_2, Q_1, Q_2, \&c.$ If the number of compounds exceed the number of elements by unity, take any one of the undetermined coefficients, then all the rest may be determined as multiples of that one, and replacing these values in equation (1), both sides will be multiplied by the same arbitrary quantity, which may therefore be omitted.

Within the last few years chemical compounds of considerable complexity have been obtained, and for their quantitative reactions such a method as that suggested might be serviceable. Annexed are two examples of the method applied to well known chemical equations.

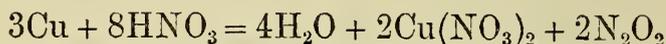
I. Copper and nitric acid yield water, copper nitrate, and nitric oxide.



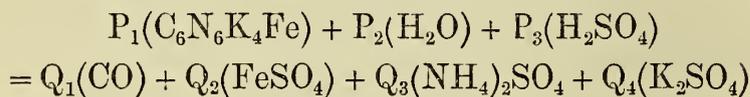
Hence we have the system of equation

$$P_1 = Q_2, P_2 = 2Q_1, P_2 = 2Q_2 + 2Q_3, 3P_2 = Q_1 + 6Q_2 + 2Q_3.$$

Hence we get $P_2 = \frac{2}{3}P_1$, $Q_1 = \frac{1}{3}P_1$, $Q_2 = P_1$, $Q_3 = \frac{2}{3}P_1$, substituting in the equation and multiplying each side by $\frac{3}{P_1}$ we get the usual equation



II. Potassium ferrocyanide, water, and sulphuric acid yield ferrous sulphate, ammonium sulphate, potassium sulphate, and carbonic oxide.



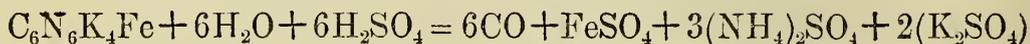
Here the group SO_4 may be treated as an element. From the above equation we get the subsidiary equations

$$6P_1 = Q_1, 6P_1 = 2Q_3, 4P_1 = 2Q_4, P_1 = Q_2, 2P_2 + 2P_3 = 8Q_3, P_3 = Q_2 + Q_3 + Q_4.$$

Hence we get

$$Q_1 = 6P_1, Q_2 = P_1, Q_3 = 3P_1, Q_4 = 2P_1, P_2 = 6P_1, P_3 = 6P_1$$

Replacing these values in the equation and dividing both sides by P_1 we get



which is the equation given by Fownes.

General Meeting, March 5th, 1878.

Dr. R. ANGUS SMITH, F.R.S., &c., Vice-President, in the
Chair.

Mr. Andrew Fairgrieve, of 4, Chester Street, Chorlton-on-Medlock, was elected an Ordinary Member of the Society.

Ordinary Meeting, March 5th, 1878.

Dr. R. ANGUS SMITH, F.R.S., &c., Vice-President, in the
Chair.

“On the decomposition of Ultramarine by Carbonic Acid,” by Mr. S. SUGIURA (Student in the Chemical Laboratory of the Owens College). Communicated by Professor ROSCOE, F.R.S., &c.

It has been proposed to employ ultramarine for the detection of free hydrochloric acid in presence of carbonic acid, on the ground that it is not acted upon by the latter, while the former easily attacks it.* In making some experiments in connection with this subject, I found however that carbonic acid itself decomposes ultramarine, as the following experiments will show.

A glass cylinder about a foot in height and an inch in diameter was filled with distilled water, and carbon dioxide evolved from sulphuric and sodium bicarbonate or hydrochloric acid and calcspar, was allowed to bubble up through

* H. Müller, *Chem. Soc. Jour.*, N. S., viii. 38.

the column of the liquid. In order to be perfectly sure of the purity of carbon dioxide gas, it was passed through a series of flasks containing distilled water, and then through a long tube containing solid sodium bicarbonate, to keep back any free acid which might be mechanically carried over.

After passing a steady current of the gas through for 15 minutes, the water in the cylinder was tested for acids other than carbonic. The absence of sulphuretted hydrogen was also proved by exposing a paper moistened with lead acetate solution.

Then about .07 grams of ultramarine infused in 2 cc. of distilled water were added to the water in the cylinder. Within 5 minutes the paper turned brown, showing the presence of sulphuretted hydrogen, which therefore can only be attributed to the decomposition of ultramarine.

In order, however, to ascertain the presence of any alkaline sulphide existing as such and not as a constituent of ultramarine, a large quantity of the latter was treated with distilled water and quickly filtered. The filtrate contained no sulphide. Hence the evolution of sulphuretted hydrogen was really due to the decomposition of ultramarine by the carbonic acid. The colour of the pigment was dulled, but could not be destroyed completely even when metallic chloride existed in the solution.

Through the kindness of Messrs. Jewsbury and Brown, I got a dozen bottles of aerated water, one half of which were charged with 2 grains of sodium chloride in each. The effects of these waters upon ultramarine were compared together week after week by putting .07 grams of the pigment into the cylinder containing these liquids. In all cases the following effect was observed: the decomposition of ultramarine did not commence at once, but on standing for about an hour the paper moistened with lead acetate solution and placed over the cylinder began to be blackened.

A few minutes afterwards the odour of sulphuretted hydrogen was distinctly observed. To prove the absence of sulphuretted hydrogen in laboratory air, a similar paper was placed over a jar containing the infusion of ultramarine in distilled water, which remained unchanged during the experiments.

These experiments, though qualitative, show that carbon dioxide in aqueous solution, viz. carbonic acid, can attack ultramarine, and therefore the latter is not a safe indicator of free hydrochloric acid in presence of carbonic if the former exist in very small quantity.

“On Siliceous Fossilization,” by J. B. HANNAY, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College, communicated by Professor ROSCOE, F.R.S., &c.

My friend Professor Young having mentioned to me that certain questions connected with fossilization were in a very undecided state, and having expressed his opinion that a chemical examination was really what was required to throw light on the question, I obtained from him some siliceous fossils in which he was interested, and of which he and Mr. J. Young, F.G.S., have published an account,* and proceeded to make an examination of them by chemical means. The rods were samples of *Hyalonema Smithii*, and, as they were in the fossil state siliceous, and probably had been so in the original state, I took for comparison in my examination of them, some rods of a recent siliceous sponge. On examination of the fossil rods it was seen that they might roughly be divided into two classes: I. rods partly transparent or opalescent, and containing some chemically combined water, and II. rods opaque, and apparently consisting of anhydrous crystalline silica, both classes, however, containing many samples stained brown by ferric oxide. Thus I gen-

* Annals of Nat. Hist., Vol. 20, No. 119, P. 425.

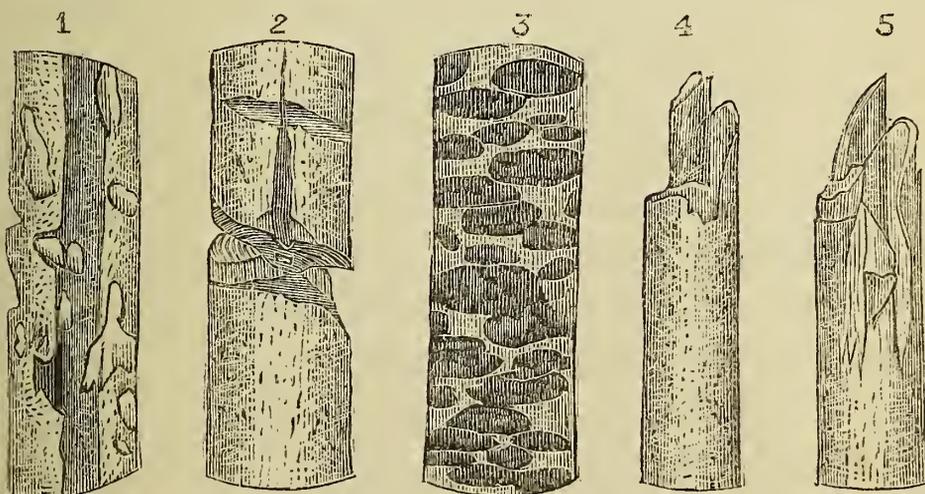
erally experimented with three different kinds, which shall be referred to as

- I. Recent quite transparent.
- II. Fossil opalescent.
- III. Fossil opaque.

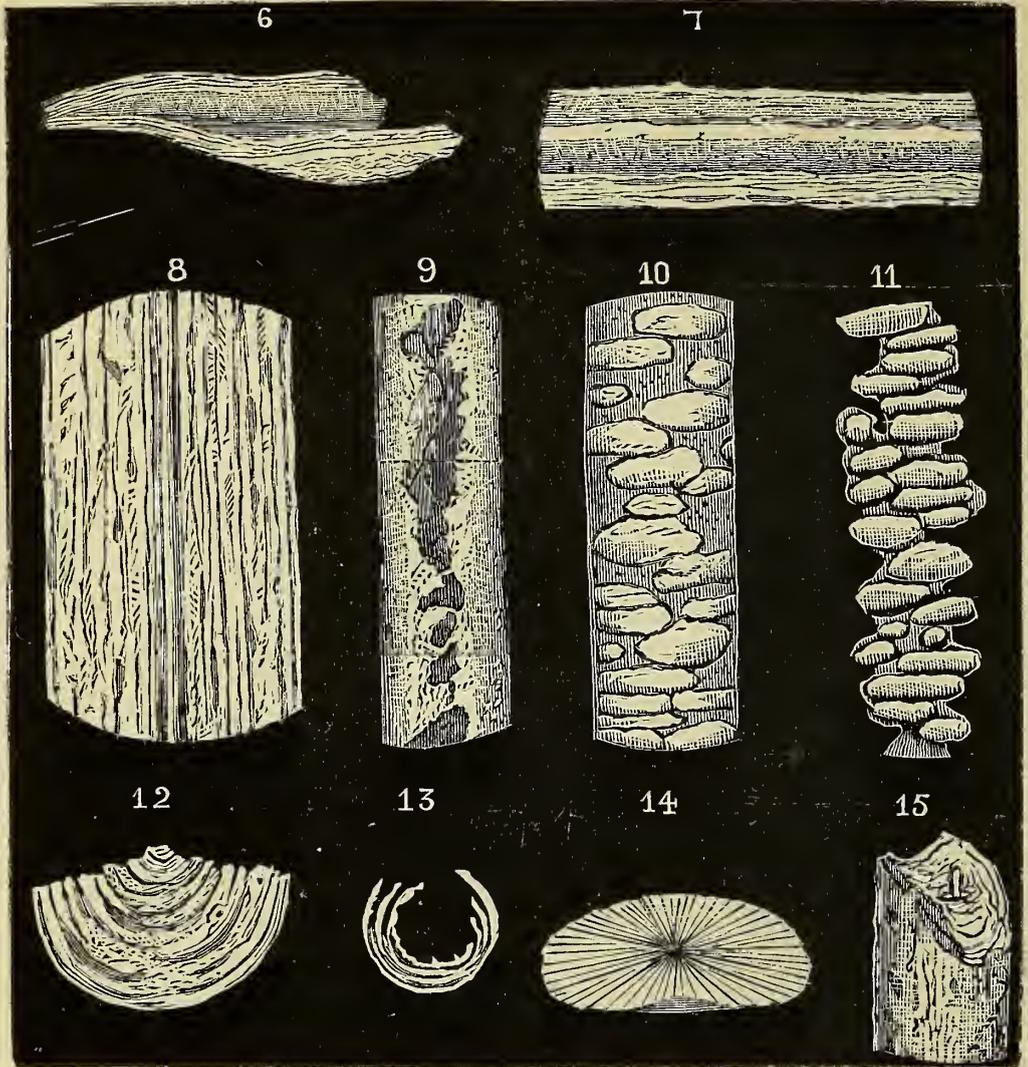
To find the general differences displayed by the three kinds, several experiments with various chemicals were first performed, placing all the three kinds in the same vessel under the same circumstances. On placing the three kinds in hydrofluoric acid it was seen that the recent rod was dissolved in about 30 minutes, the opalescent in about two hours, and the opaque rod only partially dissolved after four hours immersion in the acid. Sometimes the opaque rod dissolved irregularly, leaving protruberances, and sometimes leaving a spongy substance covering the still undissolved interior. On heating the three kinds in a four per cent. solution of caustic soda in a sealed tube to a temperature of 110° C., so that they were under pressure, the recent rods dissolved in about 10 minutes, No. II. was nearly all dissolved in 30 minutes, leaving only a thin core of anhydrous silica, while after two hours No. III. was only irregularly eroded, and in some places seemed very little affected. On heating the three specimens to 100° with water for two days very little change was observed, in fact, water at the boiling point, was found to be without practical effect, but when heated to 150° C. in a sealed tube, so that it was under a pressure of several atmospheres, the water had the effect of making No. II. much more transparent. It seemed then that at this high pressure that the substance took up water, and so was rendered more similar to the recent sponge rods. I found, however, that if a substance like ether, which boils at a much lower temperature, were introduced into the tube above the water, so that the pressure would increase much more rapidly than with water, the rods might be hydrated at a lower temperature. On heating the rods

by themselves No. I. gave off water and became friable sometimes exfoliating, No. II. also gave off water and became opaque (like No. III.) and at the same time friable, while No. III. underwent no change.

The general conclusions to be drawn from these experiments are that the principal difference between the three kinds of rods lies in the relative proportions of water they contain, and that the larger proportion of water contained in any sample, the more transparent is it. From even a cursory examination of the rods, it may be seen that as the proportion of water decreases the original features of the rods become more and more obliterated, so that a totally dehydrated rod is reduced to a mass of crystalline inorganic silica. It will be seen that an examination of the state of combination of the water in the different rods would be likely to throw light on their constitution; but, owing to the small quantity of rods at my disposal, the method for thus examining substances, which I have published elsewhere,* was in this case inapplicable, an optical examination being the method which promised to yield the best results. Heating the rods with caustic soda solution did not give results throwing any light on their structure, the general effect of this reagent being to erode them, as shown in Fig. 1. An attempt was made to gain a knowledge of



* Examination of Substances by the time method. Chem. Soc. Journ., No. CLXXVII.



the structure of the rods by imbedding a specimen of No. II. in Canada balsam so as to leave only one-half exposed, and then to etch away this upper portion by means of hydrofluoric acid. This method succeeded pretty well, giving a sectional view of the internal structure of the rod, as shown in Fig. 7. The layers are seen by this method to be pretty clearly marked, but the etching is very irregular.

The next method tried was rendering the opalescent nodules transparent by means of high-pressure steam, or rather water, as the substance was always surrounded by water, even when the temperature rose to 150°C . A fractured rod (Fig. 2), thus rendered transparent, shows on the fracture plain markings of the layers composing the rod cylindrically arranged. It was found that when a rod was

heated with water in a tube to 150° for five or six hours, the side of the rod next the glass became dissolved, this action seeming to be due to the solvent action of the soda of the glass, as water at that temperature decomposed the glass, removing the alkali. Fig. 9 shows a rod so treated, and Fig. 13 shows a section of that rod through the dotted line in Fig. 9. This action clearly shows at least four layers in the rod. Fig. 15 shows the action of water containing a small quantity of alkali at 150° on a rod the end of which has dissolved to a considerable extent, leaving a hard core of anhydrous silica, which, as my former experiments showed is unacted on by dilute caustic soda.

It occurred to me that if a section were first prepared by grinding or splitting, and this section hydrated as much as possible, and then acted upon by the hydrofluoric acid, it might show the state of the rod, as the hydration of the silica would restore the rod to its original state, and the etching action of the acid would show what that state was.

The experiment was carried on in this way. A rod was split by mechanical means and placed in a thick short tube sealed at one end along with some water. A thin tube much smaller than the other was filled with ammonium fluoride solution, sealed and placed in beside the nodule, and the larger tube was then sealed off and heated in the air bath to 150° C. In rather less than half an hour the ammonium fluoride dissolved its way through the thin tube, and escaping into the larger tube etched the split rod. By these means the silica of the rod was first hydrated and whilst in this state was etched by the fluorine. My reason for wishing the etching to go on after the hydration and without removing the rod or cooling it, was that when the rod is removed from the superheated water it begins immediately to lose its water, that is, the hydration is only temporary. By the above method the rod was not allowed to become dehydrated, but was etched just as the hydration was going on.

Figs. 8 and 12 are specimens obtained by this method, and they show the layer formation to perfection, as many as fifteen layers being shown by one specimen.

Fig. 12 is the end of Fig. 8, and it shows the concentricity of the layers very plainly.

Fig. 6 is a small splinter, the centre of which was in a higher state of hydration than the exterior, so that when treated with hydrofluoric acid it left a hollow interior. The portion left shows the layers also.

I noticed when rendering some of the opalescent rods transparent that they did not all show the same equally distributed transparency, but when viewed by transmitted light dark rounded patches of opaque matter appeared irregularly throughout the transparent portions, as shown by Fig. 3. When viewed by reflected light they showed themselves as white patches upon the opalescent ground, as in Fig. 10. It was found that by heating these rods in dilute caustic soda solution (about 2 per cent.) in sealed tubes to 130° C., the transparent portion could be dissolved away, leaving rounded nodules held together by a portion of the opalescent substance, which, viewed by reflected light, had the appearance given in Fig. 11.

Dr. Young has figured in his paper, rods which are made up of nodules, and which present very much the same appearance as shown in the above figure if the whole of the opalescent portion were dissolved away, and it would appear as though they had been formed in somewhat the same way, that is by the solution of the more highly hydrated portions. The question is, how are the rounded nodules formed in the rods? One of the largest I could find was carefully split and its structure examined, and it was seen when viewed under the microscope by reflected light to have the appearance presented in Fig. 14, that is, it had the character of a radiating nodule of crystalline silica.

Now it appears from the above evidence that in this case

of siliceous fossilization the process goes on in something like the following way. The clear transparent rods first lose a portion of their water, becoming transformed into the opalescent or semi-gelatinous-looking form No. II, and in so doing they begin to lose some of their internal structure, that is, the layers adhere more closely and the rods become brittle and lose the glass-like flexibility they originally possessed. At this point any further loss of water causes crystallization to set in, the silica not being able with a lower proportion of water to remain in the "organic" state, and if this crystallization sets in at a great many points the nodule soon becomes an opaque mass of crystalline silica as in variety No. III; but if crystallization sets in at comparatively few places and spreads slowly out, it forms the kind of nodules drawn in Fig. 10. If at this point any solvent agent comes in contact with the rod it dissolves away the non-crystalline portion, leaving nodules as shown by Dr. Young and in Fig. 11; but if no solvent action goes on the rod ultimately becomes transformed into variety III, which is crystalline silica.

The transformation from "organic" to "inorganic" silica is one of arrangement, and as the "organic" arrangement requires a certain amount of water, whenever the proportion falls below this a simpler arrangement sets in, and we have crystalline silica. That the rods of *Hyalonema Smithii* are the same in constitution at least as those from modern sponges is I think proved from these experiments, and to show a further analogy I have drawn a fractured specimen of such in Figs. 4 and 5, the similarity of which is very striking.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 11th, 1878.

J. BARROW, Esq., in the Chair.

Mr. BINNEY, F.R.S., sent a Marine Alga from the Isle of Man for identification. It was not in fruit, but was undoubtedly an unusually narrow fronded form of *Chondrus crispus* (Lamx.).

Mr. C. BAILEY, in the absence of Mr. Hurst, read a paper by the latter "On the Best Method of Collecting and Preserving Plants for Herbarium Purposes, when gathered in Tropical or Subtropical Countries."

Mr. J. BOYD exhibited slides of *Spongilla fluviatilis*, the fresh-water sponge, showing spicules.

 PHYSICAL AND MATHEMATICAL SECTION.

December 4th, 1877.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section,
in the Chair.

"Transit of the Shadow of Titan across the disc of Saturn, November 23rd, 1877," by JOSEPH BAXENDELL, F.R.A.S.

The weather at Birkdale on the evening of the 23rd of November was stormy with thunder, and showers of rain

and hail ; but between the showers the sky cleared partially and afforded opportunities, at irregular intervals, of observing the shadow of Titan on the disc of the planet. Thirteen estimations of its position at different times during the transit were made, a reduction of which gave 7h. 23·5m. as the Greenwich mean time of mid-transit, or time when the shadow was in the middle of its track ; and 3"·9 as its distance from the centre of Saturn's disc at that time. Its path appeared to be on a faint, narrow double belt. The satellite itself was estimated to be on a line perpendicular to the preceding end of the ring at 8h. 17m. All the estimations were made with a power of 180, but the shadow was distinctly visible with a power of 125, the telescope used being an equatorially mounted achromatic of 6 inches aperture by Messrs. T. Cooke and Sons, of York. The frequent interruptions by showers of rain and hail and the disturbed state of the atmosphere rendered it impracticable to use the micrometer with advantage.

February 25th, 1878.

JOSEPH BAXENDELL, F.R.A.S., President of the Section,
in the Chair.

“ Results and deductions of rain-gauge observations made at Eccles, near Manchester, during the year 1877,” by THOMAS MACKERETH, F.R.A.S., F.M.S.

The rainfall of the past year was greatly in excess of the average. The average fall at Eccles for a period of 17 years is 36·123 inches, and the average number of days upon

which rain fell for the same period is 208. But the rainfall of last year was 45·175 inches, showing an excess of 9·052 inches or about the average fall of rain for a quarter of a year, and the number of days on which rain fell was 250, showing almost as great an increase in the number of wet days for the year as the excess of rainfall. I have looked through my register of the last 17 years to see if any rule could be observed relating to the excess and deficiency of rainfall. I find that in 13 of those years there are two epochs of dry and wet years, with intervals closely resembling those of the intervals of the sun-spot periods: for example 1865 was a very dry year, having a rainfall of 27·809 inches on 177 days, and the fifth following year, viz, 1870 was the next dry year, having a rainfall of 30·404 inches on 178 days; the next interval is one of three years, viz, 1873, when the rainfall was 31·127 inches on 219 days. Then with regard to the wet years, I find in the same number of years that 1866 was very wet, having a rainfall of 43·076 inches on 232 days, and that the sixth following year was the next very wet one, having a rainfall of 48·416 inches on 264 days. Then after an interval of five years there comes another very wet one, viz., in 1877, when the rainfall was 45·175 inches on 250 days. Thus in an interval of nine years there are three dry years, and in an interval of 12 years there are three wet years.

The following table shows the results obtained from a rain-gauge with a 10in. round receiver placed 3ft. above the ground.

Quarterly Periods.		1877.	Fall in Inches.	Average of 17 years.	Differences	Quarterly Periods.	
Average of 17 years.	1877.					Average of 17 years.	1877.
52	72	January	5·277	2·940	+2·337	7·734	11·927
		February	4·192	2·337	+1·855		
		March	2·458	2·457	+0·001		
46	48	April	2·073	1·994	+0·079	6·765	7·028
		May	3·059	2·110	+0·949		
		June	1·896	2·661	-0·765		
53	61	July	5·518	3·359	+2·159	10·900	12·808
		August	4·880	3·439	+1·441		
		September	2·410	4·102	-1·692		
57	69	October	4·038	4·216	-0·178	10·724	13·412
		November	5·540	3·448	+2·092		
		December	3·834	3·060	+0·774		
208	250		45·175	36·123	+9·052	36·123	45·175

In the next table are given the results obtained from rain gauges of two different kinds, placed in close proximity in the same plane, and 3ft. from the ground, the one has a 10in. round receiver and the other a 5in. square one. The results, as shown in the table, explain themselves, and show that their monthly and annual differences are extremely small, so that the gauges are good checks upon each other.

1877.	Rainfall in inches in 10in. Round Receiver, 3ft. from the Ground.	Rainfall in inches in 5in. Square Receiver, 3ft. from the Ground.	Differences.	From 1867 to 1877.		Differences.
				Average of 10 years Rainfall in inches, in 10in. Round Receiver, 3ft. from the Ground.	Average of 10 years Rainfall in inches, in 5in. Square Receiver, 3ft. from the Ground.	
January ...	5·277	5·297	+·020	3·178	3·228	+·050
February ..	4·192	4·212	+·020	2·391	2·374	-·017
March ...	2·458	2·439	-·019	2·375	2·388	+·013
April	2·073	2·011	-·062	2·002	1·983	-·019
May	3·059	3·095	+·036	2·021	1·997	-·024
June	1·896	1·928	+·032	2·511	2·525	+·014
July	5·518	5·624	+·106	3·394	3·377	-·017
August ...	4·880	4·950	+·070	3·545	3·532	-·013
September	2·410	2·408	-·002	3·871	3·829	-·042
October ...	4·038	4·035	-·003	4·689	4·674	-·015
November	5·540	5·447	-·093	3·493	3·508	+·015
December	3·834	3·816	-·018	3·302	3·342	+·040
	45·175	45·262	+·087	36·772	36·757	-·015

In the next table I give the results obtained from two exactly similar gauges, placed at different heights from the ground and free from every interference; each gauge has a 5in. square receiver, and the one is placed 3 feet, and the other 34 feet above the ground. The total fall for the year in the one 3 feet from the ground was 45·217 inches, and in the one 34 feet from the ground was 36·576 inches. The difference between the fall in the two gauges is 8·641 inches, or about 19 per cent. less rain fell in the higher gauge than in the lower. Last year this difference amounted to only 15 per cent., but the moisture of the atmosphere was greater during the past than in the previous year. The table shows that in an average of 10 years the difference amounts to about 18 per cent.

1877.	Rainfall in inches in 5in. square receiver 3ft. from the ground	Rainfall in inches in 5in. square receiver 34ft. from the ground	From 1867 to 1877.	
			Average fall of rain in inches in 5in. square receiver 3ft. from the ground	Average fall of rain in inches in 5in. square receiver 34ft. from the ground
January	5·297	4·049	3·228	2·319
February	4·212	3·446	2·374	1·836
March	2·394	2·088	2·388	1·899
April	2·011	1·663	1·983	1·660
May.....	3·095	2·635	1·997	1·789
June	1·928	1·536	2·525	2·176
July.....	5·624	4·562	3·377	3·002
August	4·950	4·286	3·532	3·004
September.....	2·408	2·134	3·829	3·309
October	4·035	3·220	4·674	3·915
November	5·447	4·167	3·508	2·782
December	3·816	2·790	3·342	2·651
	45·217	36·576	36·757	30·342

The following table gives the ratios of the excesses of rainfall 3ft. from the ground over the amount measured at 34ft. from the ground. As I have previously pointed out, no regularity exists in the succession of these ratios during

the months of a single year; and if their difference depends upon the relative amount of the humidity of the atmosphere in each month, then it is clear that no regularity for a single year is to be expected. But if an average of six years be considered or the average of any number of years above six, and the following table presents an average of 10 years, then a definite order is manifest. According to this order, December and January are the most humid months of the year, and May and June are the driest. And if the theory be true as to the cause of these differences, then during the past year the atmosphere was driest in the following months, September, March, August, May, April, and February, and was most humid in December, January, November, July, June, and October. The fall of rain in the months when the atmosphere was driest was 19·072 inches, and in the months when it was most moist was 26·103 inches, being a difference of 7·031 inches. In the same table I have given the ratios of this difference or excess of rainfall for 10 years, and placed them in juxtaposition with the mean humidity of the atmosphere calculated from observations of the dry and wet bulb hygrometer, by means of Glaisher's tables. A glance shows that the results are identical, and points almost certainly to the cause of the excess of rainfall between the two gauges, namely, the moisture of the atmosphere.

But that another supposed cause may be fairly discussed I have prepared a table which gives the ratios of the differences of rainfall in comparison with the calculated amount of moisture in the air, and the ratios of the relative amount of the movement of the air for every day of rainfall for the past year. From this table it will be seen that almost in

every case the difference of rainfall between the two gauges is as the difference of the moisture of the atmosphere ; but if these differences be compared with the ratios of the amount of wind on each day of rainfall, it will be found that there is little or no comparison. For example, though in November there was above the average amount of wind, and an apparent consequent large difference between the rainfall in the two gauges, yet in June and July, when the amount of wind was below the average, a similar difference of rainfall happened ; and again, though the amount of wind in May was about equal to that in October, yet there is a great difference in the difference of rainfall.

The wind may play some part in causing this difference of rainfall between the two heights, but I think it is only a secondary cause, that is, when it brings with its force a large supply of relatively moist air. But when it brings a supply of dry air on a rainy day, though there be a strong current of wind blowing, the difference of rainfall will be very small, as illustrated in the amount of rainfall and wind movement in May of the past year.

1877.	Ratios of such Rainfall for 1877.	Ratios of such Rainfall for Average of 10 years from 1867 to 1877.	Mean Humidity of the Atmosphere for 10 years from 1867 to 1877.
January	·764	·718	·872
February.....	·818	·773	·857
March	·872	·795	·833
April	·826	·837	·775
May	·851	·896	·753
June	·796	·861	·743
July	·793	·888	·763
August.....	·865	·850	·793
September	·886	·864	·779
October	·798	·837	·836
November	·765	·793	·854
December	·731	·793	·873
Annual Ratios.....	·808	·825	·810

1877.	Ratios of the Amounts of Rainfall from the previous Table for 1877.	Mean Humidity of the Atmosphere for 1877. 1,000= full Saturation.	Ratios of the Mean Movement of the Wind on the days of Rainfall.
January	·764	·875	175·9
February	·818	·864	156·2
March	·872	·848	111·3
April	·826	·781	99·5
May.....	·851	·744	161·1
June	·796	·704	122·5
July	·793	·822	107·8
August	·865	·813	89·0
September	·886	·782	114·9
October	·798	·821	168·9
November	·765	·846	203·4
December	·731	·883	156·1
Means	·808	·815	138·8

In the next table I give the fall of rain for 1877, during the day from 8 a.m. to 8 p.m., and the fall during the night from 8 p.m. to 8 a.m. From 1868 to 1873 the day fall is always greater than the night fall. In 1874 the night fall was in excess, and in 1875 the day fall was again in excess; and in 1876 and last year the night fall was in excess, but at most, either way, the difference between them is very small.

1877.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	2·749	2·548	—0·201
February	1·626	2·586	+0·960
March	0·978	1·461	+0·483
April	1·346	0·665	—0·681
May.....	1·662	1·433	—0·229
June	1·382	0·546	—0·836
July.....	2·551	3·073	+0·522
August	3·107	1·843	—1·264
September	0·857	1·551	+0·694
October	1·446	2·589	+1·143
November	2·675	2·772	+0·097
December	1·399	2·417	+1·018
	21·778	23·484	+1·706

In the next table I present the average day and night fall for a period of ten years. The results of this table con-

tinue to show that the day fall exceeds the night fall so far as the whole year is concerned. In the first three months of the year the night fall exceeds the day fall. Then there is the reverse of this from April to July or August, and September and December again show excesses for the night.

AVERAGE OF TEN YEARS, FROM 1867 TO 1877.

1877.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	1·486	1·742	+0·256
February	1·023	1·351	+0·328
March	1·190	1·198	+0·008
April	1·150	0·833	-0·317
May	1·159	0·838	-0·321
June	1·510	1·015	-0·495
July	1·782	1·595	-0·187
August	1·772	1·760	-0·012
September	1·756	2·073	+0·317
October	2·378	2·296	-0·082
November	1·785	1·723	-0·062
December	1·423	1·919	+0·496
	18·414	18·343	-0·071

Errata.

Page 96—line 3—for $Q_3 = \frac{2}{3}P_1$ read $Q_3 = \frac{P_1}{3}$

„ line 6—for $2\text{Cu}(\text{NO}_3)_2 + 2\text{N}_2\text{O}_2$ read $3\text{Cu}(\text{NO}_3)_2 + \text{N}_2\text{O}_2$

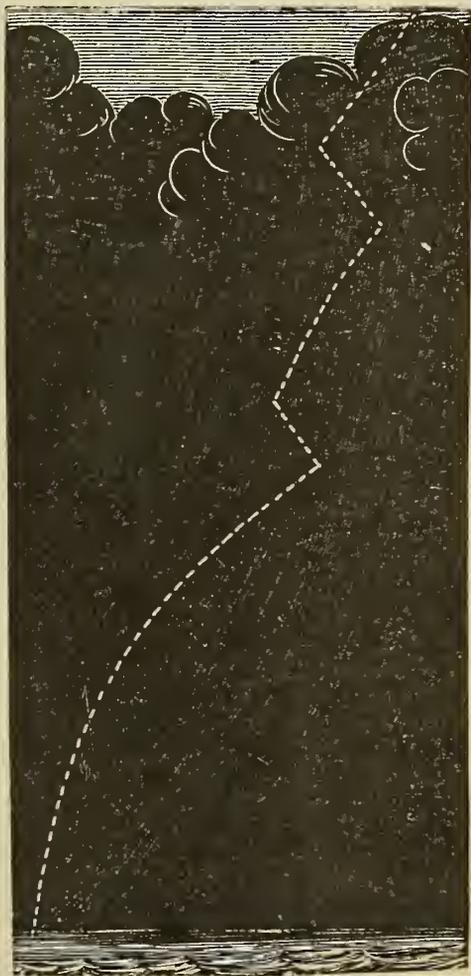
Ordinary Meeting, March 19th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

“On a remarkable Flash of Lightning,” by B. St. J. B. JOULE, Esq.

On the evening of the 16th of August last year, a heavy thunder-storm took place in this vicinity. It was preceded by a fall of the barometer not exceeding $\frac{1}{16}$ of an inch, the wind at 1 o'clock p.m. being west, backing gradually until at 9 o'clock p.m. it was south. At the time of the storm to which my present observations refer it was south-west, and consequently its direction was nearly parallel with the coast line.

I was standing at the shore end of Leicester Street, watching the approach of the storm, and observing the progress and direction of the more important flashes, when about 8 o'clock a vivid flash of lightning fell apparently into the channel (the water being not much above low water mark of a neap tide) about $\frac{1}{6}$ of a mile north of the end of the pier. In about a minute afterwards another fell about $\frac{1}{6}$ of a mile north-east of the previous one, and after a similar interval a third stream of electricity descended about another $\frac{1}{6}$ of a mile in the same direction. The first and third flashes were of the usual character of forked lightning, but the second presented an appearance which I do not recollect to have witnessed before. From its exit from the clouds to its fall into the sea it seemed composed of small detached fragments which caused it to assume the aspect depicted in the margin.



On the following day, in the course of a conversation respecting the storm of the previous evening, I mentioned the phenomenon to Mr. Thistlethwaite, who informed me that he had been particularly struck by the extraordinary appearance of this singular flash, which he had observed whilst sitting in the "parsonage" (the house adjoining the south-west side of the Manchester and Liverpool District Bank), and which to him appeared exactly as I have depicted it. This gentleman could, however, have seen the upper portion of the flash only, as the houses in Lord Street and on the Promenade intervened between his point of observation and the shore.

Heavy rain seemed to follow in the wake of the third flash, and came on with a noise like that of a great rush of wind, but as the direction of the storm was nearly coincident with the water line, inclining but slightly towards the beach, about ten minutes elapsed before the downfall reached the place where I stood.

From the information I afterwards obtained, the thunder-storm was subsequently, a few miles to the north-east of Southport, more severe than it was in Southport itself.

Southport, 11th March, 1878.

B. ST. J. B. JOULE.

Mr. BAXENDELL said that Mr. Joule's observation, confirmed as it was by Mr. Thistlethwaite, was very important, as the phenomenon he described was one of extremely rare occurrence; he had himself closely observed many thunder-storms, but had never seen a lightning flash similar to the one depicted by Mr. Joule.

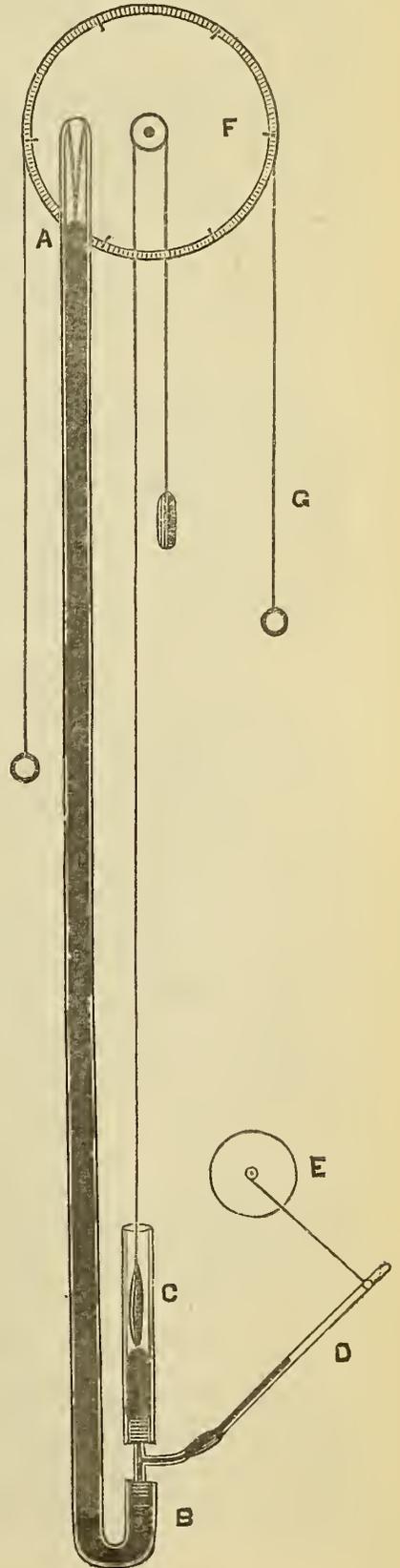
The PRESIDENT said he had seen a similar discharge at Saddleworth about 30 years ago.

"On a Barometer," by Dr. J. P. JOULE, F.R.S., &c.

Some years ago I brought under the notice of the Society a syphon barometer, the peculiarity of which consisted in the introduction of a small quantity of sulphuric acid. I hoped that the diminution of the capillary effect, and the extreme mobility of the mercury thus obtained, would present some advantages. I found however that the opinion expressed by Dr. R. A. Smith, viz., that the acid would act

on the mercury, was fully justified in the event; for the barometer, after a few weeks had elapsed, stood at too low a level, and now, after an interval of 5 years, stands 3 inches below its proper height, while a plentiful crop of transparent prismatic crystals has been formed.

In the barometer which I now venture to submit, I have applied a principle which I have found very useful in the construction of the manometer. Sealed within to the top of the long leg of a syphon barometer is a piece of thin glass rod, with the extremity of which at A the mercurial column can be brought into contact by means of the adjunct to the lower short leg. C is a tube of the same diameter as the rest of the barometer: it is connected with the short leg by means of a T tube, to which also the narrow glass tube D is attached by a piece of rubber tubing. By raising this small tube by turning the axle E, the mercury can be brought to touch the glass point at A. Affixed to the upper part of the framework of the instrument is a graduated wheel F having a groove in its periphery and also a small V groove near its centre. This last holds a fine wire with a glass plummet at one end and an exact counterpoise at the other. To bring this plummet in contact with the mercury of the lower leg, the wheel is moved by means of the thread G. The contacts of the mercurial column are observed with microscopes, and the value of the graduations of the wheel by comparisons with a standard rule.



In order to facilitate the application of the needful temperature corrections, it is desirable to secure the upper part of the barometer to the block of timber which supports the wheel, leaving the lower end free. There is also an advantage in introducing a small quantity of sulphuric acid into the small tube D, to promote the freedom of movement of the mercury in it.

The average time occupied by an observation is found to be $\frac{3}{4}$ of a minute, and the average error $\frac{1}{15 \cdot 60}$ of an inch. In the morning of yesterday the wind was high and gusty, causing the mercury to oscillate at short intervals of time through a space of about $\frac{1}{2 \cdot 60}$ of an inch.

Mr. C. H. STEARN exhibited and described a new form of portable Sprengel pump. In this arrangement all atmospheric pressure is removed from the mercury in the upper reservoir, and the lower receiver exhausted by a mechanical pump before the Sprengel is set in action. In consequence of the reduction of pressure on the reservoirs the length of fall tubes can be diminished to only a few inches; the time occupied in exhaustion is also greatly shortened as the preliminary exhaustion of the receiver removes at the same time most of the air from the vessels attached to the upper part of the pump. In the earlier form of the instrument exhibited by the author at Plymouth the fall tube was single; in the present modification this was divided into three branches, as in Mr. Giningham's pump, and far greater rapidity was obtained in consequence. Exhaustions of 0.0005 mm. have been obtained with these pumps, and the spark from a 5-inch induction coil has been completely arrested by a space of 3 mm. in the vacuum. A modification of M'Leod's gauge was also described, adapted for use with the portable Sprengel.

"A Comparison of the Standard Barometer of the Owens College Physical Laboratory with the working Barometer," by Mr. MORISABRO HIRAOKA, Student of Owens College. Communicated by Professor B. STEWART, LL.D., F.R.S.

The standard barometer tube was constructed in 1876, by Mr. Hicks, according to the plan of the late John Welsh. It has an internal diameter of about 33 millimeters. It stands on a solid block of stone, and is supported by a cast-iron framework, which was made at the workshop of Owens College.

The cathetometer, with which the height of the barometer is read, has a brass scale divided into millimeters, which reads to $\cdot 02$ mm. by a vernier. It stands on another block of stone, and its distance from the barometer is about 3.5 meters. It was made by Oertling, of London.

The working barometer is of the form usually known as a Portable Standard, constructed by Casella. It is furnished with a brass scale divided into both inches and millimeters, which reads to $\cdot 002$ inches and $\cdot 05$ mm. respectively by a vernier. It has been verified at the Kew Observatory, and reported to be correct to the third place of decimals in inches and to have a correction to metrical scale = $-.05$ mm.

Preliminary determinations.

I. Comparison of the cathetometer scale with the standard yard, placed at the same distance as the barometer.—The standard yard is made of brass, and it has been verified and found correct at the Kew Observatory. From the mean of seventeen readings it was found that 1000 divisions of the cathetometer scale is equal to 999.88 mm. (the probable error = $\pm \cdot 01$ mm.). In the reduction of inches into millimetres 1 apparent inch was assumed to be equal to 25.3916 apparent mm. when the two scales were at the same temperature.

II. Comparison of the dividing machine with the standard yard and the determination of the length of the barometer screwhead pointer by the former.—From the mean of ten readings its length was found to be = 111.340 mm. (the probable error = $\pm \cdot 001$ mm.).

III. Comparison of the English and metrical scales of the working barometer.—If both scales are correct at the respective standard temperatures, one apparent inch should

be equal to 25·3916 apparent mm. ($= \frac{25\cdot39954}{1+16\cdot67a}$ where $a=0\cdot0000187$) at any common temperature. From the mean of the following comparisons it was found that 0·052 has to be subtracted from the reading of the metrical scale to make it correct, which agrees well with the correction given in the Kew certificate, namely—0·05.

Inches.	MM. (Calculated) (1)	MM. (Observed mean) (2)	MM. Difference (2)—(1)
28	710·965	711·006	+ 0·041
28·5	723·661	723·720	+ 0·059
29	736·356	736·400	+ 0·044
29·5	749·052	749·110	+ 0·058
30	761·748	761·817	+ 0·069
30·5	774·444	774·500	+ 0·056
31	787·140	787·180	+ 0·040
			+ 0·052

The actual comparison of the two barometers.

The mode of procedure. This was done in the following order:—

A Take reading of the upper surface of mercury of the standard barometer.

B Take reading of the working barometer.

C Adjust the screw-head of the standard.

D Repeat *A*.

E Repeat *B*.

F Note temperatures.

G Read the height of the centre of cross of the screw-head (twice).

The temperature was observed by the thermometer affixed to the working barometer, another plunged in mercury of the standard, and the third hung in contact with the cathetometer scale. The last thermometer was carefully shielded from the direct radiation from the body of the observer.

Results (all corrections mad

	Standard B.	Working B.	Difference.
1.	775·21	775·35	+ 0·14
2.	775·39	775·44	+ 0·05
3.	775·28	775·17	- 0·11
4.	775·07	774·93	- 0·14
5.	775·54	775·82	+ 0·28
6.	774·63	774·80	+ 0·17
7.	774·25	774·24	- 0·01
8.	774·24	774·34	+ 0·10
9.	773·54	773·67	+ 0·13
10.	773·85	773·91	+ 0·06
11.	775·12	775·07	- 0·05
12.	775·06	775·11	+ 0·05
13.	775·24	775·18	- 0·06
14.	774·98	775·01	+ 0·03
15.	774·69	774·78	+ 0·09
16.	774·56	774·38	- 0·18
17.	774·39	774·49	+ 0·10
18.	774·52	774·52	0·00
19.	759·87	759·86	- 0·01
20.	759·90	760·03	+ 0·13
21.	759·99	759·95	- 0·04
22.	760·37	760·22	- 0·15
	<hr/>	<hr/>	<hr/>
(Mean)	772·077	772·103	+ 0·026 mm.
In inches	30·3972	30·3982	+ 0·0010 in.

If to the above results, a small correction due to the difference in height of the cisterns of the two barometers (the cistern of the working barometer being about 40 mm. lower than that of the standard), the difference becomes +0·022 mm. or less than +0·0009 inches.

The object of the experiments described in the present paper is not so much to find out the absolute difference of the two instruments employed, as to show the degree of accuracy to be attained by an ordinary amount of care in this mode of comparing standard barometers at different places. Although every precaution was taken to ensure accuracy as much as possible, still there were many other

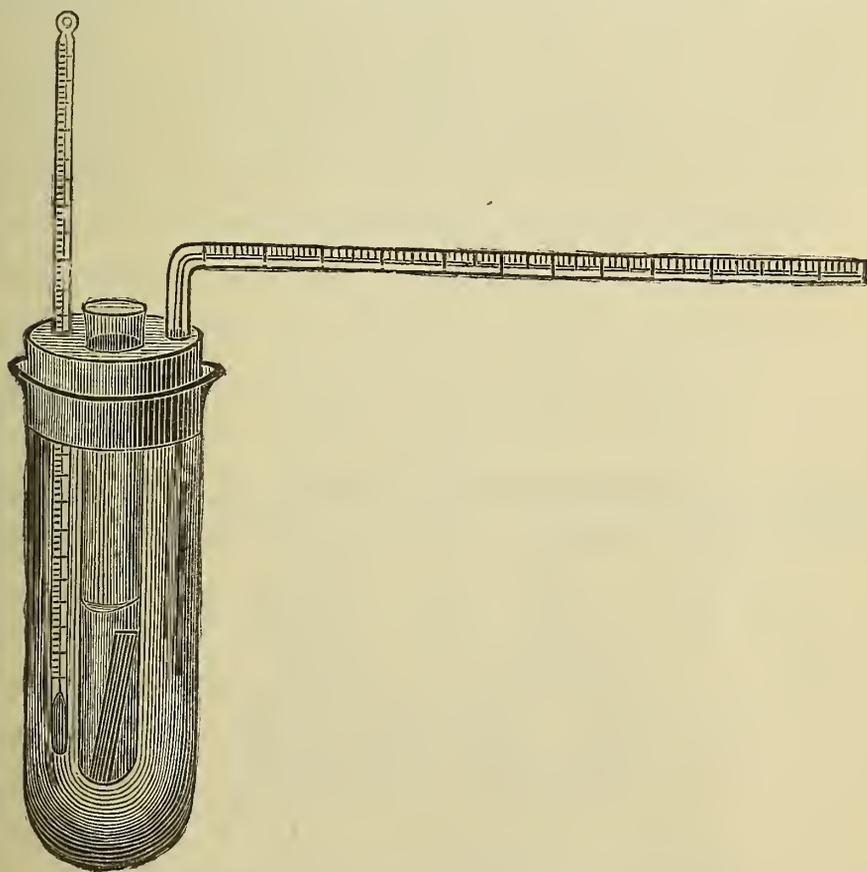
sources of error, especially probable want of uniformity of temperature in different parts of the instruments, and also too great distance (about 12ft.) between the standard barometer and the cathetometer, which was unavoidable in the present arrangement of the Laboratory. So that, as far as these observations go, the standard barometer of the Laboratory and that of the Kew Observatory may be considered to agree, within a small experimental error.

“On a New Calorimeter,” by J. B. HANNAY, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

Having been desirous some time since of making a number of calorimetric determinations, I set about constructing an instrument for the work, but I found that working in the rapid, irregular manner in which I was compelled to do by using only odd leisure moments, the difficulty of obtaining a perfectly constant temperature was very great; and as I have in a great measure overcome that difficulty, I think that an account of the instruments which I have constructed may be of some use to workers who may be placed in such a position as mine, and may desire to make measurements quickly, and without waiting until the temperature is quite constant. In the first place it is evident that as the external or accidental variation of temperature of the instrument is generally not more than 0·2 to 0·3 of a degree; if the internal variation be made greater in one case than another, the error arising from accidental variation will be smaller. Now if we use a liquid body to expand as an indicator we must, if we want a large rise of temperature, use a small quantity of the liquid or a large quantity of the body experimented with; either of these conditions, however, involves difficulties of a kind nearly as bad as the temperature variations; but if we use a gas as our index, that is to say, if we use an air vessel into the middle of which we plunge the substance whose specific

heat we wish to determine, and have the vessel surrounded by the usual non-conducting material, we can have a large rise of temperature in a considerable space of matter. Of course at the present time, when the specific heats of substances such as silver, copper, and iron, have been very accurately determined, we do not need to have a water or absolute calorimeter, but only an instrument which will accurately register the rise of temperature and the value of whose graduations may be determined by standard metals; so that although we do not know the absolute calorimetric value of the heat communicated to the air and glass of such an instrument, yet if its indications are proportional to the amount of heat communicated, we have all that is required to form a relative calorimeter. Then as to the construction of such an instrument. It was found that the use of ground joints in the instrument might be entirely avoided, as a red india-rubber stopper can be adjusted with nearly as much accuracy as a ground joint.

The apparatus shown below as Fig. I. is the first form of



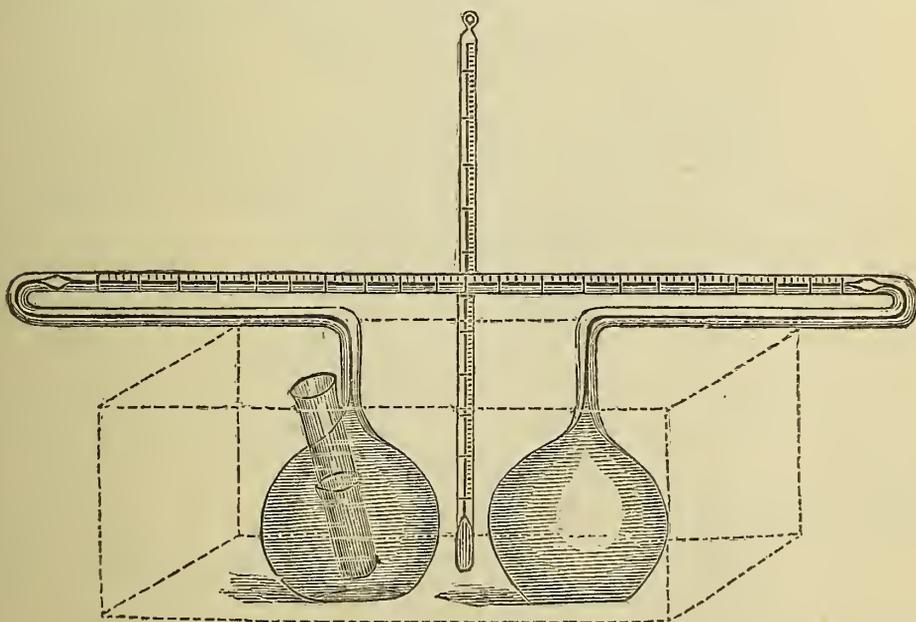
THE NEW CALORIMETER.—FIG. I.

the calorimeter I constructed. A ring was etched near the top of the wide outer tube, which was then filled with water, and the india-rubber stopper pressed in till it was level with the ring (the excess of water escaping by the index tube) and the whole weighed. This was repeated several times, and it was found that (provided the temperature were constant) the weights differed only by two or three centigrammes at the most. If, however, the stopper were permanently fixed in and the index tube and thermometer and interior tube pushed into their proper marks afterwards the weights differed only by a few milligrams. It was thus seen that the volume of gas in the apparatus could be adjusted with as great accuracy as by ground stoppers and with less chance of leakage.

The apparatus consists then of a wide outer tube having a horizontal ring etched round it (in case it may be necessary to replace the stopper) and fitted with an india-rubber stopper through which the thermometer, reception tube and index are inserted. The reception tube may also have a ring etched round it, and be filled with water or other liquid to that mark, in order to cause any non-conducting body to give up its heat quickly to the glass, but I generally prefer to use the apparatus dry and stop the end of the reception tube with cork in order to prevent convection currents. When this latter method is used the apparatus is always ready for use, and requires no other preparation than placing the index in position, which is managed as follows:—With regard to the substance of the index I have found mercury not sufficiently mobile, so I have used instead a coloured aqueous solution preferably containing a little glycerine. The calorimeter is firmly clamped and enclosed in wool, and a small drop of coloured liquid is made to touch the end of the index tube, when it is at once drawn in. The number on the scale of the thermometer level with the stopper is read off, and the thermometer, which has been slightly greased, is pulled up (screwing it from side to side to make

it rise easily) till the coloured liquid is drawn up to zero on the scale, then the number of the thermometer stem level with the stopper is again read off. We have then the volume of the scale tube in terms of degrees on the thermometer, and so whenever a calorimetric determination is about to be made the thermometer is pushed down that amount beyond its standard level, and just before a determination is made the index is introduced and sucked in (by means of the thermometer) to zero. It will thus be seen that the calorimeter is open to the air and in a state of equilibrium until the experiment is about to be made, and the index being adjusted by this simple contrivance involves no special parts for the purpose. As this calorimeter can be made and used by any student, I have found it extremely useful in inculcating the principles of thermal chemistry.

It naturally occurred to me that by connecting the end of the index tube with another air vessel which would be kept in the same conditions as the calorimeter vessel, we would have an instrument in which the external or accidental variations affecting both sides of the system would in no way affect the equilibrium, and the index would only be moved by any difference affecting only one vessel, or in other words we would have a differential calorimeter. The apparatus drawn as Fig. II. shows the construction of such an



THE DIFFERENTIAL CALORIMETER,—FIG. II.

instrument. Two glass bulbs—one holding an interior tube for the reception of the substances, and the other a little smaller in diameter in order that its cubic contents may be about the same—are fitted with an index tube of small bore, as shown in the figure, which is graduated between the two small cavities on the upper portion of the tube—the cavities being there to prevent the index being accidentally drawn into either of the bulbs. The two bulbs are placed in a box (indicated by dotted lines) surrounded by non-conducting material, and the instrument is ready at any time for use. A thermometer is placed between the bulbs in order that the temperature may be noted at which the experiment was tried. In the instrument No. I. the indications are nearly proportional to the specific heat of the substance, but in the second or differential calorimeter a curve should be drawn of the values for known metals just as is done for the spectroscopic scale, and the specific heats of the bodies reduced by reference to the curve. To show the accuracy of the instruments I give here some specific heats taken at random from many I have done.

First, by No. 1 Calorimeter :—

Substance.	Found	Correct Numbers.
Arsenic	·0820	·0814
Tin	·0575	·05623
Platinum	·0339	·0335
Bismuth	·0315	·03084

I should say that I took as my standards to draw the reference curve the following metals :—

Lead	·03065
Silver	·05701
Zinc	·09555
Iron	·11380
Magnesium	·2499

The Differential Calorimeter gave :—

Substance.	Found	Correct Numbers.
Arsenic	·0818	·0814
Tin	·0565	·05623
Platinum	·0338	·0335
Bismuth	·0309	·03084

In conclusion, I would say that the form of air calorimeter I have given as No. 1 is very well adapted for teaching, as it enables students to construct for themselves instruments of sufficient exactitude to yield numbers very closely agreeing with standard numbers, while for calorimetric investigations, where freedom from all error is the first requisite, the differential calorimeter is very well adapted. The latter may also be made with india-rubber joints, but, as there is pressure, I prefer to have the joints hermetically sealed. In determining the heat of combination by these instruments the same relative method is used as for specific heats. It will be obvious that one great advantage in these instruments is the very small amount of substance required to give the necessary deflection. I have found in practise that from 0.5 to 2 grammes is quite sufficient, the amount, of course, varying with the size of the instrument.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 11th, 1878.

CHARLES BAILEY, Esq., in the Chair.

“On Bryozoa,” by ARTHUR WM. WATERS, F.G.S.

When we meet with the term zoophytes it does not now give an idea of scientific accuracy, but we do not often think how literal a signification it had when first employed. The zoophytes have included organisms in many widely separated groups, as the Anthozoa, the Hydrozoa, Bryozoa, Sponges, and even the calcareous Algæ, and these were at one time held by some of the school of Pallas to be, as the

word implies, plant-animals, and this in a very real and extraordinary manner, for they maintained that the branching stalks were of a vegetable nature, containing organisms of an animal nature. The theory of Pallas was supported by Linnè and opposed by Ellis, and in the discussions on this question the Bryozoa were used as much as any of the other zoophytes to show the vegetable nature of what we now know to be an integral part of this animal, which, together with other zoophytes previously to Ferato, had been thought to be merely plants.*

Since then the Actinozoa have been separated, including animals, having one simple body cavity and divided by radiating calcareous or non-calcareous divisions; and the Bryozoa were shown by Milne Edwards, 1828; Thompson, 1830; Ehrenberg, 1832, to vary considerably from the Hydrozoa in having a curved digestive system with two orifices: the first called them tunicated polyps, the second Polyzoa, and the third Bryozoa (moss animals). Since then additions have been made to our knowledge of these animals, and there have been many battles fought over them, and as there are many points unsettled, we may expect "wars and rumours of wars" for a long time.

Many of the Hydrozoa in the external forms of their seaweed-like growth closely resemble the Bryozoa, but differ in having only a single cavity, and, unlike the Anthozoa, have no radial divisions.

The calcareous Algæ are now under the care of the botanists, and do not require any further consideration from us.

The economy of the animals now under consideration is in many respects as strange as anything in the animal kingdom, and we shall see that there are many points which show an external resemblance to plants. As men-

* For the history of the Bryozoa see Johnstone's *Brit. Zoophytes*; Dr. Allman, *Freshwater Polyzoa*; and Nitsche, *Beiträge zur Kenntniss der Bryozoen*.

tioned, the general form is often very closely that of the seaweed, and the polypide, which was called the flower, is not formed until the zoecium* is moderately developed; then after the polypide has enjoyed life for a time it disappears by decay and resorption, and in time another polypide buds out from the inside of the same cell, and thus it was held that the "flower" was deciduous. Again, a very large number throw out radical tubes and fibers. Some have a large number at the base, as in *Cellaria ceroides*, others from all parts of the stalk, and from specimens of *Diachoris* I find there is one root from the back of each zoecium (cell). That these points were used to maintain their animal plant nature, when but little was known of their anatomy, cannot be wondered at, and we may see why Pallas† says "Zoophyta esse animalia vere vegetantia, in plantæ formam excrescentia; plantarumque alias quoque proprietates affectantia; esse plantas quasi animatas."

The Bryozoa are divided into two orders: (1) the *Phylactolæmata* which are fresh water. The tentacular disk is horse-shoe shaped and the mouth has an epistome or cover which closes it. (2) the *Gymnolæmata* have the tentacular disk circular. These are marine, and as my studies have been confined to these I shall only consider them to-night.

The first suborder, the *Cheilostomata* represented by *Flustra*, *Leprælia*, *Eschara*, &c., is the largest and most important. They have an operculum to the aperture of the zoecium (cell) while the *Cyclostomata* have none. These last are all calcareous, and the form of the cell is more tubular than in the *Cheilostomata*, and on this character was based d'Orbigny's classification. The best known form is *Crisia*. The *Cyclostomata* have had much less attention than the other divisions.

* The zoecium is the cell in which is the polypide, consisting of the alimentary canal and the tentacles. As the word cell is used for a single cell of cellular tissue and for a chamber (as the bee's cell) it is necessary to use the term zoecium for the sake of clearness.

† *Elenchus Zoophytorum*, p. 19.

The next suborder is the Ctenostomata which is distinguished by the cell orifice being surrounded by a fringe of bristles. These are horny or fleshy, and none are calcareous, and have a soft stalk with zooecia springing out from the side.

The Endoprocta (Pedicellina, &c.) consist of some half dozen species on the border land, and need not be further discussed in remarks of so general a character.

The form of the Bryozoa is very various, but all are fixed, and this, as we have seen, was so much associated with vegetable life that it was one of the reasons which led to the strange conceptions alluded to.

The marine forms live at various depths, occurring probably in greatest abundance in from 20—100 fathoms, but there are also many species living just below low water; and here, as with other marine animals, we can, from the species, judge of the depth. In depths where the currents are slight and which we may consider as below the Laminarian the firm forms as *Eschara*, *Idmonea*, *Retepora*, &c., abound, whereas in the zone, where they would be broken by the moving waters none, except incrusting forms, are rigid for any considerable extent, but some have chitinous joints at shorter or longer intervals as in *Crisia*, *Cellaria*, *Salicornaria*, &c., some have but very little calcareous matter so that they are flexible like most *Flustra* and the Ctenostomata, and even in *Flustra membranacea* the thin calcareous portion has been shown by Nitsche to be in discontinuous plates, thus giving a large degree of flexibility to this semi-calcareous species. Although the rigid forms are not found in the lesser depths many of those of great flexibility as species of *Crisia* *Salicornaria*, &c., &c., are found in the deeper waters. The Challenger found Bryozoa at all depths, and doubtless this expedition will add very much to our knowledge of their range about which but little is yet known.

In many the branches anastomose, thus forming a reticu-

lated structure as in *Retepora*, *Retihornera*, and *Fascicularia*; and the *Fenestellidae*, which were so numerous in the Paleozoic seas, grew also in a reticulated form, and from this they have been considered to be allied to *Retepora*, but they entirely differ in every other respect from *Retepora*, and as I have just had the opportunity of observing in *Actinosoma fenestratum*, Young, the so-called denticles are so thin and reach so nearly to the centre as to appear like distinct surface septae and seem to indicate that the *Fenestellidae* should not be placed with the *Bryozoa*.

We said that the *Cheilostomata* were sometimes calcareous and sometimes horny, and as the *Flustra* (sea-wrack) and *Eschara* are the best known to casual observers we may describe the polypides in these and say that they only vary from others in minor details. The polypide is provided with tentacles (varying from 8 to 30) which are covered with cilia, and from this the *Bryozoa* were called by Farre* the *ciliobrachiata*. The polypide, including the tentacles, when withdrawn, lies in a transparent sheath, which is held in its place by sets of muscles from the cell wall, and besides these there are the large retractors attached near the base of the polypide; the opercular muscles, which in some species, as *Myriozone truncatum*, attain great dimensions; and in the non-rigid species the parietal muscles.

The muscles, instead of occurring in solid bundles as they do in the higher animals, are in separate threads. Although not entering into any details concerning the muscular system, it is not because their position is not of considerable importance when considering the zoological affinities of the various *Bryozoa* and other lower forms of life; and in advocating their relationship with the worms, Schneider† has pointed out that in the worms their specific and generic place can be determined by means of their muscular system, in the same way as that of the vertebrates by their bones.

* "Observations on the minute structure of some of the higher forms of *Polypi*." *Phil. Trans.*, 1837, pt. 2.

† Schneider. "Archiv. für Micro. Anat.," tom V., 1869.

The reproduction and increase takes place by means of budding and of eggs. The egg develops into a ciliated larva, which undergoes a change that has been compared by Joliet* to that taking place in the developed polypide, so that the young polypide buds in the larva, which thus represents the zooecium. This zooecium now divides into two in somewhat different ways in various species, and the growth of the colony proceeds by division of the growing zooecia. We may, however, examine a little further as representative, the growth of a *Flustra* or *Eschara*, as worked out by Nitsche in *Flustra membranacea*. The cells or zooecia become long, almost tubular, and after the polypide has begun to bud in the lower part a division takes place across from the lateral walls, thus dividing the lower part off into a rectangular cell the usual shape and size. In this way the *Flustra* increases in length, and the increase in breadth is brought about by the division of these terminal zooecia in the direction of their longer axes.

The budding of the polypide inside the cell has been followed out by several observers, as Nitsche, Claparède, Smitt, &c.; and a reference to their figures will explain more than pages of description. The bud, which at first is only a cellular protuberance, soon shows divisions which represent the tentacles, and ultimately these become separated tentacles, the canal of the polypide becomes fully developed, the oral aperture is now formed in the zooecium, and the now completed polypide can extrude itself through this opening, and the cilia on the tentacles create currents which bring a supply of food to the mouth of the polypide. After a time, it shows symptoms of loss of vitality, and then in a very short time the polypide has passed through a gradual diminution until it has disappeared, leaving only a small, dark body, enclosed in a thin membrane, and the zooecium is now empty, excepting

* "Bryozaires des cotes de France," par M. Lucien Joliet.

Lacaze-Duthiers, *Archives de Zoologie Experimentale*, 1877, pt. 2.

the parietal muscle, the funiculus, and the "dark body." Joliet says, in his recent paper, that he has found this process of decay and absorption to take place in one case in ten hours. As we have before said the zooecium does not remain empty but another polypide may bud inside the same zooecium and frequently traces may be seen showing that there have been two, three, or even occasionally more.

The "dark body" just mentioned is the remains of the polypide, and as recent observations seem clearly to prove has no vitality, though it has been maintained by Smitt and Hincks that the new polypide sometimes buds from this; and by early observers this was thought to be an ovum. When the polypide should happen to bud near this "dark body" it may become enclosed in the new bud, and in this case will, as watched by Joliet, be passed out through the intestine with the first undigested food.

The funiculus mentioned also requires further explanation. The funiculus is well known as a flexible cord at the base of the polypide in the fresh water Bryozoa, but the homologous organ in the Cheilostomata has much larger extension in the zooecium though not as prominent. The polypide is here also attached at the base by the funiculus, and it is from this that the budding of the polypide takes place, and in the Cheilostomata the funiculus of the one cell has a connection with those of the neighbouring cells. It is thus quite clear that what is called the funiculus is of primary importance. This term has been used by many recent writers on the Bryozoa and *Nitsche calls the lateral funiculus the *seitenstränge*, or "funiculi laterales," which he says does not prejudice the physiological signification, and in the same sense the term is used in this communication.

This filament is composed of fusiform cells, and this is covered with the corpuscles of the chylaqueous fluid. From the principal filaments branches are given off. This is what

* "Beiträge zur Kenntniss der Bryozoen." *Zeitsch für wissensch. Zool.*, 1871.

was called by †Reid the contractile substance; by ‡Müller colonialnervensystem; by §Reichert the colonialbewegungsorgan; and within the last few months ||Joliet has suggested the term *coenosare*; but I think he may have overlooked the fact that Hincks and others have already used this term in a somewhat different sense in the Hydrozoa, though as the term *funiculus* is so far from satisfactory now that more is known of the funicular system, it is quite possible that if this stands criticism, and Joliet is satisfied to retain it, that it may be used instead of the older term which has lost much of its original significance.

Reid mentions change of this contractile substance in the avicularia; Reichert found a slow change take place in his colonialbewegungsorgan which he did not perhaps interpret quite correctly, and Joliet has shown that the finer anastomosing threads of the funiculus or coenosarc often change their positions altogether in the course of one or two hours. This is of course a very strong argument against this being a nervous system, for we see that these threads occur in various positions in the different cells of the colony, and in each the arrangement varies from time to time, which has no parallel in any nervous tissue.

Joliet believes the greater extension of the funiculus in the marine Bryozoa is represented by the middle layer of the endocyst of the fresh water, but as I am not practically acquainted with this group I cannot express any opinion.

In the fresh-water Bryozoa a nervous ganglion has long been known, but with the exception of *Pedicellina* none is known with certainty in the Gymnolæmeta, though in a few instances it has been thought that one could be distinguished.

† "Anatomical and Physiological observations on some Zoophytes," by Jno. Reid, M.D., *Ann. Nat. Hist.*, Vol. XVI., 1845.

‡ "Das Kolonialnervensystem der Moosthiere." *Wiegmann's Archiv*, XXVI., 1860.

§ *Abhandl. der König Akad. der Wissensch.* Berlin, 1869.

|| *Loc cit.*

As the only part which geologists are likely to find, except in special circumstances, is the calcareous shell, this has for me the most interest, but as I have recently entered into the points which I more fully examined, it will not be necessary to do more than briefly review a few points concerning the shell.

Through the shell of most there are tubes passing from the interior to the exterior, and these tubes are filled with the chylaqueous fluid of the animal, and upon these perforations or pores depends for the most part the sculpturing of the shell. There is an external growth which is most apparent in *Eschara*, *Myriozoon*, &c., and this takes place independently of the life of the polypide, as is shown by a considerable external growth over the opercula.

The stalk of the *Ctenostomata* is divided by diaphragms in which there are minute perforations through which the funiculus above and below is connected, and in the same way each polypide is joined with the main funiculus. In the walls of the *Cheilostomata* there are similar thin portions, which have been called by Reichert "rosettenplatte," and the object of my recent communication, to which I have referred, was to show that the position and form of these rosettenplatte should be given as a character, since they differ very much in various species. The points to be noticed are their number, position, and if they are separated by dividing ridges. Part of the characters it will be seen can be made out in fossil species, and therefore it is most important that it should be mentioned, as thus the position of an internal organisation is recorded in the shell.

Although this is an important point that may be learnt by an examination of thin sections of the shell, it is by no means all, for sections will show the shape of the cell, mode of growth, varying structure of the shell, from which in some cases even the general sculpturing of the exterior would be revealed. Nor is it probably with the more recent

formations only that sections will aid in our knowledge of the soft portions. I have as yet had little opportunity of examining Bryozoa older than the chalk, but in some Scotch Carboniferous shale I had given me there are some so well preserved that much of the structure of the shell can probably be worked out besides the shape of the cell; and the pyritic crystals between the cell walls show where there was organic tissue so many million years ago.

I cannot help thinking that if more attention was given to sections of the small fossils, then we should be able to fix the age of most limestones from *minute* fragments, and should be able to know with much more exactness what animals lived in the seas of past ages; but it is necessary that recent forms should first be examined to give a basis of comparison. Enough has been said about these animals to enable the next point to be now understood.

It may be a matter of surprise that I should speak of the Bryozoa while the two first authorities, Dr. Busk and Dr. Allman, employ the name Polyzoa for this class, and I therefore think it necessary to state my reasons, and leave each one to consider whether they are sufficient or not.

My principal reason is, that after a struggle of nearly half a century, the Polyzoists are in a great minority, for in all the countries of Europe, with the exception of England, the term Bryozoa is used, and has been adopted by d'Orbigny, Hagenow, Bronn, Van Beneden, Krohn, Haime, Leuchart, Reichert, Reuss, Nitsche, Kirchenpauer, Smitt, Roemer, Claperède, Manzoni, Joliet, and many other zoologists who have not made them a speciality. Sars, in a short paper, uses Polyzoa, and Leidy and Hyatt, in America, have followed their English leader, while Dana is a Bryozoist.

As uniformity of nomenclature is in science of such great importance, it is much to be wished that one term should be used by all. However, if there were not other reasons, it would seem right for science to be encumbered by the

British name as well as the more general one. Yet those who have called these animals Bryozoa have not done so without a reason nor without fully recognising the fact that Thompson has priority over Ehrenberg. Bronn, as far as I am aware, had not devoted special attention to this class, but as he was so justly ranked, among a generation now passing away, as the greatest authority on classification, his opinion carries much weight, though he may have received assistance in the details. He points out that the term Polyzoa, used in the separation of animals from others which this name would as correctly describe, is very unphilosophical and could not be accepted. The term Bryozoa may be thought open to the same objection, though as here only an external appearance is taken and nothing approaching a zoological character, the objection does not apply in the same way.

But the great objection of those who have refused to adopt Polyzoa is the entirely different meaning* Thompson attached to the word to that which it now conveys. The heading of Thompson's paper is most important, and runs, "On Polyzoa, *a new animal* discovered as an inhabitant of some zoophytes," and in the text he says, speaking of Flustra, "in many of which I have clearly ascertained the *animals* to be *Polyzoæ*" (italics mine). Dr. Busk says that etymologically the name as now used is not the same as when originally given. I am unable to quite agree with Dr. Busk in this way of putting it, for the difference seems to arise from an entirely distinct zoological conception, and "the inhabitant of the zoophyte" is the polypide in the zooecium, or cell, and the Polyzoæ are the polypides, so that by the term Polyzoa nothing more was then meant than a single polypide. The intimate connection of polypide and zooecium seems to be ignored as much as if we were being taken back to the time of Pallas. Our know-

* "Zoological Researches," Mem. 5.

ledge of an intercolonial connection shows how unfortunate was the conception of Thompson. If it was not for conflicting claims the due meed of praise could be given to Dr. Thompson for such good investigation in this and several other branches of zoology, without pointing out that the general idea is not in accordance with the present knowledge of the subject.

As far as I am acquainted with the subject, it appears that all the fossil Bryozoa, with the exception of the tertiaries, have been too superficially described for any palæontological generalisations to have any value, for until much careful examination has been made, we shall feel uncertain as to which are Cheilostomata and which Cyclostomata, and the fact of Fenestella, which occur in such great numbers in the Paleozoic rocks, possibly not belonging to the Bryozoa, shows how much there is still to be done in micro-palæontology.

One point which adds much to the interest of the Bryozoa is their wide relationships. As mentioned, they have long been removed from the Anthozoa. Milne-Edwards, Dr. Allman, and others have shown that there is an affinity with that interesting class, the Ascidians, the great interest of which consists in their connection of widely separated classes. Then with the Brachiopoda the connection is undoubted, and Schneider has shown that the embryonic development most nearly resembles that of the Gephyrea, an order of worms. Schneider also proved that Cyphonautes was the larva of Bryozoa, so that here, as in many other cases, the young form was previously thought to be an animal with an altogether different zoological position. As embryology is such an important factor in assisting classification, the Bryozoa are now considered to be most nearly related to the worms through the Gephyrea, and this has also removed the Brachiopoda towards the worms, so that instead of being most intimately related to

the Gasteropoda and Lamellibranchiata they are less closely connected.

The question of individuality it will be seen is, in such a class, one of great difficulty, and if we formulate it that may give us some satisfaction, but it may, after all, be a question which we do not understand any better in consequence. The recent investigations, which show that a much greater importance must be attached to the funiculus than was supposed, somewhat alter the basis upon which this question has been previously considered.

It has been seen that the difference of the growth of the polypide and zoecium was felt to be so great as to place the zoophytes in a kingdom intermediate between the animal and vegetable kingdoms, and the general conception, as far as it concerns this class in showing the independent growth of the polypide and the cell containing it, was correct as far as they do not necessarily grow simultaneously.

Lamarck* thought the polypides were quite distinct and had no more connection with one another, nor any more with the cell than the wasp has with its cell. This is, perhaps, the most simple theory of any concerning their individuality, but at the same time apparently the least correct.

Grant, and other observers afterwards up to the time of Dr. Allman, looked upon an individual as made up of zoecium and polypide. Dr. Allman, however, considered that besides the zoecium and polypide, the ovarium and testes each represented a zooid and the embryo also represented one. This, in a more or less modified form, has been generally accepted, but the recent researches in the marine Bryozoa seem to require, at any rate, that the ovaria and testes should not be looked upon as separate from the polypide. Schweigger,* on the other hand, thought the Bryozoa colony represented an individuum with a number of mouths

* See Nitsche, loc. cit., p. 59.

and stomachs; and Claparède has since then maintained that the polypides should be considered as organs of the Bryozoa.

In whatever way we may express ourselves the nature of my recent investigations has impressed upon me the fact of a certain connection of the polypides, and we have seen that the polypide is not necessarily a descendant of the zooecium, as it may grow from the funiculus. I further think that the zooecium may grow from the same organ. In *Cellaria* one or more chitinous tubes are thrown out from the oral aperture, and through this the funiculus passes, and I believe that it is from this, and not from the chitinous tube, that the zooecium and then the polypide is formed. This I can only say with great reserve until I have the opportunity of watching the growth of this or a similar genus; or I may perhaps better say that I point this out as a line of investigation for anyone at the sea-side.

The early stages of such organs as the avicularia radical fibres, ovicells are the same as the early stage of the zooecium, so that the "cystide" (Nitsche) of each is, according to all theories which recognise the zooecium as an individuum, to be looked upon as of the same value, each being also an individuum.

Perhaps in every scientific subject the man who has entered into it feels how much there still remains to be done, and certainly the present could be no exception, as there are in this field of research many parts of virgin soil requiring the co-operation of many workers.

MR. PLANT, F.G.S., exhibited specimens of auriferous quartz from Australia and North Wales, showing on their surfaces certain changes and metallic growths, which had taken place within the last ten years. He also referred to a paper recently read by Mr. T. A. Readwin, F.G.S., at the Mineralogical Society "On Mineral Growth at ordinary temperatures and under ordinary circumstances."

The object of its author was mainly to prove that with specimens in cabinets and in cases he had observed a spontaneous metallic-growth chiefly upon the surfaces of rock specimens from mines and upon ores of some of the metals. The paper described in details many such, from the author's cabinet, and from the museums in England and on the continent, chiefly growths of gold, silver, electrum, or argentiferous gold, copper and titanium.

The specimens exhibited by Mr. Plant had been identified by Mr. Readwin as gold-growths in thin sheets, and as globules of gold, and Mr. Plant was able to authenticate the development or growth of the gold upon the specimens since 1868. He had specimens of copper also recently developed as stalactites or moss copper. Changes in pyrites and sulphides generally were a well-known nuisance in all collections, but these metallic-growths were a new thing to him, and it is yet an interesting point to learn their cause, and watch whether the end be amorphous bundles or true crystals.

The paper, of which the following is an abstract, was read at a meeting of the Section, held January 14th, 1878:—

“The Mollusca of Cymmeran Bay,” Part IV., by JOHN PLANT, F.G.S.

For four years a systematic search has been made in this locality for the local mollusca, and already the Section has had several communications upon the species, lists of which have appeared. The additions in 1877 were very few, as the summer was unfavourable for much dredging outside the bay, but the total of species found on shore or dredged is now two hundred.

The bay is five miles wide from Rhoscolyn Beacon to Ynys Mabion, with a broken shore line of above 9 miles, part bold cliff, part sands, and some reef—the bay is full of reef rocks running NNE by SSW., with narrow vallies of sand

between—averaging 9 fathoms deep at high water—the reef ends about two miles from the shore, and abruptly, as the water outside deepens from 14 to 20 fathoms. This reef is a barrier cliff which causes a great roll or swell in the water when the tide is coming on strongly. No dredging is possible within the bay because of the reef, and all the shells dredged are got outside on shingly, sandy or rough bottoms in from 15 to 30 fathoms. The oyster beds lie out here. No loose valves of shells would ever get into the bay by bottom drifting from distant places, as the submerged cliff of the reef would stop them outside. The bay is too cold for luxurious growth of sea weed, although after storms the shore is strewn with stalks and leaves of the larger algæ. Yet the rocks at low tide are not covered with much growth,—the sea and storms are too rough and the shore often changes its aspect from sand or shingle drifted here or there. The tide rises 24 feet at its highest on the coast. The fauna is more northern than southern in its character—the proportions out of 100 dredged shells are, Arctic and Boreal species 28, Celtic and Southern 14, leaving 58 species as common to the British Coasts.

The scarcity or abundance of some of the dredged species are interesting points to record. *Saxicava rugosa*, *Venus striatula*, *Cyprina*, *Phasianella*, *Trochus ziziphinus*, *Nassa* and *Nucula* are plentiful; *Astarte sulcata*, *Velutina*, *Lamel-laria*, *Lucina borealis*, *Leda caudata*, *Fusus antiquus*, *Trophon muricatus* and *clathratus*, *Trochus tumidus*, *T. Montagui*, *Venus casina*, *V. verrucosa*, *V. fasciata*, *V. ovata*, *Pileopsis*, *Otina*, and others were sparingly found. The new species are *Lima Loscombii*, *Circe minima*, from 18 fathoms, and several *Odostomia* and *Rissoa*. The young *Cyprœa* dredged were empty, and none of these dredged shells were so fresh or of so good a colour as others found on the shore.

Ordinary Meeting, April 2nd, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The PRESIDENT said that he had come into possession of three interesting letters from the late Sir W. Fairbairn, Bart., F.R.S., a former President of the Society, to the late Professor Hodgkinson, F.R.S., also a former President. As both parties have left this world there can be no harm in giving the letters to the public. My friend Professor Hodgkinson, as is well known to the older members of this Society, devoted his whole life to science for its own sake, and gave nearly fifty years' labour to the world without fee or reward. His formulæ for the construction of cast iron beams and pillars are known and used wherever that metal is employed; and this is the only reward he received, either at home or abroad, with the exception of a couple of medals and a marble bust in this hall. The letters speak for themselves, and are as follows:

Rotterdam, Aug. 6th, 1836.

My dear Sir,

I have no doubt you have already deemed me a lame assistant in preparing the report for the approaching meeting of the British Association, a mere sleeping partner: ready to share honours due to your exclusive labours. I have tried in vain to make something of the experiments you were kind enough to hand to me in London. I have examined them with great care, but unless I had all the facts and subsequent experiments before me I find it would be worse than useless to attempt, or rather spoil, a much more accurate and satisfactory description from Mr. Hodgkinson. The fact is, I must confine my enquiries to an examination of the metals experimented upon; investigate the mixtures and make such experiments during the winter as will lead to pretty correct results tending to show the

application of the different metals and their combinations for the purposes of the arts. I think this will prove an important enquiry, exceedingly useful in its application, and not without interest as an appendage to your more elaborate investigation on the strength of the metals.

I made several attempts at an introductory paragraph for the forthcoming report; but as it was with the experiments, so it was with the introduction. I was short of the correspondence, and had to abandon an almost hopeless case for want of material to go on with. If I can assist you on my return this day week, I shall have great pleasure in doing so, particularly as we shall have 8 or 10 days before the meeting.

I have given up all hopes of being able to attend at Bristol. I trust you will go and be present at the meeting, and I hope your sister's health will permit of your absence for that period of time. I am already a week behind time. I must apply myself to business immediately on my return, and abandon all thoughts of absence for some time to come. Anticipating the pleasure of seeing you in a few days,

I am, my dear Sir, in haste,

Ever faithfully yours,

W. FAIRBAIRN.

To Eaton Hodgkinson, Esq.,
Windsor Bridge,
Manchester.

London, April 24th, 1838.

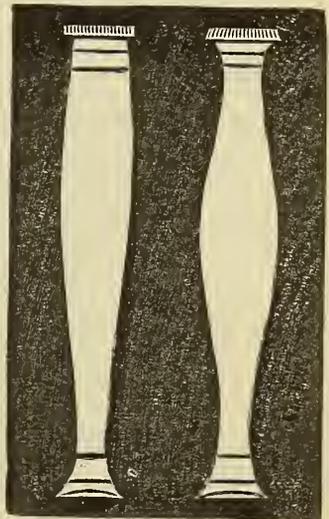
Eaton Hodgkinson, Esq.

My dear Sir,

I was so pestered for time and had so much to do before leaving Manchester that I left some matters on which I intended to consult you untouched. I am glad you are making such good progress with the pillar experiments. Now that you are fairly embarked, I am desirous you should see them to a conclusion upon terms that ensure

satisfaction. I think a great deal may be done and much light thrown upon the subject by a careful and well conducted class of experiments; in hollow tubes there is great scope, particularly as relates to varied diameters, thickness of metal, and the proportions of the upper to the lower diameters. Also an investigation into the proportions of the ancients, in order to ascertain whether the small rising from the lower base of the shaft is intended for beauty or strength: probably both qualities are included, and may be the result of experiment. In point of form you are aware that the symmetry varies in the different ages of the Egyptians, Greeks, and Romans, at any rate the swell observable in the best proportioned columns of the antique are valuable as far as it prevents deflection and must add to the strength of the shaft. I speak from memory as regards the proportions, but I think it would be useful as well as interesting to ascertain the strongest form, and probably the diameter near the middle will be the nearest approach. The antique is of this form:—but you can easily ascertain these particulars in any work on architecture.

Having completed the experiment on columns, the next business will be to prepare the papers for the Association. I have written a few pages introductory to the experiments on the transverse strains, but I think it will want remodelling, as well as considerable additions.



When I have the pleasure of your presence here in London it will be a good opportunity to get the papers printed. We could then read over the proofs and superintend the press at the same time. What think you of this? Will you write to Phillips and arrange it accordingly? We shall never have a more fitting opportunity.

Although it would afford me gratification to have my name associated with so able an experimenter as yourself, yet I cannot consent to have it attached to any document to which I am not a contributor. You are sensible I am no mathematician, and on that account cannot afford to lose a single fraction of the small offerings I have made at the shrine of science. It is different with my friend Hodgkinson, whose colossal dimensions might stand paring and is not so easily reduced. The fact is, I must be permitted to rest upon such humble merits as I possess, and not in a character which the world might justly call assumed. If you think my name can be respectably and justly introduced as a coadjutor in the more simple forms of transverse strain, I am satisfied: if otherwise, I will then take the experiments on temperature, continued strains, &c., and leave all the other to a mind of much greater profundity than my own.

London, Aug. 17th, 1843.

My dear Sir,

(After alluding to a domestic affliction.) Under these afflicting circumstances of course you will not expect me at Cork. I am however with you in mind, and would have been much gratified to have spent not only the days of the meeting with you, but to have accompanied you through some of the most interesting scenes in Ireland. This however cannot be accomplished, and all that I can wish you is an agreeable and satisfactory meeting with your scientific and learned friends.

I have written both Mr. Phillips and Mr. Taylor, requesting they will renew the committee with the grant of £150 for enquiring into the changes produced in metallic bodies by concussion. You will have the goodness to support the application; and having collected all the broken axles on the Leeds railway and the drawings of a considerable num-

ber on the South Western, I do hope with your assistance to make up a report by the time of the next meeting.

In requesting your assistance I must confess I have some little misgivings. I think, but I may be wrong, that I have observed some faint appearance of an ugly monster which they call —— which sometimes darkens the amiable and bright side of your character. You must however dispose of these impressions and deal honestly with me in all your thoughts. I can assure you you have nothing to fear from the quarter you seem to apprehend danger. I have neither the time nor the talent to trench upon your investigations as a mathematician and an experimentalist. I rejoice in your success in anything. I am never jealous of your power, and why deny to your friend and supporter a small share in the merits of a reputation which on many occasions he has endeavoured to extend and by every means in his power to perpetuate? You must never look upon me as your competitor, but as your friend and fellow laborer in a totally different department of science. The time is going, and begging you will receive the assurance of my attached friendship and regard,

I am yours affectionately,

W. FAIRBAIRN.

Eaton Hodgkinson, Esq.,
British Association,
Cork.

P.S.—I have no time to reexamine what I have written, and must give it you with all its imperfections.

RIDER.

I feel grateful for the close and continued attention you pay to the lads. In my hurried and anxious life I derive much consolation in the thought that my sons have so able and so willing a preceptor at hand; I can assure you I am not insensible to the advantages they must attain from your

unwearied attentions. I live in hope to see them do credit to their instructor; and on some future occasion to prove themselves no despicable mathematicians. At present you have no sons of your own, and until the time arrives when some sturdy brat shall scream Papa I must view you in the light I have always done, as a second father to my children. I sat down to write you a few lines about the pillars, it has turned out a long letter, which altho' of some slight interest to myself is nevertheless unworthy of postage. I must therefore look out for a frank, and remain

Ever faithfully yours,

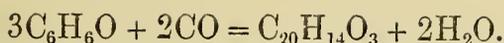
W. FAIRBAIRN.

I have a scheme in embryo, which I think I shall ruminate on, namely, an account or disquisition upon our iron ship building, accompanied with a few readable experiments for the Association. I will want you to put a correcting hand to the document in order to prevent it turning out the mountain in labour.

“On Aurin,” by R. S. DALE, B.A., and C. SCHORLEMMER, F.R.S.

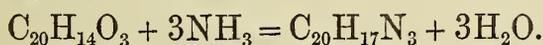
In a paper read before this Society, on 31st October, 1871, we gave a short account of this colouring matter, which was discovered by Kolbe and Schmitt in 1861. We showed that the commercial product, which is called aurin or corallin, and is obtained by heating of phenol with sulphuric acid and oxalic acid, is a mixture of different bodies from which we isolated a colouring matter which crystallised exceedingly well and for which we retained the name of Aurin.* The analysis of this body gave numbers agreeing with the formula $C_{20}H_{14}O_3$, from which we concluded that it was produced by the action of nascent carbonic oxide on phenol:

* A full account of this investigation is found in Journ. Chem. Soc. (21, XI., 434.)



We further showed that aurin, when treated in an alkaline or acid solution with zinc-dust, combines, like most other organic colouring matters, with hydrogen, yielding leucaurin $\text{C}_{20}\text{H}_{16}\text{O}_3$, which readily crystallises in large colourless prisms, whilst on heating aurin with ammonia under pressure a new red colouring matter is formed, which is called red aurin or peonin.

The further investigation of this body showed that it contained nitrogen, which on heating with soda-lime was evolved in some cases completely as ammonia, while in others, where the action of the ammonia went on longer it was given off, at least partly, in the form of aniline. We then found, that on continuing the heating, the red colour of the liquid became paler, and at last a yellowish solution was obtained, from which water precipitated a white crystalline body, possessing all the characteristic properties of rosaniline.* The formation of this compound might be explained by the following equation :



But Hofmann, who has most carefully examined rosaniline as well as a great number of its derivatives, found that the formula of this base is $\text{C}_{20}\text{H}_{19}\text{O}_3$, and this result has been confirmed by Caro and Gräbe,† who showed that rosolic acid, which is obtained from rosaniline by a reaction, which shortly may be stated to be the inverse of that above given, has the composition $\text{C}_{20}\text{H}_{16}\text{O}_3$, while its properties are in almost all respects so similar to aurin, that some chemists, including ourselves, have been inclined to regard them as identical.

In order to clear up these points it was necessary to prepare a considerable quantity of pure aurin. We have already shown in our memoir read before the Chemical Society,‡ that a pure compound is readily obtained by

* Journ. Chem. Soc. (2)XI, 434. † Liebig's Ann. CLXXIX, 148.

‡ Journ. Chem. Soc., 1877, II., 121.

heating pure phenol with oxalic and sulphuric acids for a few days to about 105°C . Although a good product is thus obtained, the yield is but small, while on heating the mixture longer or to a higher temperature bye-products are formed. Some chemists have stated that the formation of the latter cannot be avoided, and that it is very difficult to obtain thus a pure aurin. We believe that we are able to explain these different results; the formation of a pure compound depends much on the proportions and the manner in which phenol and the acids are mixed. As however the preparation of pure aurin by this method is a very tedious process, we endeavoured to obtain it from the commercial product by the method which we have described in our first paper and purifying it by repeated recrystallisation from alcohol, the crystals separating out first being used for further purification, because we found that the purer aurin is, the less soluble it becomes, its solubility being increased by the admixtures which are present in the crude product.

Another method for purification which we employed consisted in washing good commercial aurin with cold alcohol until only a comparatively small residue was left, which was then further treated as above stated.

The different crops of crystals were analysed, and the purification continued until the product did not change its composition any more. A great number of analyses were thus required, and we have great pleasure in expressing our best thanks to Mr. L. T. O'Shea, who performed them with great care.

The results did not, however, agree with our former; the product having a constant composition, and crystallising exceedingly well, gave numbers agreeing with the formula $\text{C}_{19}\text{H}_{14}\text{O}_3$. We therefore again prepared aurin from pure phenol, and crystallised it repeatedly from alcohol, until its composition became constant, and found it to have the same formula.

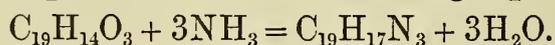
Aurin therefore appears to be the lower homologue of rosolic acid; like the latter, it readily forms a tetrabromocompound, which we obtained pure by the following process. Pure aurin was dissolved in hot glacial acetic acid, and this solution poured into a mixture of an excess of bromine and acetic acid. The liquid remained perfectly clear, but on standing for a few days the tetrabromaurin crystallised out almost completely. It forms small brownish green crystals, with a metallic lustre, which dissolve in alkalis with a purple colour. The determination of the bromine gave numbers agreeing closely with those required for the formula $C_{19}H_{10}Br_4O_3$.

Whilst this investigation was going on a new light was thrown on the subject by the researches of E. and O. Fischer. In order to elucidate the constitution of rosaniline these chemists endeavoured to convert rosaniline into the hydrocarbon from which it is derived. In this they did not succeed, but they were able to transform leucaniline $C_{20}H_{21}N_3$, which stands to rosaniline in the same relation as leucaurin to aurin, into a hydrocarbon $C_{20}H_{18}$, which is the mother substance of this base, and from which it follows that the hydrocarbon corresponding to rosaniline has the composition $C_{20}H_{16}$, which also confirms the correctness of Hofmann's formula for rosaniline. The leucaniline, which they employed was obtained from commercial rosaniline; but Rosenstiehl has shown that this body, which is prepared by oxidising a mixture of aniline, solid paratoluidine and liquid pseudotoluidine, contains several, probably isomeric, bases. E. and O. Fischer therefore in continuing their research* prepared one of them in the pure state by using a mixture of pure aniline and pure paratoluidine. The product was converted into the leuco-compound, from which, to their surprise, they obtained, not as before, the hydrocarbon $C_{20}H_{18}$, but one having the composition $C_{19}H_{16}$ or the lower

* Ber. Deutsch. Chem. Ges., XI. 195.

homologue of the former. They found the new hydrocarbon to be identical with one already known, which was discovered by Kekulé and Franchimont, and called Triphenylmethane $\text{CH}(\text{C}_6\text{H}_5)_3$ as it is marshgas or methane, in which three atoms of hydrogen are replaced by three phenyl-groups. Hemilian, who afterwards obtained the same hydrocarbon by a different method, examined it more fully and found amongst other results that concentrated nitric acid converts it into the trinitro-compound $\text{C}_{19}\text{H}_{13}(\text{NO}_2)_3$; E. and O. Fischer prepared the same body, which by the action of nascent hydrogen was converted into a base having all the characteristic properties of leucaniline. This was heated with arsenic acid, in order to abstract hydrogen by oxidation, and the rosaniline which formed the starting point of their last investigation was obtained again.

From these results it follows that commercial rosaniline consists principally of the base $\text{C}_{20}\text{H}_{19}\text{N}_3$, and is probably a derivative of pseudotoluidine, while the rosaniline from paratoluidine and which has therefore been called pararosaniline is its lower homologue, having the formula $\text{C}_{19}\text{H}_{17}\text{N}_3$. The rosaniline which we obtained from aurin is undoubtedly identical with the latter,* its formation being readily explained by the following equation:—



The question which now has to be solved is how to explain the formation of aurin; we have already made experiments in this direction and obtained interesting results, which, however, are not yet sufficiently worked out to publish them.

Another question to be elucidated is, what is the chemical constitution of aurin? According to E. and O. Fischer that of pararosaniline is $\text{C}_{14}\text{H}_{11}(\text{NH}_2)$, from which it follows that

* Since this was written E. and O. Fischer prepared some pure aurin according to our directions, and converted it into a leucorosaniline, from which they also obtained triphenylmethane (Deutsch. Chem. Ges. Ber. XI., 473).

aurin is $C_{14}H_{11}(OH)_3$. Already, in our first research, we obtained results which seem to confirm this view. On heating aurin with acetyl chloride, we obtained a colourless crystalline compound, which on analysis gave results nearly agreeing with the composition of triacetyl aurin $C_{19}H_{11}(OC_2H_3O)_3$, and on using benzoyl chloride a similar benzoyl-compound was obtained. The analytical numbers did not, however, sufficiently agree with those required by theory, and therefore we did not publish them; and, as the purification of these compounds was found to be beset with several difficulties, we did not then examine them further, because we believed the study of red aurin would more readily throw light on the subject. We have now prepared them again from pure aurin, and hope to be soon able to settle these points.

“The Origin of some Ores of Copper.” Part II. By CHARLES A. BURGHARDT, Ph.D., The Owens College.

Atacamite.

This interesting and beautiful mineral occurs generally in radiating foliated masses, or in bundles of acicular crystals which are characterised by a fine vertical striation upon their vertical faces. Globular, reniform, and stalactitic atacamite is also known. Thus it is safe to infer that in a great majority of cases the mineral has been formed in some way from a solution of copper salt, and not by igneous action. It is also a well known fact that atacamite crusts are occasionally found resting upon clefts in the lava of some volcanoes, being formed by the action of hydrochloric acid gas evolved by the volcano upon copper, or a compound of that metal, but these crusts are insignificant in quantity and comparatively scarce. In Chili, Bolivia, and the West Coast of Africa, very large quantities of atacamite are obtained as a commercial product; it therefore becomes interesting to the mineralogist to ascertain under what conditions these formations arose, and in order to do this a thorough chemical examination of the mineral itself is

necessary. Atacamite is known to occur in nature in three states of hydration, and as an example of the first I may mention a specimen from Algodon Bay, Bolivia, analysed by Bibra (Jahresbericht, 1858, 740), which had the following composition, viz. :—

Cu	59·25	per cent.
Cl	16·11	”
O	12·51	”
H ₂ O	12·13	”
	<hr/>	
	100·00	

The formula corresponding with the above analysis being $\text{Cu}_4\text{O}_3\text{Cl}_2 + 3\text{H}_2\text{O}$. The atacamite of Copiapo, Chili, as analysed by Field (Journ. Chem. Soc. (7) 3, 193) has the following composition, viz. :—

Cu	56·38	per cent.
Cl	14·95	”
O	10·78	”
H ₂ O	17·89	”
	<hr/>	
	100·00	

The formula for this being $\text{Cu}_3\text{O}_6\text{Cl}_4 + 9\text{H}_2\text{O}$, or $(\text{Cu}_4\text{O}_3\text{Cl}_2)_3 + 9\text{H}_2\text{O}$. The third state of hydration is represented in the atacamite from Botallack, Cornwall, analysed by Church (Journ. Chem. Soc. (2) 3, 212), and that from Tocopilla, near Cobija, Bolivia, analysed by Berthier (Ann. des Mines (3) 7, 542) the composition being as follows :—

Botallack Atacamite.		Tocopilla Atacamite.	
Cu	52·90	Cu	53·26
Cl	14·76	Cl	14·92
O	10·49	O	9·37
H ₂ O	22·45	H ₂ O	22·45
	<hr/>		
	100·00		100·00

and the formula $\text{Cu}_4\text{O}_3\text{Cl}_2 + 6\text{H}_2\text{O}$.

Having given a general idea of the composition of the various atacamites, I will now point out how some of them can be prepared artificially. Field (Phil. Mag. (4) XXIV., 1862, 124) prepared the last-mentioned variety by adding a solution of calcium hypochlorite to an excess of cupric sulphate, and boiling the mixture for some time, when an

apple-green precipitate was observed, which proved on analysis to be identical with Berthier's atacamite from Cobija. If the ebullition were not sufficiently kept up, the compound was found to contain only 4 atoms of water. Proust prepared a green compound (which no doubt was atacamite, although its exact composition is not given) by exposing moist cuprous chloride to the air, also by digesting a solution of cupric chloride with cupric oxide. In a former communication to this Society (Proc. Lit. and Phil. Soc. XVII., 1877-78, 27—36) I mentioned that on heating some crystals of cuprous chloride in water in a sealed tube at a temperature ranging from 160° — 180° C, that minute green spots of a substance resembling atacamite were observed upon the sides of the tube. Further, on heating cuprous oxide and a strong solution of chloride of sodium together in a sealed tube, at 150° — 180° chalcotrichite was formed, and also a green substance resembling atacamite. Wishing to ascertain whether atacamite could be prepared in larger quantity (in accordance with my supposition that all the copper ores are products of the decomposition of cuprous oxide, the latter substance being a secondary product of the oxidation of the metal itself) I made numerous experiments, mostly consisting in the heating of cuprous oxide with sodium chloride solution in sealed tubes, and obtained excellent results, by simply covering a quantity of cuprous oxide with a concentrated solution of sodium chloride, exposing this mixture to the air at the ordinary temperature, and stirring it up occasionally. By proceeding in this way a very concentrated solution of cuprous chloride in sodium chloride can be obtained. After standing a few days this solution begins to decompose, a green insoluble substance separating out continually, which can easily be filtered off from the liquid portion and well washed in cold distilled water, in order to free it from all traces of cuprous chloride and sodium chloride. The substance thus obtained

was carefully dried over calcium chloride until its weight was constant, and portions weighed out and analysed quantitatively with the following result, viz. :—

Cu	56·25	per cent.
Cl	14·29	„
O	10·95	„
H ₂ O	18·51	„
	<hr/>	
	100·00	

It will readily be seen that the above analysis agrees very closely with the one made by Field already referred to; whence it follows that the substance prepared by myself is identical in composition with the atacamite of Copiapo, Chili. The rationale of the formation is briefly this—“the cuprous chloride held in solution by the sodium chloride takes up oxygen from the air very rapidly, giving rise to the formation of cupric oxychloride and cupric chloride (the latter soluble in water), both eventually becoming hydrated.” The following equation expresses the first stage of the reaction, viz. :—



Nearly all specimens of atacamite are intimately associated with cuprite, so that it appears highly probable that the former mineral has been obtained from the latter in the manner stated, more especially as atacamite is found in veins of other copper ores, associated with quartz, chalcedony, &c., in diorite and syenite. Again, at Sierra do Lembe, near Ambriz, on the West Coast of Africa, there is a large occurrence of atacamite in the malachite deposits, the “gangue mineral” being quartz. Debray (*Zeitsch. f. Chem.* 1867, 276) prepared crystallised atacamite by heating together basic cupric nitrate and a concentrated sodium chloride solution for several hours at 200° C, also by heating copper ammonium sulphate with a strong sodium chloride solution at 100° C. Up to the present time I have not succeeded in preparing the mineral in a crystalline state. A curious fact I may mention, viz., that natural cupric oxychloride should increase in hydration by three atoms of water.

Ordinary Meeting, April 16th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

“Note on the Occurrence of Dioptase on Chrysocolla, from Peru,” by CHARLES A. BURGHARDT, Ph.D., the Owens College.

A short time ago, Mr. W. M. Hutchings, F.C.S., of Birkenhead, sent me some specimens of chrysocolla ($\text{CuSiO}_2 + 2\text{H}_2\text{O}$), from Peru, accompanied with a statement that there were some minute crystals in a cavity in one or two of the specimens which might possibly prove to be the rare mineral dioptase ($\text{CuSiO}_2 + \text{H}_2\text{O}$). I proceeded to make a crystallographical and chemical examination of the crystals, and found that although extremely small, the forms could be recognised under the microscope. The chrysocolla mass is eaten into in one or two spots, cavities being produced which are divided into numerous cells by the intersection of thin partitions of chrysocolla substance. The dioptase crystals occur particularly fine in small green tufts and sheaves attached to the partition walls of the cells, whilst those crystals clothing the interior of the cells are not so well developed as the others. The measurements so far obtained have not been satisfactory, owing to the extreme smallness of the crystals, but the forms observed are those peculiarly characteristic of dioptase, viz.: $\infty\text{P}2-2\text{R}$; the rhombohedron being extremely well defined. No other forms were observed, but a great many fine acicular sub-individuals growing parallel with each other build up a large individual. Some of the crystals I carefully picked out and examined chemically with the following results,

viz.: heated before the blowpipe they were infusible and turned brown, *not black*, probably owing to one of two causes, either (1) the flame was not a pure oxidising flame, and a little of the cupric oxide was reduced to cuprous oxide; or, (2) there was a slight admixture of quartz with the diopside crystals. The presence of copper was proved by dissolving a crystal in a drop of hydrochloric acid, evaporating off the latter, redissolving the residue in a drop of acetic acid and adding potassium ferrocyanide, when the characteristic copper reaction was very marked. There was not the slightest effervescence on dissolving the mineral in hydrochloric acid, but flocculent particles of silica separated out. Mr. Hutchings obtained identical results in a chemical examination of the crystals. From the above tests, the crystallographical examination and the fine emerald-green colour of the crystals, there can scarcely be a doubt that they are really those of diopside. Colourless quartz crystals were also observed associated here and there with the diopside, but $+R$ and $-R$ were both present, generally nearly in equilibrium, and the prism faces exhibited strongly the characteristic horizontal striation, therefore coupling the marked differences in colour and crystal-form a confusion of the two minerals could scarcely be possible.

I believe this is the first instance observed of diopside occurring simultaneously with chrysocolla, and according to my knowledge the first time it has been found in Peru, in fact out of Russia. The exact locality of the mine cannot unfortunately be ascertained. I am of opinion that the diopside has been formed by the action of water upon the chrysocolla, the latter being a product of the decomposition of cuprite (which is always intimately associated with it) by a solution of silicic acid in water. Very fine botryoidal malachite sometimes occurs associated with chrysocolla and cuprite in the same locality in Peru. I hope shortly to obtain accurate measurements of the diopside crystals.

“On Indigo-blue from *Polygonum tinctorium* and other plants,” by EDWARD SCHUNCK, Ph.D., F.R.S.

The author, after referring to his investigation of *Isatis tinctoria*, the common woad plant, the results of which were communicated to the Society many years ago,* proceeded to give an account of some experiments he had recently made with *Polygonum tinctorium*, a plant employed by the Chinese for the manufacture of indigo, his object being to ascertain whether the colouring matter is contained in this plant in the same form as in the *Isatis*, viz. as a glucoside. His experiments led to the conclusion that the leaves of *P. tinctorium* contain a substance which cannot be distinguished from the indican of the woad plant. It is amorphous, soluble in water, alcohol, and ether, and by the action of acids is decomposed into indigo-blue and a substance giving the reaction of glucose, probably indigucine. When its watery solution is boiled or left to stand for some time, it undergoes a complete change, and then no longer yields indigo-blue by decomposition with acids, but indigo-red and other products, indican, as formerly shown, undergoing a similar metamorphosis under the same circumstances.

The author recommends for the preparation of this substance the following process :—The leaves of the plant having been carefully dried, are ground to powder and extracted with spirits of wine. The green alcoholic extract is evaporated at the ordinary temperature, a current of air being employed to assist evaporation. After evaporation of the alcohol there is left a brown watery liquid, which is filtered from the deposited chlorophyll and fatty matters, and mixed with acetate of lead solution. This gives a copious dirty yellow precipitate, which is filtered off. Basic lead acetate added to the filtrate produces a primrose-yellow precipitate, which is filtered off, washed with water, then with alcohol,

* Memoirs, 2nd ser., XII. p. 177, and XIV. p. 181.

and then suspended in absolute alcohol. On passing a current of carbonic acid gas through the liquid the precipitate is decomposed, yielding lead carbonate, while the liquid acquires a yellow colour. The filtrate is evaporated in a current of air, and water is added to the residue, which it dissolves for the most part. Sulphuretted hydrogen is passed through the filtrate to precipitate the lead in solution, and the liquid after filtration is again evaporated. The residue is treated with ether, which leaves a portion undissolved. The ethereal solution leaves on evaporation a yellow amorphous residue, having all the properties of indican.

The author made some experiments with the fresh leaves of the plant, from which he concludes that the leaf cells contain no ready-formed colouring matter with the exception of chlorophyll, and that the indigo-blue which is formed when the vitality of the cells is destroyed by extreme cold, organic lesion, or chemical reagents, is produced by the decomposition of indican, which commences as soon as the protecting influence of the living cell is removed.

It has long been known that some orchidaceous plants, such as *Bletia Tankervilleae* and *Callanthe veratrifolia*, yield indigo-blue. The author examined the leaves of the former plant and obtained a solution, giving the reactions of indican, and he is consequently inclined to suppose that the latter will be found in all cases to be the source from which indigo-blue is derived.

The author mentions a fact which he thinks may be of interest not only to the chemist, but also to the physiologist. On one occasion an alcoholic extract of dried woad leaves, in which the indican had undergone partial decomposition by long standing, yielded on evaporation a quantity of a substance which, when purified, was found to have all the properties of tyrosine. Though it is possible that the tyrosine may have pre-existed in the plant, the author is inclined

to think that, like the leucine previously discovered by him, it was a product of decomposition of indican under conditions of which he is at present ignorant, especially as some connection is supposed by chemists to exist between tyrosine and indigo-blue, and on the other hand tyrosine and leucine so frequently occur together as products of decomposition of protein compounds.

“On the Internal Cohesion of Liquids and the Suspension of a Column of Mercury to a height more than double that of the Barometer,” by Prof. OSBORNE REYNOLDS, F.R.S.

Introduction.

The ease with which under ordinary circumstances the different portions of liquid may be separated is a fact of such general observation that the inability of liquids like water to offer any considerable resistance to rupture appears to have been tacitly accepted as an axiom. In no work on Hydrostatics does it appear that the possibility of water existing in a state of tension is so much as considered; and suction is always described as being solely attributable to the pressure of the atmosphere.

The limit of 32 feet, or thereabouts, to the height to which water can be raised by suction in the common pump and the sinking of the mercury in the barometer tube, leaving the Torricellian vacuum above, until the column is at most only 31 inches—sufficient to balance the highest pressure of the atmosphere—are phenomena so well known as to be almost household words with us. It is not, therefore, without some fear of encountering simple incredulity that I venture to state

The object of this Communication.

In the first place my purpose is to show that certain facts already fully established afford grounds for believing that almost all liquids, and particularly mercury and water, are capable of offering resistance to rupture commensurate with

the resistance offered by solid materials. In the second place I have to describe certain experimental results which as far as they go completely verify these conclusions and subvert the general ideas previously mentioned as to the limits to the height to which mercury can be suspended in a tube or water raised by suction. And in conclusion I shall endeavour to explain the nature of the circumstances which have resulted in the practical limits to these phenomena.

The Separation of Liquid is not caused by Rupture.

Although the smallness of the force generally requisite to separate a mass of liquid into parts leads to the supposition that the parts of the liquid have but little coherence, it may be seen on close examination that this supposition is not altogether legitimate. For such separation of a liquid as we ordinarily observe takes place at the surface of the liquid—is caused by an indentation or running in of the surface, and not by an internal rupture or simultaneous separation over any considerable area. Thus, when we see a stream of liquid break up into drops, the drops separate gradually by the contraction of the necks joining them,

FIG. 1.



FIG. 2.



as shown in fig. 1, and not suddenly as in fig. 2. And the ease with which portions of a liquid may be separated by the forcing or drawing in of the surface affords no ground for assuming that the liquid is without coherence, any more than does the ease with which we may cut a piece of string, cloth, or metal with

sharp shears, or even tear some of these bodies by beginning at an edge, prove that they are without strength to resist great force when these are applied uniformly so as to call forth the resistance of all the parts of the body simultaneously. It is true that under certain circumstances we observe the

internal rupture of liquid—whenever bubbles are formed, as when water is boiled—but under these circumstances we have no means of estimating the forces which cause the internal rupture; they are molecular in their action, and for all we know they may be very considerable. Having thus pointed out that the ease of separation of the parts of a mass of liquid does not even imply a want of cohesion on the part of the liquid, I shall now point out that we have in common phenomena

Evidence of Considerable Cohesion.

These are for the most part what are considered minor phenomena; they are confined to the surface of the liquid, and are included under what is called “capillarity,” or “surface-tension.”

The phenomena of capillarity, or surface-tension, have recently attracted a great deal of attention, and many important facts concerning them have been clearly elucidated, some of which bear directly on my present subject.

Of the phenomena I may instance the suspension of drops of water, the rising of water up small tubes, the tendency of bubbles to contract, and the spherical form assumed by small fragments of mercury.

These phenomena and others are found to be explained by the fact that the surface of these liquids is always under a slight but constant tension, as if enclosed in a thin elastic membrane.

No satisfactory explanation as to the cause of this surface-tension has, I believe, been as yet found; but the fact itself is proved beyond all question. It is a molecular phenomenon, and in order to offer any explanation as to its cause it would be necessary to adopt some hypothesis respecting the molecular constitution of the liquid. Such an explanation making the surface-tension to arise from the cohesion of the molecules of the liquid is, I believe, possible; but this is beside my present purpose, which will be completely served by showing that

The Surface-Tension proves the existence of Cohesion.

To prove this requires no molecular hypothesis, but before proceeding it may be well to define clearly the term cohesion.

Cohesion in a liquid is here to be understood as a property which enables the fluid to resist any tendency to cause internal separation of its parts—any tendency to draw it asunder, or more definitely, it is the property which enables a liquid to resist a tension or negative pressure.

Let us suppose a mass of liquid without internal cohesion. Then any external action tending to enlarge the capacity within the bounding surface of the liquid would at once cause the interior of the liquid to open, and a hollow would be formed within the liquid without any resistance on the part of the liquid. Such a condition is inconsistent with surface-tension, for the tension of the surface of the internal hollow would tend to contract the hollow; and since the interior of the hollow is supposed to be empty, there could be no resistance to the tendency of the surface to contract such as that offered by the pressure of the gas within an ordinary bubble. Hence any force that might, under the circumstances, balance the surface tension and keep open the hollow, must be supplied by the suction or cohesion of the liquid outside.—Q.E.D.

Again; the intensity of cohesion is determined by the intensity of the surface-tension, and the smallness of the least possible opening over the surface of which tension exists.

In so far as has yet been determined by experiment, it has been found that the surface-tension is independent of the curvature of the surface—is constant for the same liquid. Assuming that this is the case it follows, that the intensity of the force necessary to keep a spherical bubble or opening from contracting (whether this force arises from the pressure of the gas within the bubble or the cohesive traction of the liquid without the opening) is equal to twice the intensity

of the surface-tension divided by the radius of the sphere. Hence the cohesive tension must be equal to twice the surface-tension of the liquid divided by the diameter of the smallest opening for which the surface-tension exists.—
Q.E.D.

It immediately follows from the foregoing proposition, that no matter how small the surface-tension may be, provided it is finite, even when the opening is infinitely small, then the cohesion of the liquid must be infinitely great. For if the liquid were continuous in its origin the opening must always be infinitely small; and hence to cause such an opening would require infinite tension.

That the cohesion is infinitely great is not probable, to say the least. Hence it is improbable that the surface-tension remains finite when the opening becomes infinitely small. As has already been stated, it has been found that the surface-tension is constant, or nearly so, under ordinary circumstances; but it has never been measured for bubbles of very small diameter, and there appears to be every probability that when the size of the bubble comes to be of the same order of small quantity as the dimensions of a molecule the surface-tension must diminish rapidly with the size of the bubble.

If this is the case, then we have a limit to the cohesion, although it is probably very great for most liquids. Something like the cohesion of solid matter of the same kind. That is to say, it is probable that it would require nearly as great intensity of stress to rupture fluid as it would to rupture solid mercury; or as great tension to rupture water as to rupture ice.

The Effect of Vapour.

Nothing has yet been said about the effect of the pressure of vapour within the bubbles in balancing the surface tension. It may, however, be shown that this can be of no moment. Even supposing that the tension of the vapour

within the opening of the liquid were equal to the tension due to the temperature under ordinary circumstances, this would be inappreciable. So that unless the tension of vapour within small openings were much greater than that in larger openings for the same temperature, its effect might be neglected; and so far from this being the case, Sir William Thomson has shown that the pressure of the vapour within a bubble at any particular temperature diminishes with the size of the opening. Hence it is clear that this vapour can have no effect on the result—a conclusion verified by the now well known fact that water may be raised to a temperature high above 212° without passing into steam.

Experimental Verification necessary.

This line of reasoning has been apparent to me now for several years. I find notes on some of the principal points which I made in 1873, and for several years I have pointed out the conclusions arrived at as regards the probable cohesion of water to the students in the engineering class at Owens College. I have, however, hitherto refrained from publishing my views, because I had no definite experimental results to appeal to in confirmation of them. Experimental indications of such a cohesive force were not wanting, but they were not definite. And although methods of making definite experiments have often occupied my thoughts, certain difficulties, which turn out to have been somewhat imaginary, kept me from trying the experiments.

It had always appeared to me that in order to subject the interior of a liquid mass to tension, it would be necessary to, as it were, hold the surface of the liquid at all points to prevent its contracting. To accomplish this it was necessary to have the liquid in a vessel, to the surface of which the liquid would adhere as water adheres to glass. The experiment which I had conceived would have been equivalent to a vertical glass tube more than 32 feet long, closed at the upper end and open at the lower, so that when the

tube was full of water the column would be higher than the pressure of the atmosphere would maintain, and hence could only be maintained by the cohesion of the water. The difficulty of such an experiment, however, appeared to be great. It was clear that if mercury could be substituted for water this difficulty would be much reduced, but then mercury does not readily adhere to glass, and the ordinary method of making barometers seemed to disprove the possibility of making it adhere.

It was only on the 2nd of this month that an accidental phenomenon at once afforded me the experimental proof for which I had been looking.

First Experiments.

The phenomenon was observed in a mercurial vacuum gauge (a syphon gauge, which allowed of a column of mercury 31 inches long). Before the mercury was introduced the tube had been wetted with sulphuric acid, a few drops of which covered the mercury on both ends of the column.

The gauge had been in constant use as a vacuum gauge for three weeks, and, probably owing to the action of the acid on the mercury, a little gas had generated between the mercury and the closed end of the tube, sufficient to cause the column to sink to $27\frac{1}{2}$ when the barometer stood at 29. To get rid of this air the tube was removed from its situation and placed in such a position that the bubble of air passed along the tube and escaped, the open end of the tube being entirely free. Before the tube was tilled in this way the unbalanced column was $27\frac{1}{2}$ inches long. When tilled the mercury ran back right up to the end of the tube as the bubble of air passed out. On erecting the tube again the mercury remained up to the end of the tube, except about one-eighth of an inch which filled with sulphuric acid. The unbalanced column of mercury was therefore 31 inches long. At first the full significance of this phenomenon was not recognised,

but in order to ascertain that the tube was cleared of air it was moved gently up and down to see if the mercury clicked, as it usually does when the tube is free from air, but the mercury did not move in the tube. The rapidity of the oscillation was thereupon increased until it became a violent shake, and as the mercury still remained firm it was clear that some very powerful force was holding it in its place. The tube, being in a vertical position, was then left in order that the barometer might be consulted. This was standing at 29 inches. After a few seconds, when the gauge was again examined, the column no longer reached the end of the tube, but stood at 29 inches. As it was singular that the mercury should have quietly settled down after having resisted such violent shaking, the tube was again inclined until the mercury and acid came apparently up to the end of the tube; but this time on the erection of the tube the mercury at once settled down. That is to say, it settled down gradually as the tube was erected. At first what appeared to be a very small bubble opened in the sulphuric acid, and this enlarged as the top of the tube was raised. On again inclining the tube until it was horizontal, and examining it closely, a minute bubble could be seen in the acid, and it was this bubble which expanded as the tube was erected, and so allowed the mercury to descend. To get rid of this bubble the tube was turned down so as to allow the bubble to pass along the tube, but owing to its small size it would not pass many inches along the tube before it became fixed between the mercury and the glass. When the bubble came to a standstill at about 6 inches from the end of the tube the gauge was again erected, the bubble immediately began to move back, but so slowly that it was some seconds before it entered the region of no pressure. During this interval the mercury remained up to the end of the tube, but the bubble, as soon as it neared the top of the tube, expanded and rapidly rose to the top of the tube,

leaving the column at 29 inches. This operation having been repeated several times, it became quite evident that it was this small bubble which either by rising up the tube or being generated at the top had caused the mercury in the first instance to sink. As the bubble would not pass out by itself the tube was tilted so as to allow a larger bubble of air to enter, and having been left standing for about 12 hours to allow the small bubble to unite with the larger one, it was again tipped so as to allow the air to pass out. When this was done the mercury again remained firmly against the end of the tube and did not descend when violently shaken. The open end of the tube was then connected with an air pump and exhausted until the pressure within it fell to about 4 inches of mercury. This operation occupied some seconds, but all this time the mercury did not move from the end of the tube, but eventually the column opened near the bottom of the tube and a large bubble appeared which rose up the tube, the mercury falling past through the opening. That the breaking of the column so near the bottom of the tube was owing to the presence at that point of a small bubble of air was almost proved by the fact that on readmitting the air to the open end of the tube and inclining the tube to see if it was free from air, there was found a minute bubble which played exactly the same part as the small bubble which had been previously examined.

At the instant previous to the rupture of the column at the bottom of the tube there must at the top of the tube have been an unbalanced tension or negative pressure equal to 27 inches of mercury, and this tension did not break the continuity of the column. Hence I had a proof that the cohesion within the mercury and the sulphuric acid as well the adhesion of the sulphuric acid to the mercury and the glass is sufficient to resist this very considerable tension.

Further Experiments

In the hope of improving the experiments another gauge

was constructed, the tube being $\frac{5}{8}$ ths of an inch in internal diameter, and 35 inches high. Into this tube mercury and sulphuric acid were introduced, as in the first tube. But on trying to get rid of the small bubbles of air, it was found impossible to do so as small bubbles were continually generated. Hence it appeared that the three weeks during which the mercury and sulphuric acid in the first tube had remained in contact had had an important influence on the result. Failing in this attempt, it occurred to me to try if water would answer the purpose as well as sulphuric acid. Having in my possession an old vacuum gauge, with a column 3 inches long, which had originally been wetted with sulphuric acid, but into which a considerable quantity of water had accidentally been introduced, I carefully allowed all the air to escape, and then applied a mercurial air pump to the open end of the gauge, and exhausted as far as the pump would draw. The mercury did not descend. As I could apply no further tension I shook the gauge up and down, but still the mercury remained unmoved. I then tapped the gauge smartly on the side, the mercury then fell 3 inches, until it was level. Having succeeded so far, I extracted the mercury and sulphuric acid from the 35 inch gauge and introduced some water without washing the tube, and having boiled the water in the tube, again introduced the mercury.

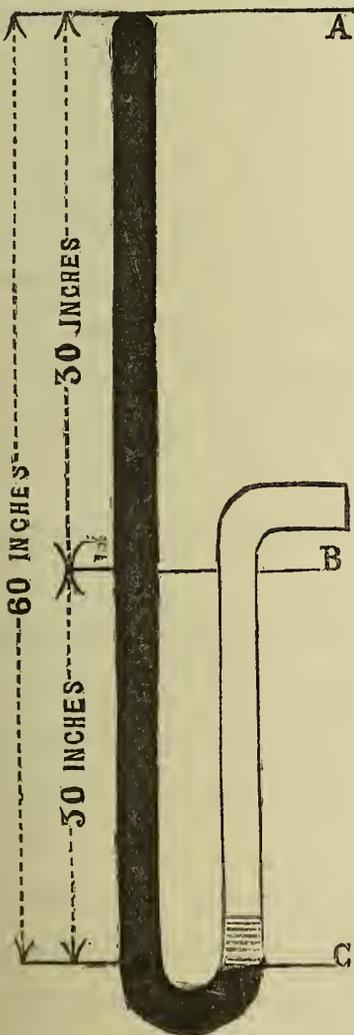
Having extracted all the air, I found no difficulty in making the gauge to stand up to the 35 inches without any immediate tendency to fall. On applying the air pump to the open end the mercury several times remained up until the exhaustion had proceeded so far that when it fell it fell from 22 to 28 inches, and when the rupture took place it was accompanied with a loud click. I could not on that occasion get the mercury to withstand complete exhaustion, but after leaving the gauge with the mercury suspended for 24 hours at 35 inches, I was able to exhaust the open end of

the tube as far as the pump would draw, without bringing the mercury down, so that I had a column of 35 inches of mercury suspended by the cohesion of the liquids.

There was no reason to suppose that this was the limit or anywhere near the limit. It was clearly possible to suspend a longer column, but as the length of the column increased so would the difficulty of getting rid of the disturbing causes, and I determined to rest satisfied with the 35 inches; but in order to see if this could be maintained, I obtained a gauge 60 inches long, so that this would leave 30 inches above the pressure of the atmosphere.

The difficulty of getting rid of the air in this tube sufficiently to allow of the mercury standing 60 inches was very considerable. Before filling the tube it was rinsed out with

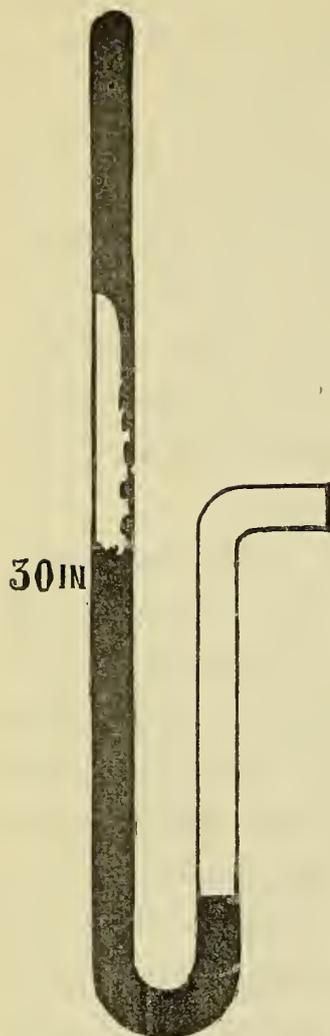
FIG. 3.



concentrated sulphuric acid, then twice washed with distilled water, and then water put in and boiled in the tube. Then sufficient mercury was introduced to fill the long leg and the bend, so that the column when complete was 59 inches long, the barometer being at 29.5.

After the tube had been tilted several times so as to allow the air to pass out, the mercury would be suspended as the tube was slowly re-erected, until it had attained an elevation of 40, 50, or sometimes the full height, as shown in Fig. 3; but only for a few seconds. When the mercury fell, if the column broke anywhere near the top of the tube, it gave way with a loud click. But this was by no means always the case. The mercury would sometimes sepa-

FIG. 4.



rate nearly 30 inches down the tube, and then the appearance of the upper portion falling was very singular. The upper portion of the column remained intact, and a stream of mercury fell from its under surface, as shown in Fig. 4, breaking up into globules as it came into contact with the lower portion with a loud rattling noise. I was unable to get the column in the tube thus filled to maintain itself for more than 20 or 30 seconds, which failure was clearly due to the presence of air, for after the mercury had fallen a small quantity of air was always found to collect above it. Sometimes when, on inclining the tube, the liquid again reached the top, the bubble which remained was so small as to be scarcely visible, although subject to no pressure other than the surface tension, but its presence always became apparent instantly on erecting the tube. In no case was it possible, after the mercury had once fallen, to get it to remain up to, any considerable height above that due to the pressure of the atmosphere until the bubble of air collected had been allowed to pass out.

The tube was then again emptied, washed, and filled with glycerine. This behaved much in the same manner as the water, but the difficulty of getting rid of the air was greater.

Similar results were obtained when very dilute ammonia liquid was tried.

The tube was then again carefully washed, first with water and then several times with concentrated sulphuric acid. The mercury was subjected to nitric acid, washed and dried, and then filtered into a bottle of sulphuric acid, from

which it was poured into the tube, some acid passing in with the mercury. When first introduced into the tube a few small bubbles could be seen rising between the mercury and the tube and passing up through the sulphuric acid into the vacuum above; but after it had stood for five or six hours no bubbles were perceived, the surface of the mercury against the tube being perfectly clear; nevertheless, on erecting the tube the mercury would not rise above the height of the barometer, and air was always found to have collected above the mercury. Water was then introduced so as to dilute the acid. Then the mercury was suspended as before only for a few seconds. The tube was then placed in a position with the closed end lowest, so that the air and water might ascend towards the end and pass out, and after being in this position for some hours, when it was again erected, the column remained intact.

It was thereupon again lowered, and left to drain for forty-eight hours. On being again erected the mercury was still suspended. The tube has since been carried in a more or less horizontal position some three miles to the Society's rooms, in order that I might exhibit the phenomenon. If it has not been affected by the shaking you will see a column of mercury suspended some fifty-nine inches, or twenty-nine inches above the height due to the atmosphere.*

Conclusion.

The difficulty of obtaining a column of mercury 30 inches above the pressure of the atmosphere does not, I think, prove that the limit of the cohesive power of the liquid has been arrived at, or even the limit of the adhesive power of the water for glass and mercury; but simply shows that, although imperceptible, there are still bubbles of air in the liquid between the mercury and the glass which will not readily pass out.

It seems to me to be probable that with sufficient care,

* At the meeting not only did the mercury remain suspended when the tube was erect, but on the pressure of the atmosphere being removed with an air pump it still remained suspended, although the tension at the top of the tube was nearly equal to two atmospheres.

or by using apparatus more suitable to the purpose, much greater heights might be attained. But however this may be, we have proof that mercury and water will by their cohesion resist a tension of at least one atmosphere, or that the common pump would, if the water were free from air, raise water by suction to a height of more than 60 feet. At first sight it cannot but appear remarkable that such a fact should for so long have escaped notice, but a little consideration removes the difficulty.

Water is almost always more or less saturated with air, which separates into bubbles as soon as the pressure is relieved, and in the common pump a single minute bubble would be sufficient to cause the column to break, and prevent it being raised to a greater height than that due to the pressure of the atmosphere.

In the case of barometers it is the custom to fill the tubes full and boil the mercury, so as to get rid of the air; but the column falls to the usual height not by the rupture of the mercury, but by the separation of the mercury from the glass, for which it has but little adhesion. Whether the ordinary method of boiling the mercury really disengages all the air is, I think, an open question. In vacuum gauges of small diameter it is not uncommonly found that the mercury sticks to the glass until the pressure has fallen considerably below what is represented by the height of the mercury, so that on the gauge being shaken the mercury falls with a sudden drop. Although it does not seem to have attracted any especial notice, this phenomenon is clearly due to the same cause as that which I have found capable of maintaining 30 inches of mercury suspended in a comparatively large tube.

It would seem then that although the facts which I now bring before the Society have little bearing on the practical limits to the height of the column of mercury in the barometer or the column water in the common pump, they show

that these limits are owing to the presence of air or some other minor disturbing cause, and are not, as seems to have been hitherto supposed, owing to the want of cohesion in the liquid. And it seems to me that the cohesion now found to exist occupies an important as well as interesting place in the properties of liquid.

Appendix, 26th April.

Previous Notices of the Cohesion of Liquids.

Besides the hanging of mercury in small gauges, another phenomenon which has long been known, shows a small degree of cohesion in water; this is, that water will rise up small tubes by capillary attraction as well in the receiver of an air-pump as in air at the ordinary pressure. This fact was shown before the Royal Society by Robert Hooke.

Prof. Maxwell, in his *Treatise on the Theory of Heat*, p. 259, after commenting on the fact that water has been raised to a temperature of 356° F. without boiling, remarks: "Hence the cohesion of water must be able to support 132lbs. weight on the square inch." From which it would appear that he recognises cohesion as a property of water, and considers that the possibility of raising the temperature above the boiling point is evidence of such cohesion, but I am not aware that he has anywhere given his reasons for such a conclusion.

I am indebted to Dr. Bottomley for reference to a paper in the "*Ann. de Chim. et de Phys.* III., XVI., 167," by M. F. Donny, in which M. Donny gives an account of experiments in which he found that columns of sulphuric acid could be suspended in vacuo to a height of 1.3m., about 50 inches, showing a tension of about 7 inches of mercury, care having been taken first to remove all the air from the acid. M. Donny further describes experiments made with water in exhausted tubes, in which he showed the effect of cohesion by shaking the tube. M. Donny does not, however, appear to have thought of the plan which I adopted of

making mercury adhere to the tubes by wetting them with sulphuric acid or water. Not being able to use mercury the tensions which he obtained were comparatively small, and although he seems to have considered that greater tensions might be obtained he mentions one or two atmospheres as probably possible. It would therefore appear that he had not conceived the possibility of the cohesion of liquids being comparable with that of solids.

M. Donny appears to have been influenced in adopting this limit to his idea of cohesion by a passage from Laplace, *Mécanique Celeste*, Supplement au X^e livre, p. 3, which he quotes.

Laplace, who was the first to investigate systematically the phenomena of capillary attraction, proceeded on the hypothesis that the molecules of a liquid exercise attraction for each other at insensible distances only, and from this assumed attraction he deduces the surface phenomena. The entire passage quoted by M. Donny is too long to introduce here, but the gist of it is comprised in the following extract : —

“Son expression analitique est composée de deux termes: le premier, beaucoup plus grand que le second, exprime l'action de la masse terminée par une surface plane; et je pense que de ce terme dependent la suspension du mercure dans un tube de baromètre à une hauteur deux ou trois fois plus grande que celle qui est due à la pression de l'atmosphère, le pouvoir refringent de corps diaphanes, la cohesion, et généralement les affinités chimiques.”

Laplace here speaks of the suspension of mercury to 60 or 90 inches as if it were a well known phenomenon; but I cannot find any reference to experiments, or indeed any further mention of the phenomenon, in his memoir.

I did not refer to Laplace in the first instance, although I knew well that it is to him we are indebted for the theory of surface tension almost in the form now accepted, because I wished to avoid all reference to molecular hypothesis, and particularly the molecular attractions assumed by Laplace,

lest it might in any way appear as if the conclusion that continuous liquids are as capable of resisting tension as solids, at which I arrived simply from considering the phenomena of surface tension was based on such assumptions. I was not aware, however, that Laplace had at all inferred or attempted to apply his theory to prove the ability of liquids to resist great tensions, nor do I find on again reading his memoir that he anywhere, with the exception of the almost casual reference quoted above, treats of such a property of liquids. His purpose appears to have been solely to explain the phenomena of capillarity. It appears obvious, moreover, that his line of reasoning must have forced upon his notice the conclusion that according to his hypothesis liquids ought to possess the property of very great cohesion, so that from the extremely slight notice which he has accorded to this property, one can only infer that he was not completely convinced of its existence.

“On the Estimation of Hyposulphites and Sulphites,” by J. GROSSMANN, Ph.D. Communicated by Professor ROSCOE F.R.S.

Bunsen, in his treatise “On a Volumetric Method of very general applicability,” *Ann. Chem. & Pharm.*, has shown what degree of exactness may under certain circumstances be attained by indirect analysis. Upon the latter I have founded a very simple method for the estimation of hyposulphites and sulphites, which yields exact results. I shall speak in the following only of the sodium salts of the above compounds, as it will make the explanation clearer.

Let us assume first that we have only a mixture of sodium hyposulphite and sulphite, free from sulphate, in solution. If on the one hand we estimate how many grammes of iodine are required by an aliquot part of the mixture—acidulated with acetic acid—to yield the well known final blue reaction with starch, and if, on the other hand, we find how many grammes of sodium sulphate an equal measure of the mixture yields on perfect oxidation with chlorine or bromine, all the data necessary for the calculation of the respective quantities of

hyposulphite and sulphite are in our possession, and we can form two equations from which both the unknown quantities x_1 and y_1 can easily be calculated.

Let x_1 = grammes iodine corresponding to $\text{Na}_2\text{S}_2\text{O}_3$ present,
 y_1 = grammes iodine corresponding to Na_2SO_3 ,,
 A = the quantity of iodine which the unit requires for testing with starch.

and B = grammes Na_2SO_4 which the unit yields on perfect oxidation,

then

$$\begin{aligned} x_1 + y_1 &= A \\ \frac{2\text{Na}_2\text{SO}_4}{\text{Na}_2\text{S}_2\text{O}_3} \cdot \frac{\text{Na}_2\text{S}_2\text{O}_3}{\text{I}} \cdot x_1 + \frac{\text{Na}_2\text{SO}_4}{\text{Na}_2\text{SO}_3} \cdot \frac{\text{Na}_2\text{SO}_3}{2\text{I}} \cdot y_1 &= B \\ \text{or } \frac{2\text{Na}_2\text{SO}_4}{\text{I}} x_1 + \frac{\text{Na}_2\text{SO}_4}{2\text{I}} \cdot y_1 &= B' \end{aligned}$$

which gives us

$$\begin{aligned} x_1 &= \frac{2\text{I}}{3\text{Na}_2\text{SO}_4} \cdot B - \frac{1}{3}A \\ y_1 &= \frac{2}{3}A - \frac{2\text{I}}{3\text{Na}_2\text{SO}_4} \cdot B. \end{aligned}$$

To get the result as grammes $\text{Na}_2\text{S}_2\text{O}_3$ and Na_2SO_3 it is only necessary to multiply x_1 by $\frac{\text{Na}_2\text{S}_2\text{O}_3}{\text{I}}$, and y_1 by $\frac{\text{Na}_2\text{SO}_3}{2\text{I}}$. From

$$\begin{aligned} \text{this we find } x &= \text{grammes } \text{Na}_2\text{S}_2\text{O}_3 = \frac{2\text{Na}_2\text{S}_2\text{O}_3}{3\text{Na}_2\text{SO}_4} \cdot B - \frac{\text{Na}_2\text{S}_2\text{O}_3}{3\text{I}} \cdot A \\ &= \cdot 741784 B - \cdot 414698 A. \\ y &= \text{grammes } \text{Na}_2\text{SO}_3 = \frac{2\text{Na}_2\text{SO}_3}{3\text{I}} \cdot A - \frac{\text{Na}_2\text{SO}_3}{3\text{Na}_2\text{SO}_4} \cdot B \\ &= \cdot 661417 A - \cdot 295775 B. \end{aligned}$$

Care must of course be taken that the dilution while titrating is sufficient, and at least two titrations ought to be made.

If besides hyposulphite and sulphite, sulphate is present it is necessary to estimate this by itself, and the Na_2SO_4 thus found subtracted from the total Na_2SO_4 is B. As long as there is no sulphide present this can be done in the following manner. The liquor to be examined is heated in a flask and carbonic acid passed through till all the air is expelled. Then hydrochloric acid is added in excess, and the liquid boiled down in a CO_2 atmosphere to one fourth of its volume. After filtering it is precipitated with barium chloride as usual.

·21385 grammes Na_2SO_4

were mixed with a very great excess of $\text{Na}_2\text{S}_2\text{O}_3$ and Na_2SO_3 and yielded after being treated as described above

·2151 grammes Na_2SO_4

If sulphide is present this must be got rid of by means of cadmium carbonate before starting the analysis.

A mixture was prepared containing per unit

Na_2SO_4	·4165 grammes	=	39·06
$\text{Na}_2\text{S}_2\text{O}_3$	·3946	„	= 37·01
Na_2SO_3	·2551	„	= 23·93
	<hr/>		<hr/>
	1·0662		100·00

and the following data got

Total Na_2SO_41·4174 grammes

Actual Na_2SO_4 ·4165 „

therefore $B = 1·0009$

$A = ·833189$

from which we calculate

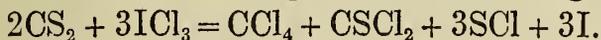
Na_2SO_4	·4165 grammes	=	38·98
$\text{Na}_2\text{S}_2\text{O}_3$	·3969	„	= 37·15
Na_2SO_3	·2550	„	= 23·87
	<hr/>		<hr/>
	1·0684		100·00

I have used this method with great advantage in the analysis of intermediate as well as final products in the manufacture of soda ash and caustic soda.

Laboratory of Gerard's Bridge Chemical Works,
St. Helens.

“Note on the Action of Iodine Trichloride upon Carbon Bisulphide,” by J. B. HANNAY, F.R.S.E., F.C.S.

It is stated by Weber that when iodine trichloride is added to carbon bisulphide a new product is formed. Now I have examined the action of these two substances upon each other, and find it is according to the following equation:—



On adding carbon bisulphide to pure iodine trichloride till it was all decomposed, considerable heat was evolved, and on cooling, iodine crystallized out. A qualitative examination of the liquid showed the presence of sulphur chloride in large quantities, and on decomposing this with water, the charac-

teristic smell of the sulphochloride of carbon was observed. This was further recognised by its deportment with alkalies, and after its removal from the liquid, the tetrachloride of carbon was easily recognised by its peculiar sweet smell strongly reminding one of the smell of primrose leaves.

A weighed portion of the trichloride was heated with the requisite amount of carbon bisulphide to convert it into the above products, when it was found there was neither an excess of carbon bisulphide nor of the trichloride present. The liquid was allowed to cool, and when all the iodine had crystallized out, it was passed through a small filter of asbestos and washed with a few drops of carbon tetrachloride. The iodine on the filter was estimated and gave only a little under the required amount. The filtrate was then treated with caustic potash, and the separated carbon tetrachloride weighed after transference to a tared bulb. The sulphur in the liquid, left after removal of the tetrachloride of carbon, was oxidised by means of potassium chlorate and strong nitric acid, and estimated as barium sulphate.

The following table gives the figures which were obtained on working with 10 grammes of iodine trichloride, and also those deduced from the above equation:—

	Found.	Calculated.
CCl ₄	2·05	2·198
CSCl ₂	1·71	1·641
CS ₂ Cl ₂	2·90	2·890
I	5·37	5·428

“Note by A. W. WATERS to his paper on the Bryozoa, page 125.”

At the time I wrote my paper I was unable to consult Thompson's “Zoological Researches,” but upon referring in London I find that as the quotation given relates to the Flustraceæ it would have been better to have given the following from page 97: “The other species of Sertularia in which the animals have been determined to be Polyzoæ.”

Also on page 132 of my paper read for “cœnosare”—“endosare,” which term is used by Joliet for the first time.

April 11th.

Annual General Meeting, April 30th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. William Dugdale Harland, of Manchester, was elected an Ordinary Member of the Society.

Report of the Council, April, 1878.

It will be seen from the Treasurer's Annual Statement appended to this report that there is apparently a balance against the General Account of £10 14s. 5d.; but this is much more than covered by rents which are due from the Geological Society and the Scientific Students' Association for the use of the Society's rooms.

The number of ordinary members on the roll of the Society on the 1st of April, 1877, was 162, and three new members have since been elected; the losses are—deaths 4; resignations 4; defaulter 1. The number on the roll on the 1st instant was 156. The deceased members are Mr. Colin Mather, Mr. G. V. Vernon, F.R.A.S., Mr. W. L. Dickinson, and Mr. John Thomas Hobson.

Mr. George Venables Vernon was the only son of Mr. John Venables Vernon, of the firm of Vernon and Edge, engravers to calico printers, David-street, Manchester, and was born on the 7th of October, 1831. He received a good education, and appears to have displayed a strong taste for scientific pursuits at an early age. On the 14th of January, 1853, he was elected a Fellow of the Royal Astronomical Society, and on the 30th of April, 1861, he was elected a Member of this Society, and afterwards contributed many valuable papers and communications, chiefly on meteorological subjects, to its Proceedings and Memoirs. He served

as a member of the council, and was for several years Honorary Secretary of the Physical and Mathematical Section. He was a Fellow of the Meteorological Society, and of the Anthropological Society, and a Member of the Meteorological Society of Scotland, and of the Meteorological Society of France.

Mr. Vernon carried on the business of a cotton spinner until his death, which occurred on the 11th of January, 1878, at the age of 46 years.

In private life Mr. Vernon was genial and kind-hearted and he was very fond of music and other accomplishments.

William Leeson Dickinson was the eldest son of Robert Dickinson, mercer, of Nottingham, and Mary his wife, eldest daughter of William Leeson, of Halam, near Southwell, gentleman, and was born on the 15th of August, 1814. He was educated at Arnold Vicarage, near Nottingham, by the Rev. William Howard, and afterwards apprenticed to Mr. George Gamble, of Gainsbro', chemist. Upon attaining his majority he went to Manchester, and was for some time assistant to the late Mr. Hargreaves, chemist, Market-street, upon leaving whom he commenced business as a drysalter. From his boyhood he was passionately fond of music, and took great delight in the study of mathematics, astronomy, and statistics. He was elected a member of the Society on the 23rd January, 1855, and afterwards contributed the following papers and communications to the Society's Proceedings :—

Eclipse of the Sun, March 15, 1858	Vol. I., p.	7
Eclipse of the Sun, July 18, 1860	„	206
Occultation of Jupiter, May 24, 1860.....	„	244
Note on the Transit of Mercury, Nov. 12, 1861 ...	„ II.,	151
On the Eclipse of the Sun, Dec. 31, 1861	„	152
Eclipse of the Sun, May 17, 1863	„ III.,	66
Eclipse of the Sun, Oct. 10, 1865	„ V.,	2
Eclipse of the Sun, Oct. 8, 1866.....	„	170
Occultation of Aldebaran, Sep. 28 and Nov. 22, 1866	„	170

Occultation of Aldebaran, Jan. 18, 1867	Vol. VI.,	47
On the Eclipse of the Sun, March 5, 1867	„	107
On three Occultations of Saturn by the Moon	„ IX.,	149
On the Eclipse of the Sun, Dec. 21-22, 1870	„	158

On the 11th of October, 1871, Mr. Dickinson had a paralytic attack, from which time he never was able to return to his warehouse; he was removed to Halam on the 15th of March, 1872, and died February 23rd, 1878.

John Thomas Hobson, Ph.D., F.C.S., was born in Manchester and educated mainly at Owens College, where he obtained the Dalton Scholarship. He afterwards studied in Marburg under Professor Kolbe, and took there his degree. He spent several years as chemist with the firm of Thomas Hoyle and Sons, and in 1863 he was appointed sub-inspector under the Alkali Act. To the duties of this office he gave all his attention, until an accident broke down his vigour, which after about ten years' illness ended in his death in December, 1877.

The Natural History Fund of £1,500, presented by the late Natural History Society, has been received from the Trustees of the Owens College by the Treasurer, and the Council have given instructions for its investment in any of the stocks or securities authorised by the trust deed.

The following papers and communications have been read at the Ordinary and Sectional Meetings of the Society during the session :—

October 2nd, 1877.—“A Case of Flowering of *Chaemerops Fortunei* (Hook), at Alderley,” by Arthur W. Waters, F.G.S.

“Table of Effect of Movement of the Surface of the Globe on the Shifting of the Axis of the Earth,” by Arthur W. Waters, F.G.S.

October 8th, 1877.—“Address on the Recent Progress of Natural History,” by Professor W. Boyd Dawkins, F.R.S., &c.

October 16th 1877.—“On some Coal Measure Plants and other Organic Remains from Spain,” by E. W. Binney, F.R.S., F.G.S., President.

“Note on an Edible Clay from New Zealand,” by M. M. Pattison Muir, F.R.S.E.

“On the Decomposition of Calcium Sulphate by Alkaline Chlorides. A contribution to Agricultural Chemistry,” by M. M. Pattison Muir, F.R.S.E.

“On Some Thionates,” by H. Baker, student in the Owens College. Communicated by Professor C. Schorlemmer, F.R.S.

November 5th, 1877.—“On the Position and Affinities of the Genus *Rostellaria* (R. *Martini* Marrat),” by J. Cosmo Melvill, M.A.

November 13th, 1877.—“Note on Envelopes and Singular Solutions,” by Sir James Cockle, F.R.S., Corresponding Member of the Society.

“On the Formation of Hailstones, Raindrops, and Snow-flakes,” by Professor O. Reynolds, F.R.S.

November 27th, 1877.—“On the Origin of some Ores of Copper (Part I.),” by Charles A. Burghardt, Ph.D., the Owens College.

“On the Construction of a Room, or Series of Rooms, free from Germ Life, proposed for use in the performance of Surgical Operations,” illustrated by a model room and apparatus, by William Thomson, F.R.S.E.

December 4th, 1877.—“Transit of the Shadow of Titan across the Disc of Saturn, November 23rd, 1877,” by Joseph Baxendell, F.R.A.S.

December 11th, 1877.—“Note on the Daguerreotype Portrait taken of the late Dr. Dalton,” by J. B. Dancer, F.R.A.S.

“Note on Metallic Niobium and a New Niobium Chloride,” by Professor H. E. Roscoe, F.R.S., &c.

“On the Retention of Saline Impurities by Hydrated Ferric Oxide,” by Mr. Charles Frederick Cross, Dalton Scholar in the Owens College. Communicated by Professor H. E. Roscoe, F.R.S.

December 26th, 1877.—“Notice of a Large Boulder Stone at Old Trafford, Manchester,” by E. W. Binney, F.R.S., F.G.S., President.

“On the Geometrical Representation of the Equation of the Second Degree,” by Charles Chambers, F.R.S., Superintendent of the Colaba Observatory, Bombay. Communicated by J. A. Bennion, F.R.A.S., A.C.P.S.

January 8th, 1878.—“On a Large Meteor Observed on the 31st of December, 1877,” by E. W. Binney, F.R.S., F.G.S., President.

“Note on the Decomposition of Water by Iron-pyrites,” by C. A. Burghardt, Ph.D.

“On a Remarkable Japanese Picture,” by R. D. Darbishire, F.G.S.

“On the Microscopic Conditions of a Slab from the Mountain Limestone of Bolland,” by Professor W. C. Williamson, F.R.S.

January 14th, 1878.—“The Mollusca of Cymmeran Bay, Part IV.,” by John Plant, F.G.S.

January 22nd, 1878.—“On the Cubic Integral

$$u = \int \frac{\alpha dx}{\beta \sqrt{(a-x)(b-x)(c-x)}},$$

by Robert Rawson, Esq., Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society; Mem. of the London Math. Society.

“Notice of a Fossil Plant found at Laxey, in the Isle of Man,” by E. W. Binney, F.R.S., F.G.S., President.

“On Ptolemy’s Geography, with Reference to the Coast from Caernarvon to Cumberland,” by Thomas Glazebrook Rylands, Esq. Communicated by Dr. R. Angus Smith, F.R.S., &c.

February 5th, 1878.—“On an Improved Method of Projecting Lissajous’ Figures on the Screen,” by J. Dixon Mann, L.K.Q.C.P.

February 11th, 1878.—“On the Best Method of Collecting and Preserving Plants for Herbarium Purposes, when gathered in Tropical or Subtropical Climates,” by H. A. Hurst, Esq.

February 19th, 1878.—“Note on a Method for Determining the Coefficients in Chemical Equations,” by James Bottomley, D.Sc.

February 25th, 1878.—“Results and Deductions of Rain-gauge Observations made at Eccles, near Manchester, during the year 1877,” by Thomas Mackereth, F.R.A.S., F.M.S.

March 5th, 1878.—“On the Decomposition of Ultramarine by Carbonic Acid,” by M. S. Sugiura (student in the Chemical Laboratory of the Owens College). Communicated by Professor Roscoe, F.R.S., &c.

“On Silicious Fossilization,” by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College. Communicated by Professor Roscoe, F.R.S., &c.

March 11th, 1878.—“On Bryozoa,” by Arthur Wm. Waters, F.G.S.

“On Metallic Growths on the Surfaces of Specimens of Auriferous Quartz from Australia and North Wales,” by John Plant, F.G.S.

March 19th, 1878.—“On a Remarkable Flash of Lightning,” by B. St. J. B. Joule, Esq.

“On a Barometer,” by Dr. J. P. Joule, F.R.S., &c.

“On a New Form of Portable Sprengel Pump,” by C. H. Stearn, Esq.

“A Comparison of the Standard Barometer of the Owens College Physical Laboratory with the Working Barometer,” by M. Morisabro Hiraoka, student of Owens College. Communicated by Professor B. Stewart, LL.D., F.R.S.

“On a New Calorimeter,” by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

April 2nd, 1878.—Three Letters from the late Sir William Fairbairn, Bart., F.R.S., a former president of the Society, to the late Professor Hodgkinson, F.R.S., also a former president of the Society. Communicated by E. W. Binney, F.R.S., F.G.S., President.

“On Aurin,” by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.

“The Origin of some Ores of Copper (Part II.),” by Charles A. Burghardt, Ph.D., the Owens College.

April 16th, 1878.—“On Indigo-blue from *Polygonum tinctorium* and other Plants,” by Edward Schunck, Ph.D., F.R.S.

“On the Internal Cohesion of Liquids,” by Professor Osborne Reynolds, M.A., F.R.S.

“On the Estimation of Hypo-sulphites and Sulphites,” by F. Grossmann, Ph.D. Communicated by Professor Roscoe, F.R.S.

“On the Action of Iodine Trichloride on Carbon Disulphide,” by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College. Communicated by Professor Roscoe, F.R.S., &c.

Many of the above papers have already been printed in the new volume of *Memoirs*, and others have been passed by the Council for printing.

The Council consider it desirable to continue the system of electing Sectional Associates during the ensuing session,

and a resolution on the subject will be submitted at the annual meeting for the approval of the members.

The Librarian reports that since the last annual meeting the binding of the books has been very nearly finished up to date, and we have completed many of the series of the Societies that were wanting. The Microscopical and Natural History Section have added many standard works to the Library during the year, amongst which are "The Ibis," Hewitson's "Exotic Butterflies," Johnstone's "British Sea Weeds," Semann's "Flora Vitiensis," Hooker's "Niger Flora," Hooker's "Flora of British India," vol. 1, Hooker's "Flora of Tasmania," Booth's "Carex," (last volume), Hawey's "Flora Capensis," Oliver's "Flora of Tropical Africa," &c.

The number of Societies with which we correspond continues about the same as last year.

On the motion of Mr. A. BROTHERS, seconded by Mr. T. MACKERETH, the Report was unanimously adopted, and ordered to be printed in the Society's Proceedings.

On the motion of Mr. A. W. WATERS, seconded by Dr. C. A. BURGHARDT, it was resolved unanimously :—

That the system of electing Sectional Associates be continued during the ensuing Session.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year :—

President.

JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S.

Vice-Presidents.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.

HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.

OSBORNE REYNOLDS, M.A., F.R.S.

Treasurer.

CHARLES BAILEY, F.L.S.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Other Members of the Council.

REV. WILLIAM GASKELL, M.A.

ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.

WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.

BALFOUR STEWART, LL.D., F.R.S.

CARL SCHORLEMMER, F.R.S.

WILLIAM E. AXON, M.R.S.L., F.S.S.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

CHARLES BAILEY, TREASURER, IN ACCOUNT WITH THE SOCIETY FROM 1ST APRIL, 1877, TO 31ST MARCH, 1878, WITH A COMPARATIVE STATEMENT OF ACCOUNTS FOR THE TWO PRECEDING YEARS.

	1877-8.	1876-7.	1875-6.	
	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Dr.				
1877—April 1. To cash in hand	247 19 9	236 19 2	542 1 1	
1878.—March 30. To Members' Contributions:—				
Arrears, 1875-6, 1 at £2 2s.	2 2 0			
1876-7, 7 at £2 2s.	14 14 0			
New Member's Admission, 1876-7, 1 at £2 2s.	2 2 0			
Half Subscription	1 1 0			
Admission, 1877-8, 2 at £2 2s.	4 4 0			
Subscription	4 4 0			
Old Members, 1877-8, 145 at £2 2s.	304 10 0			
To One compounder (Mr. W. H. Johnson)		332 17 0	344 8 0	
Two Associates' Subs. (for Library)	1 0 0	26 5 0		
Sectional Contributions, 1877-8:—	2 2 0			
Physical and Mathematical Section	2 2 0			
Microscopical, Natural Hist. do.	2 2 0			
Use of Society's Rooms:—	30 0 0			
Geological Society to 31st Mar., 1877	11 10 0			
Scientific Students' Association, to 31st March, 1877	41 10 0	41 10 0	78 5 0	
Sale of Society's Publications	2 10 0	5 6 0	2 6 7	
Natural History Fund—Interest from Owens College to 22nd Feb., 1878	68 16 9	59 6 3	59 10 0	
Capital Fund—Natural Hist. Society's donation	1500 0 0			
Bank Interest, less Bank Postage, to 31st December, 1877	4 11 10	3 17 1	8 16 0	
£2229 14 4	£714 7 6	£1040 10 8		
Cr.				
1878.—March 30. By Charges on Property:—				
Chief Rent	12 15 2			
Insurance against Fire	13 7 6			
Property Tax	2 2 6			
Repairs, Whitewashing, &c.	2 0 2			
By House Expenditure:—				
Coals, Gas, Candles, and Water	18 17 9			
Tea and Coffee at meetings	18 14 6			
House Duty	6 7 6			
Cleaning, Brushes, Sundries	6 2 7			
By Administrative Charges:—				
Wages of Keeper of Rooms	57 4 0			
Postages and Carriage of Parcels	26 17 4			
Attendance on Sections and Societies	15 14 0			
Stationery and Printing Circulars	8 9 3			
By Publishing:—				
Printing Memoirs	74 4 6			
Printing Proceedings	63 15 0			
Wood Engravings	6 3 3			
Editor of Memoirs and Proceedings	50 0 0			
By Library:—				
Binding Books	35 2 6			
Books and Periodicals	11 2 6			
Assistant in Library	11 6 0			
Old Memoirs of the Society	1 12 0			
Geological Record for 1875	0 11 2			
Palaeontographical Society for 1878	1 1 0			
By Natural History Fund:—				
Grant to Microscopical and Natural History Section for Books	60 0 0			
By Balance	1726 4 2	247 19 9	236 19 2	
£2229 14 4	£714 7 6	£1040 10 8		

	1877-8.	1876-7.	1875-6.	
	£ s. d.	£ s. d.	£ s. d.	£ s. d.
1878.—April 1.—To Cash in hand, viz.:—				
Compounders' Fund	103 1 10	125 0 0		
Natural History Fund:—	68 16 9			
Interest Account—Balance 1st April, 1877	171 18 7			
Interest to 1st April, 1878	60 0 0			
Less Grant for Books 20th April, 1877	111 18 7			
Capital Account	1500 0 0			
By Balance against the General Account	1736 18 7	10 14 5	1726 4 2	
£2229 14 4	£714 7 6	£1040 10 8		

April 9th, 1878.—Audited and found correct,

(Signed)
THOS. MACKERETH,
JAMES BOTTOMLEY.

“The Distribution of Ammonia,” by Dr. R. ANGUS SMITH, F.R.S., &c.

If organic matter is everywhere ammonia is everywhere possible, and if that matter is decomposing, ammonia is everywhere. This is the general statement which this paper illustrates. It is now many years since it was observed by me that organic matter could be found on surfaces exposed to exhalations from human beings; but it is not till now that the full significance of the fact has shone on me, and the practical results that may be drawn from it in hygiene and meteorology. These results are the great extension of the idea that ammonia may be an index of decayed matter; the idea itself has been used partly and to a large extent, as illustrated in my “Air and Rain.” The facts now to be given enable us to claim for it a still more important place. The application seems to fit well the conditions already examined, and by this means currents from foul places have been readily found. This does not apply to the substances which may be called germs, whether it be possible to see them or not, because these are not bodies which have passed into the ammoniacal stage although some of them may be passing; those for example which are purely chemical and exert what we may call *idiolytic* action. This word, from ἴδιος, its own (peculiar), and λύσις, decomposition, may serve to mark this peculiar action which was left by Liebig unnamed; he used the vague term invented by Berzelius, namely catalytic. I have elsewhere recognised the two classes of germs, instead of any disputed one, without naming them.

It is now many years since Liebig first surprised me by saying that iron ores and aluminous earths were capable of taking up ammonia, and if they were breathed upon we were able even to smell that substance. He, much about the same time, made numerous experiments in order to find the ammonia of the atmosphere, and to measure its

amount in rain. The result for science was great, and Professor Way continued the enquiry for the Royal Agricultural Society. Dr. Gilbert, F.R.S., amongst his many labours in the department of agricultural science, has made this enquiry into ammonia of rain in still later times, but I shall not at present quote his results, as this paper does not intend to go fully into the subject, but rather to indicate its magnitude and importance. The first paper I ever read to this Society was on the ammonia found in peat—I was unable then to see the extent of the subject.

I shall give parts of the fuller paper without the long tables of results.

Ammonia must ever be one of the most interesting of chemical compounds. It comes from all living organisms, and is equally necessary to build them up. To do this it must be wherever plants or animals grow or decay. As it is volatile, some of it is launched into the air on its escape from combination, and in the air it is always found. As it is soluble in water it is found wherever we find water on the surface of the earth or in the air, and probably in all natural waters, even the deepest and most purified. As a part of the atmosphere it touches all substances and can be found on many; it is in reality universally on the surface of the earth, in the presence of men and animals, perhaps attached more or less to all objects, but especially to all found within human habitations, and we might also add with equal certainty, the habitations of all animals.

If you pick up a stone in a city and wash off the matter on the surface, you will find the water to contain ammonia. If you wash a chair or a table or anything in a room, you will find ammonia in the washing, and if you wash your hands you will find the same; and your paper, your pen, your table cloth, and clothes, all show ammonia, and even the glass cover to an ornament has retained some on its surface. You will find it not to be a permanent part of the glass,

because you require only to wash with pure water once or twice, and you will obtain a washing which contains no ammonia. It is only superficial.

This ammonia on the surface is partly the result of the decomposition of organic matter continually taking place and adhering to everything in dwellings. The presence of organic matter is easily accounted for, but it is less easily detected than ammonia. It is probable that the chief cause of the presence of ammonia on surfaces in houses and near habitations is the direct decomposition of organic matter on the spot. If so, its presence, being more readily observed than organic matter itself, may be taken as a test and the amount will be a measure of impurity. A room that has a smell indicating recent residence will, in a certain time, have its objects covered with organic matter, and this will be indicated by ammonia on the surface of objects. After some preliminary trials, seeing this remarkable constancy of comparative results, and the beautiful gradations of amount, it occurred to me that the same substance must be found on all objects around us whether in a town or not; I therefore went a mile from the outskirts of Manchester and examined the objects on the way. Stones that not twenty hours before had been washed by rain, showed ammonia. It is true that the rain of Manchester contains it also, but considering that only a thin layer would be evaporated from these stones, it was remarkable that they indicated the existence of any. The surface of wood was examined—palings, railings, branches of trees, grass (not very green at the time), all showed ammonia in no very small quantities. It seemed as if the whole visible surface around had ammonia. I went into the house and examined the surfaces in rooms empty and inhabited, tables, chairs, ornaments, plates, glasses, and drawing-room ornaments. A (parian) porcelain statuette, under a glass, showed some ammonia, a candlestick of the same material (but un-

covered) showed much more; the back of a chair showed ammonia, when rubbed with a common duster, very little. It seemed clear that ammonia stuck to everything.

If, then, ammonia were everywhere, the conclusion seemed to be that it was not at all necessary to do as I had been doing, namely, wash the air so laboriously; it would be quite sufficient to suspend a piece of glass and allow the ammonia to settle upon it. For this purpose small flasks were hung in various parts of the laboratory, and they were examined daily. The flasks would hold about 6 ounces of liquid, but they were empty, and the outer surface was washed with pure water by means of a spray bottle; it was done rapidly, and not above 20 cc., two-thirds of an ounce, of water was used. This was tested for ammonia at once with the Nessler solution. The second washing produced no appearance of ammonia, done immediately. Ammonia could be observed after an hour and a half's exposure at any rate, but I do not know the shortest period. The results of the washings were as follows. They are the average of 34 experiments for some, and 17 for others, in all 238 experiments.

	Height from floor.		Ammonia. Mgms.		Height from floor.		Ammonia. Mgms.
	Ft.	In.			Ft.	In.	
Front laboratory	7	3	... ·013	...	4	2	... ·019
2nd landing.....	6	0	... ·032				
Balance room	5	1	.. ·015	...	0	8	... ·009
1st landing	4	10	... ·007				
Back laboratory.....	4	5	... ·010	...	0	6	... ·010
Entrance lobby	6	5	... ·007				
Office.....	4	7	... ·003				
Back yard	4	8	... ·036	...	0	7	... ·042
Back closet	2	3	... ·105				
Midden.....			·572				

The first three belonging to the working laboratory are not very regular, as we might suppose, but they never rise very high, nor do they sink to the lowest. The rest, except the second, keep a remarkable similarity, and the differences are very great. In the second there is a disturbance caused

by sweeping the floors. On the other days it was requested that everything should be kept still. This of course brings in a practical difficulty and limits the use of the test to cases where care can be used and thoughtful observation, since there are many ways by which dust may be made to interfere, even although the act of sweeping should not take place. The house experiments gave similar gradations.

The result seems to be that a piece of glass, of a definite size, hung up in any place, will receive deposits of ammonia, or substances containing ammonia, in a short time, and by washing the ammonia off with pure water, and testing it with a Nessler solution, it may be seen whether there is too much or not. It is the simplest test for ammonia yet found. Its discoverer deserves great thanks. It must not be forgotten that we may have ammonia in very different conditions; it may be pure, or it may be connected with organic matter. This mode of enquiry is better suited as a negative test to show that ammonia is absent, than to show what is present. When ammonia is present there may be decomposing matter, when absent there is not. I am hoping to make this a ready popular test for air—a test for sewer gases*, for over-crowding, for cleanliness of habitations, and even of furniture—as well as for smoke and all the sources of ammonia. Of course it must be used with consideration, and the conclusions must not be drawn by an ignorant person.

How far it may be used as a test of climate is a matter to be considered also.

After this I made another series of trials with air, Nesslerising the washings at once and not after laborious distillings, as in former cases; the results are very valuable, showing that we obtain comparative quantities equally in this way.

The amount of ammonia obtained in this ready way does not give exactly the same results as the more laborious methods which I have used, but it may be taken as the

* For sewage also to a larger extent than it has yet been used.

most convenient. It must be observed that the amount rises exactly where you might expect more organic matter to exist. The lowest is from Prince's-road, outside the town and almost a half a mile from the extreme of the Manchester houses. The next is obtained from an empty yard behind my laboratory, but it is still pure because there was wind and rain, and any one who observes how unusually pleasant it is to breathe the air even of a smoky town during rain and wind will not be surprised. I have not yet, however, had the purest air. I shall require to make a campaign on the moors, hills, and seas before I can give numbers for this. I have not even obtained the best given on land at a distance from manufactures. All this will be done in time.*

In my office the amount is larger than outside, but the air is not so bad as it is in front and not so good as sometimes in the front where it is open. From the back of the laboratory during fog the ammonia was much higher, but during one day it was excessive, and a special examination of it was made in several streets. The highest amount was obtained at the front of the Cathedral, about mid-day, on the 8th of February, 1878, when the amount was 1.25, or $14\frac{1}{2}$ times more than it had been found in Prince's-road, showing a considerable range.

	Mgms. of Ammonia per cubic metre of air.
Prince's-road	·086
Open yard during rain.....	·119 and ·102
Front of laboratory	·167 ordinary.
Office.....	·167
Front and back during fog.....	·476
Close shut-up room	·413
Closet outside.....	·800 to ·900
Densest part of fog	1.25

* Since the paper was read I examined trees and stones at Skelmorlie and Wemyss Bay, finding very little ammonia.

“On the Origin of Elementary Substances, and on some New Relations of their Atomic Weights,” by HENRY WILDE, Esq.

The hypothesis, that the solar system, as at present constituted, was formed by the successive condensations of a gaseous substance rotating under the influence of a central force, has so much evidence in its favour, that it may be affirmed to equal the best of that obtained from the geological record of the changes, which, in past times, have taken place on the surface of the terrestrial globe. That this gaseous or primordial substance consisted of a chaotic mixture of the 65 elements known to chemists is a notion too absurd to be entertained by any one possessing the faculty of philosophic thinking, as the regular gradation of properties observable in certain groups of elements, clearly shows that elementary species are not eternal, but have a history which it is the proper object of physical science to unfold.

One of the principal facts which to my mind establishes the nebular theory of the formation of planetary systems on a firm basis, is Bode's empirical law of the distances of the members of the solar system from each other, and from the central body, as in this law is comprehended the idea of nebular condensation in definite proportions. Now, if elementary species were created from a homogeneous substance, possessing a capacity for change in definite proportions, it is probable that the greater number of elements would be formed during, or after, the transition of the nebular matter from the annular to the spheroidal form. Moreover, as great cosmic transitions are not made *per saltum*, it might be expected that some modification of the law of nebular condensation into planetary systems, as ex-

hibited in Bode's law, would be found on the further condensation of the primitive matter into elementary species.

That relations such as I have indicated exist between the nebular and elementary condensations, represented by the planetary distances on the one hand, with the atomic weights of well defined groups of elementary substances on the other, will be evident on comparing the numbers in the following tables :—

I.

$0 \cdot 0 \cdot 4 =$	4 Mercury.
$1 \times 3 + 4 =$	7 Venus.
$2 \times 3 + 4 =$	10 Earth.
$4 \times 3 + 4 =$	16 Mars.
$8 \times 3 + 4 =$	28 Ceres, Pallas, &c.
$16 \times 3 + 4 =$	52 Jupiter.
$32 \times 3 + 4 =$	100 Saturn.
$64 \times 3 + 4 =$	196 Uranus.

In the above table the numbers expressing the relative distances of the planetary bodies from the sun, and from each other, are obtained by multiplying successively the difference (3) between the distance of the first and second members of the system, by a geometric series, and adding to the products, the constant distance (4) of the first member from the sun. Now, if the atomic weight of the second member of the alkaline and silver group of metals ($\text{Na} = 23$) be multiplied successively by an arithmetical series, then will the products, minus the atomic weight of the first member ($\text{Li} = 7$), be the atomic weights of all the elements belonging to that group.

II.

$0 \cdot 0 \cdot 7 =$	$\text{Li} =$	7
$1 \times 23 \cdot 0 =$	$\text{Na} =$	23
$2 \times 23 - 7 =$	$\text{Ka} =$	39
$3 \times 23 - 7 =$	$\text{Cu} =$	62
$4 \times 23 - 7 =$	$\text{Rb} =$	85
$5 \times 23 - 7 =$	$\text{Ag} =$	108
$6 \times 23 - 7 =$	$\text{Cs} =$	131
$7 \times 23 - 7 =$	-- =	154
$8 \times 23 - 7 =$	— =	177
$9 \times 23 - 7 =$	$\text{Hg} =$	200

Again, by multiplying in like manner the atomic weight of the second member of the alkaline earth and cadmium group of metals, the products, minus the atomic weight of the first member ($G_1=8$), are the atomic weights of all the elements of this group.

III.

$$\begin{aligned}
 0 \cdot 0 \cdot 8 &= G_1 = 8 \\
 1 \times 24 \cdot 0 &= Mg = 24 \\
 2 \times 24 - 8 &= Ca = 40 \\
 3 \times 24 - 8 &= Zn = 64 \\
 4 \times 24 - 8 &= Sr = 88 \\
 5 \times 24 - 8 &= Cd = 112 \\
 6 \times 24 - 8 &= Ba = 136 \\
 7 \times 24 - 8 &= \text{---} = 160 \\
 8 \times 24 - 8 &= \text{---} = 184 \\
 9 \times 24 - 8 &= Pb = 208
 \end{aligned}$$

The further relations observable between interplanetary voids and atomic condensations of the natural groups of elements in tables II. III., are as follows:—

1. The regular geometric series of the planetary distances commences at the second member of the system, and the regular arithmetical series of atomic weights commences at the second and corresponding member of each group.

2. As the atomic weight of the second element in each group is half the sum of the atomic weights of the first and third elements, so is the distance of the second member of the solar system an arithmetical mean, or half the sum of the distances of the first and third members.

3. The atomic weight of the fourth member in each group of elements is equal to the sum of the atomic weights of the second and the third; and the distance of the fourth member of the solar system is also equal, within a unit, to the sum of the distances of the second and third members.

4. As the smallest planetary distance is a constant function of the distances of the outer planetary bodies, so is the smallest atomic weight in each group a similar function of all the higher members of the series to which it belongs. It will also be observed that the plus and minus signs of

these constants are correlated respectively with the interplanetary spaces, and the elementary condensations.

5. Each of the atomic weights, after the third in the groups, is an arithmetical mean of any pair of atomic weights at the same distance above and below it; and the distance of each member of the solar system (minus the constant 4) is a mean proportional of the distances of any two members, externally and internally to it, from the central body.

6. The geometric ratio of the planetary distances from each other terminates at the two members nearest the central body, and approaches to an arithmetical one; and a similar departure is also noticeable from the regular arithmetical series of the atomic weights of the first two members of the groups, which renders the third less than an arithmetical mean of the atomic weights of the second and fourth members.

While most of the atomic weights in tables II. III., excluding fractions, agree with those generally received by chemists, the remainder, except Cæsium = 133, do not vary more than a unit from the classical numbers. When it is considered that some of these numbers have been obtained by doubling the fractions of the old atomic weights, and that slight differences in the determinations may arise from the latent affinity which some elements have for minute quantities of another, the numbers in the tables are remarkably near to those determined by experiment—more so, in fact, than is Bode's law to the actual distances of the planets from the sun.

It will be observed that there are gaps to be occupied by two elements in the first group, with atomic weights 154 and 177, and by their homologues of position in the second group, with atomic weights 160 and 184, which remain to be discovered.

The numerical relations subsisting among the atomic weights in tables II. III., and their resemblance to the homo-

logous series in organic chemistry, afford further evidence in support of the theory that elementary species are formed by the successive condensations of a primordial substance of small specific gravity and low atomic weight. The physical and chemical properties of hydrogen, especially its low atomicity and its exact multiple relations with many elementary substances, long since suggested to Prout that this element might be the ponderable base of all the others.* Prout's hypothesis has not, however, made much progress, as chemical knowledge was not sufficiently advanced in his time to enable the intermediate steps to be perceived by which elements of high atomicity could be built up from hydrogen; and, besides this, the hypothesis afforded no explanation of the widely diverging properties of elements having nearly the same atomic weights. If, however, it be assumed that a particle of hydrogen combines successively with one, two, three, or more of its own particles, to form the molecules H_2 , H_3 , H_4 , H_5 , H_6 , H_7 , and that each of these molecules forms the type of a group of elements under it, the intermediate steps between the low atomic weight of hydrogen, and the high atomic weights of other elements are perceived, and the different properties of elements of approximately equal atomic weights admit of a rational explanation.

Although it is herein assumed that hydrogen is the ponderable base of all elementary species, it is probable that this element itself, as further maintained by Prout, may have been evolved from an ethereal substance of much greater tenuity.† Further knowledge of the outer regions of the solar atmosphere, and of the zodiacal light, may possibly indicate the steps by which hydrogen was formed.

I would also observe that the term "molecule" is here used only in the sense of a larger or denser particle of

* *Annals of Philosophy*, vol. vi., 330, 1815; vol. vii., 113, 1816.

† Prout's *Chemistry and Meteorology*, 8th Bridgewater Treatise, p. 130.

matter, and does not imply the idea of a composite aggregation of the separate particles, each preserving its distinctive character after the molecule is formed, any more than rain drops preserve their distinctive character after falling into the ocean. It appears to me much more in accordance with the truth of nature to suppose that the smallest conceivable particle of a chemical substance or compound, has the same physical properties absolutely as the mass. If it be objected that such a union of particles would have relations of infinity, and is therefore inconceivable, it may be answered that the central particles of a rotating body have mathematical and physical relations of a similar kind, and as the instrument of thought is incapable of forming a distinct conception of the magnitude of the infinitesimals involved in a centre of rotation, still less is it capable of comprehending the mode of union of the unknowable essences on which the physical qualities of chemical substances, after combination, depend. Philosophical chemists, I apprehend, will hereafter be able to refer the origin of the theory of the composite structure of matter, after chemical union, to the influence of ideas derived principally from the mechanical mixtures employed in pharmacy, and in the culinary art.

In the present hypothesis, it is assumed that a mass of hydrogen, of a curvilinear form, acquired a motion of rotation about a central point, which caused it to take a spiral or convoluted form. As each successive spiral or convolution was formed, the particles of hydrogen combined with themselves as far as the septenary combination, to constitute the type of each group of elements—the number of types or groups being equal to the number of convolutions of the rotating gas. According to this view, the elementary groups may be represented as forms of Hn , $H2n$, $H3n$, $H4n$, $H5n$, $H6n$, $H7n$; the internal convolutions forming the highest type $H7n$, and the outer convolution the type Hn .

That on a further condensation of the elementary matter, a transition from the spiral to the annular form occurred, during, or after which, the group or species under each type was generated in concentric zones, and in the order of their atomic weights, until the highest member of each species was formed. That as the elementary vapours began to condense, or assume the liquid form, their regular stratification would be disturbed by eruptions of the imprisoned vapours from the interior of the rotating mass. This disturbance would be further augmented by the subsequent combination of the negative with the positive elements, and also by the variable solubility of their newly formed compounds; so that the evidence of such stratification of the elementary vapours as I have indicated, must necessarily be more fragmentary than that of the geological record. The constant association in nature, however, of several elements belonging to the same group, a remarkable example of which is the presence of lithium, potassium, rubidium, and cæsium in a single mineral, *lepidolite*, appears to confirm this view of the primitive arrangement of elementary vapours.

In the annexed table are arranged all the known elements in natural groups, wherein gaps appear as in tables II. and III., which indicate the existence of missing elements. The atomic weights of other elements which have not been sufficiently investigated are also determined.

If the theory which I have enunciated of the evolution of elementary substances from hydrogen in definite proportions be correct, the numbers representing the atomic weights, also represent the number of particles of hydrogen from which the elements were formed. Where these numbers do not coincide exactly, as in the case of $\text{Cu}=62$, and its homologue of position, $\text{Zn}=64$, which are each a unit less than the classical numbers, it is not to be supposed that these discrepancies are due to errors of experiment, but to some unknown cause which prevents their true atomicity from being ascertained.

Although the ideas of chemists on the classification and quantivalence of elements have greatly changed during recent years, there is no question that the alkaline metals, lithium, sodium, potassium, rubidium, and cæsium, belong to the group which I have classified under Hn . Chemists are also agreed that silver, notwithstanding the great divergence of some of its characteristics from those of the alkaline metals, also belongs to the same group. Now some of the physical and chemical properties of copper and mercury are more nearly allied to those of silver than to metals of other groups, and recent investigations have shown that silver may, like copper, be regarded as bivalent, since many of its compounds can be represented by formulæ exactly analogous to those of cuprous compounds with which they are isomorphous.* The position of Hg, Ag, and Cu, as alternate members of the series Hn , indicate their relationship with sodium, and are thereby brought into still closer connexion with Li, K, Rb, and Cs. That a relationship exists between sodium and silver, by the isomorphism of their anhydrous sulphates, and in other ways, has already been pointed out by Odling. The greater specific gravity of sodium, while possessing a lower atomic weight than potassium; its passivity in the liquid state to the action of chlorine;† and its inferior volatility and oxidability to K, confirm the relationship of Na to the heavy metals of the series.

From what cause elements possessing physical properties so widely different, should be associated alternately in regular order in the same series, can only, in the present state of knowledge, be a subject of speculation; but, if the views which I have enunciated on the formation of the types Hn — $H7n$ be correct, it may be conceived that after the transition of the cosmical vapours from the spiral to the annular form, the gaseous material of each pair of members

* Quantivalence of Silver,—Wislicenus, Watts, Dic. Chem., 2nd Suppl., 1088.

† Watts, Dic. Chem., Suppl., 1030.

might rotate in concentric zones, separate from each other by an interval of space. It may be further conceived, that the rotating zones of elementary matter were of sufficient thickness to cause a difference of density between their upper and lower regions. That the zones were in a highly electrical condition, and that their mutual influence on each other, through the annular space between them, would induce opposite electrical conditions in their external and internal regions; all the inner and denser regions of the zones being in a negative, and the outer or rarer regions in a positive electrical condition. Each zone would then be in a condition to form an electro-positive, and an electro-negative element, which, on a subsequent condensation, would separate and form two zones of elements having dissimilar properties alternating with the other members of the same series.

Just as silver and sodium are the connecting links between Hg and Cu, and the alkaline metals Li, K, Rb, and Cs; so do cadmium and magnesium connect lead and zinc with the alkaline earth metals glucinum, calcium, strontium, and barium, which I have classified as forms of $H2n$. The classification of glucinum with the alkaline earth metals has only recently been made, but chemists are not yet agreed upon the atomic weight of this element, as it has been fixed at $G1=7$ (Awdejew) and $G1=9\cdot4$ (Reynolds). It may, however, be suspected from the anomalously high specific gravity assigned to glucinum (2.10) as compared with that of magnesium (sp. g. 1.74), and with their homologues of position Li. sp. g. 0.59, and Na. sp. g. 0.97, that this element has not yet been isolated in a state of purity.* By assigning to glucinum the atomic weight $G1=8$, it enters as a multiple

* Since this paper was written, MM. Nilson and Petterson have communicated to the French Academy the results of their researches on the physical properties of glucinum, and have found for the metal a density equal to 1.64, which, although still too high, the theoretical density being about 1.3, is less than that of magnesium, and, consequently, stands in the same order of density as lithium and sodium.—*Comptes Rendus*, April 1st, 1877, p. 825.

into all the members of the series $H2n$, and may be regarded as the product of the first, second, or third powers of $H2$.

While the property of quantivalence would appear to be correlated with the number of hydrogen particles in the typical molecules from which the elements were evolved and is a valuable aid in the classification of elementary species, this property in the present state of knowledge is not in many cases sufficient, of itself, to indicate the group to which an element belongs. This will be seen from the recognised bivalency of copper and mercury, and by the doubtful quantivalence of silver, and by analogy of sodium, all of which belong to the series Hn . That tetratomic lead=208, is a member of the group $H2n$, is shown by the isomorphism of its oxide, carbonate, and sulphate, with the oxides, carbonates, and sulphates of barium, strontium, and calcium, besides which there is no other place vacant in the system of elements where one with the atomic weight and physical properties of lead would fit.

Were it not for the analogous physical properties, and the numerical relations subsisting among the elements grouped as forms of $H3n$, their classification from the property of quantivalence alone would have hardly been possible. There can, however, be little doubt that aluminum, yttrium, erbium, and thorium are rightly classified together, and that indium and thallium are true analogues of each other. As considerable interest attaches to this group at the present time, on account of the recent additions which have been made to it by the aid of spectral analysis, I here show the atomicities of its members in a separate table, calculated on the same principle as those in tables II. III.

IV.

0 . 0 . 12 = C =	12
1 × 27 . 0 = Al =	27
2 × 27 - 12 = — =	42
3 × 27 - 12 = — =	69
4 × 27 - 12 = — =	96
5 × 27 - 12 = Yt =	123
6 × 27 - 12 = In =	150
7 × 27 - 12 = E =	177
8 × 27 - 12 = Tl =	204
9 × 27 - 12 = Th =	231

It will be observed that there are three elements missing in this group, the atomic weights of which can be predicted in like manner with those of the missing elements in the preceding groups. The table also affords the means of correcting and determining the atomicities of elements of the series which, from their rarity, have not been sufficiently investigated. It will be further observed that, besides the similar numerical relations of the members of this group with those shown in tables II. III., the atomic weights are all multiples of 3, and are classified accordingly as forms of $H3n$.

The spectral reactions of this series of elements are remarkable from the oxides of carbon, and erbium, giving a spectrum of lines at low temperatures, and by the simplicity of the spectral lines of indium and thallium in the more refrangible parts of the spectrum. The atomic weights of C, Al, Tl, and Th, are identical with those generally received, and afford presumptive evidence that the atomic weights of the intermediate members are equally correct. It will, however, be observed that the atomic weights of yttrium and indium are double the accepted numbers ($Y = 61.7$, $In = 75.6$); but in regard to the latter element, it has not yet been definitely agreed which multiple of 37.6, the original determination, shall be the classical one, as the atomicity has been fixed by different chemists at 75.6, 113, and 150, the number assigned to it in the table. The relations which the double atomic weights of In, and Yt, have to each other, and with their homologues of position Cs, Ba, and Ag, Cd, in tables II. III., render it highly probable that the atomic weights of Y and In, in the table are correct. For similar reasons it is probable that the atomic weight of erbium will be found to be 177. It is only very recently that any investigations of the atomic weight of this rare element have been made, from the difficulty attending its isolation from yttrium, with which it is found associated in nature. According to some chemists, the atomic weight of

erbium is 112·6, which, in relation to 177, is nearly in the ratio of 5 to 8. The more recent researches of M. Cleve, on the quantivalence of this element have, however, raised its atomic weight to 170·55* which, considering the wide difference between it and the previous determination, is a near approximation to the number in the table. The researches of the same chemist have also raised the atomic weight of yttrium from 61·7, the accepted determination, to 89·5, or three-fourths the calculated value. Now the history of chemical science abundantly shows that it is only after long and repeated investigation that the highest quantivalence of an element can be ascertained, and the result of M. Cleve's researches is a further confirmation of the correctness of the atomic weights of yttrium and erbium given in the table.

By comparing the electro-positive members of the series Hn with those of $H2n$, it will be seen that a complete parallelism exists between them; the light alkaline, and alkaline earth metals alternating with the heavy members in homologous positions in both series. Odling has already indicated that this is the natural order of the dissimilar members of the zinco-calcic group of elements,† and similar alternations in other natural groups have been recognised in the arrangement of elements proposed by Mr. Newlands‡ and Mendeleeff.||

Just as $Cu = 62$, $Ag = 108$, and $x = 154$, alternate with $Rb = 85$, $Cs = 131$, and $x = 177$, in the series Hn ; and $Zn = 64$, $Cd = 112$, and $x = 160$, alternate with $Sr = 88$, $Ba = 136$, and $x = 184$; so in the series $H3n$, do $x = 69$, $Yt = 123$, and $Eb = 177$, alternate with $x = 96$, $In = 150$, and $Tl = 204$.

* Bull, Société Chimique, Paris, tome xxi., 344, 1874.

† Watts, Dic. Chem., 1865, vol. iii., 963—Classification of Metals.

‡ Chem. News, vol. xii., 83; vol. xiii., 113.

|| Die periodische Gesetzmäßigkeit der chemischen elemente—Ann. Chem. Pharm.; Suppl. Band, viii., 133—229, 1872; Phil. Mag. 5th S., vol. i., 543.

Again, just as K, Rb, Cs, and $x=154$, are analogues of each other in the series H_n , so are $x=42$, $x=96$, In, and Tl, analogues of each other in the series H_{3n} , and are in homologous positions with the alkaline, and alkaline earth metals in the series H_n , and H_{2n} . The specific gravities of analogous members of these two series, except glucinum, which is anomalous, increase in the order of their atomic weight, and so far as the specific gravities of the members of the series H_{3n} , have been ascertained, they follow the same order. Now M. Lecoq de Boisbaudran has shown that the new metal which he has discovered, and named gallium,* is, from its spectral reactions and other properties, the analogue of indium and thallium. The position of the new metal in the series H_{3n} , should therefore be either $- = 42$, homologous with Ca, and K, or $- = 96$, homologous with Sr, and Rb. In comparing the alkaline metals of the series H_n , the specific gravity of sodium (0.97) as will be seen, is greater than that of potassium (0.86), although Na has a less atomic weight; and the same inversion of specific gravities in relation to atomic weights is observable in their homologues of position Mg., sp. g. 1.74, and Ca, sp. g. 1.58, in the series H_{2n} . It may therefore be assumed that the missing member $x=42$, H_{3n} , would have a less specific gravity than Al, sp. g. 2.56; probably 2.5. Now, the specific gravity of gallium as determined by M. Lecoq de Boisbaudran is 5.9;† and its analogues indium and thallium have specific gravities of 7.42, and 11.9 respectively, consequently $x=42$, is not gallium. If gallium were $x=69$ it would be the analogue of Y, E, and Th, and homologous in position with Zn and Cu, whereas it has been shown to be the analogue of In, and Tl, and homologous in position with Sr and Rb. There is then no other place for a metal having the physical properties of gallium but the one assigned to it in the series H_{3n} ,

* Comptes Rendus, tome lxxxii., 403, 1000, 1865.

† Phil. Mag. 5th S., vol. ii., 398.

with the atomic weight = 96, and forming a triad with indium and thallium. If, however, the experimental determination of the atomicity of gallium pass through the same stages as the atomicities of indium, yttrium, and other members of the series, its atomic weight will be represented by the sub-multiple and proportional numbers 48 and 72.*

Just as silver and copper are analogues of each other, and are frequently associated in nature; and, just as their homologues, cadmium and zinc are analogues, and are also found together; so is yttrium the analogue of $x=69$, and will be found associated with it in nature. Now, if $x=69$ be not the terbium of Mosander and Delafontaine, and the researches of Bahr and Bunsen render the existence of this element doubtful, it is probable that $x=69$ is cerium, as this element and yttrium are nearly always found associated in the mineral species cerite and yttrocerite. Moreover, it will be observed that $x=69$ is just 1.5, or 0.75 the atomic weight of cerium, according as it is regarded as 46 or 92. Mendeleeff and other chemists have already proposed 138 as the atomic weight of cerium,† which is double that of $x=69$. MM. Hildebrand and Norton have recently obtained cerium, lanthanum, and didymium in a massive state, and have thereby been able to investigate some of the physical properties of these rare metals.‡ According to these experimenters the specific gravities of Ce, La, and Di, range between 6 and 6.7. Bearing in mind that elements of approximately the same atomic weights and specific gravities generally belong to different series, and that the

* From a calcination of the gallo-ammoniacal alum, M. Lecoq de Boisbaudran has recently found for gallium the equivalent 70.03, and from a calcination of the nitrate, 69.6—*Comptes Rendus*, April 15th, 1878. The researches of M. Berthelot on the specific heat of gallium indicate, however, a higher equivalent for the metal than 70.03, as the atomic heat calculated from this determination (5.52 solid) is lower than that of any other metal except silicium.—*Ibid*, April 15th, 1878.

† Ann. Chem. Pharm. Suppl. viii., 185—190.

‡ Chem. Soc. Journal, 1876, vol. ii., 276.

specific gravities of analogous members in each series increase in the order of their atomic weights, it would appear that cerium does not belong to the same series as lanthanum and didymium. Moreover, considering the important position which $x=69$ occupies in relation to its analogues Al, Y, and the position which these three elements occupy in relation to their homologues Mg, Zn, Cd, and Na, Cu, and Ag, it may be doubted if $x=69$ should, up to the present time, have remained undiscovered, especially as all its analogues of the series Th, E, Y, and Al, are well known. If, therefore, $x=69$ be cerium, the only element missing in the series $H3n$ is $x=42$, the analogue of Ga, In, and Tl. As these elements have been discovered by spectrum analysis, it is probable that $x=42$ will also be found by the same means. It may however be observed, that the characteristic lines of the alkaline metals in the series Hn , and of their homologues $H3n$ advance in the blue or violet end of the spectrum, towards the more refrangible parts in the inverse order of their atomic weights. The spectral lines of $x=42$ must therefore be sought for in the violet or ultra violet part of the spectrum. The high refrangibility of the lines which the missing element will have, may be the reason why it has hitherto escaped detection, as from the wide distribution in nature of its homologues of position Ca, and K, in relation to their respective analogues Sr, and Rb; $x=42$ ought to be more abundant in nature than gallium.

From the physical and chemical relations which subsist among the halogens Fl, Cl, Br, I, and the alkaline metals Li, Na, K, Rb, Cs, chemists have already justly considered these elements as positive and negative analogues of each other and of hydrogen. In accordance with this view, I have classified the halogens as negative forms of the series Hn . By assigning to these elements the positions shown in the table, it will be seen that besides the triad of atomic weights formed by Cl, Br, and I, there is a common differ-

ence of 4 between the atomic weights of the halogens and their positive homologues of position Na, K, Rb, and Cs. Now if the groups of oxygen elements O, S, Se, Te, be considered as negative forms of H_{2n} , homologous in character and position with the negative forms of H_n , it will be seen that besides the triad of atomic weight formed by S, Se, and Te, there is a common difference of 8 between them and their positive homologues Mg, Ca, Sr, and Ba; or double the common difference between the positive and negative members of the series H_n . The oxygen elements are multiples of 2, 4, 8, and 16, and may accordingly be considered as products of the first, second, third or fourth power of H_{2n} . Whichever view be taken of the formation of the first negative member of the series H_{2n} , it is probable that both fluorine and oxygen were not formed direct from H_n , and H_{2n} , but from members homologous in position with Li, and Gl, but which have become extinct by absorption into F and O.

Another numerical relation subsisting among the halogens which it may be of interest to point out is, that the difference of a unit in their atomic weights will make them multiples of 3 and 9, and these numbers, commencing with Cl = 36, are all respectively three times the atomic weights of the first three members of the series H_{3n} . These relations would indicate that the halogens, usually regarded as monatomic, are also built up in multiple proportions, and may also throw some light on the variable quantivalence which Wanklyn and other chemists have shown the alkaline metals and halogens to possess.

The recent researches of chemists leave no doubt that all the elements which I have classified as forms of H_{5n} , except boron, belong to the same group. Now, boron bears a greater resemblance to phosphorus in its combinations and occurrence in nature than it does to other elements, and whether the first three members of the series be considered

as forms of $H5n$, or $H5n + 1$, they form a triad as well defined as their homologues of position in $H3n$, $H2n$ and Hn . Triads are also formed by antimony, arsenic, and phosphorus:—bismuth, antimony, and phosphorus:—tantalum, niobium, and boron:— $x = 140$, $As = 75$ and $B = 10$:— $x = 140$, $Nb = 95$, and $V = 50$. The atomic weights of boron, phosphorus, and vanadium have been so carefully determined by chemists, as to preclude any doubt of their being represented by $H5n + 1$, rather than $H5n$; but the fact that arsenic, antimony, and bismuth are better represented by the formula $H5n$, and that Cu, and Zn, in the series Hn , and $H2n$, exhibit the same constant minus difference from the classical atomic numbers as B, P, and V, are further indications of some unknown property of the elements which conceals their exact multiple relations from view. If the discovery of two new elements of this group by Hermann,* to which this chemist has given the names of neptunium, and illmenium be confirmed, the former element will have an atomic weight of 140, and the latter element an atomic weight of 165, as shown in the table.

Although the numerical relations of the members of the series $H5n$ are very interesting, yet, it will be seen that the ratios are not so simple as those of the series Hn , $H2n$, $H3n$, as multiples of the second member, minus the first, do not give the atomic weights of the other members of the series.

The series $H4n$, besides being incomplete by reason of the absence of several of its members, the atomicity of lanthanum and didymium is not yet agreed upon by chemists. There can, however, be no question as to the position of titanium as the third member of this series, as there is no other place vacant where an element with an atomic weight of 48 would fit, while the isomorphism of rutile with cassiterite and zirconia indicate the relation of tin and zirconium with the same series.

* *Nature*, April 12th, 1877. *H. Kolbe's Journal für praktische chemie*, Feb. 1877, p. 105—150.

The classification of uranium presents some difficulty on account of the fewness of its analogies with other elements, but there can be little doubt that the atomic weight assigned to $U = 120$, until recently, is much too small; as there are no elements with atomic weights so low, correlated with specific gravities so high as that of $U = 18.3$. From a study of the chemical combinations of this element, Mendeleef has assigned to it the atomic weight of 240,* or double the number formerly received, and which number I have adopted. The admission of this high atomic weight, however, separates uranium from chromium, molybdenum and tungsten with which it has been classified, as there are no elements of approximately the same high specific gravities as tungsten = 18.26, and uranium = 18.3, correlated with so great a difference of atomic weights as $U = 240$, and $W = 184$. From the fact that the highest places in all the series, except that in $H4n$, are filled up with their highest members, and that uranium is generally found in combination with the mineral species *yttrotantalite*, *fergusonite*, *polykrase*, *pyrochlore*, *pyrrhite*, containing elements of the series $H3n$ on the one side, and in combination with minerals containing elements of the series $H5n$ on the other, I have classified uranium as the highest form of $H4n$. The two lower forms of $H4n$, as will be seen from the table, are missing; but, assuming that titanium is the highest member in a triad with the missing elements, the atomic weights of the latter are 16 and 32, isomeric with oxygen and sulphur. It may, however, be surmised that no elements now exist to fill the gaps in the series, as they may have become extinct by absorption into titanium and its analogues, or by transformation into the negative forms of $H2n$.

The elements which I have classified as forms of $H6n$ are only three in number, and the atomic weight of chromium = 52.2 establishes its position as the third member of the

* Ann. Chem. Pharm. Suppl., viii., 178—184.

series, and there is no other place for an element with the chemical and physical properties of chromium vacant in the table. For like reasons the positions in the series of molybdenum and tungsten (the analogues of chromium) are also determined. By assigning to chromium the constitution $9 H6$, it forms a triad with the missing elements $x=36$, and $x=18$, which are within a unit, the atomic weights of fluorine and chlorine.

In the arrangement of the elements which I have classified as $H7n$, little assistance is derived from known analogies, when nitrogen and silicium are admitted in the same series with the iron and platinum groups of metals; yet, it might be expected that elements so abundant, and so widely diffused in nature as nitrogen, silicium, and iron, would occupy important positions in any rational classification of elementary species. We have seen that the first three places in the preceding series Hn , $H2n$, $H3n$, $H5n$, are all occupied by elements with atomic weights which exclude nitrogen, silicium, and iron, while the latter element is excluded from the series $H4n$, and $H6n$, by chromium and titanium. The atomic weight of N, Si, and Fe, besides being whole numbers, are exact multiples of 7. N, and Si, are, consequently, excluded from the vacant homologous positions in the series $H4n$, $H6n$.

Since the investigation of the properties of silicium by Berzelius, who regarded silicic acid as a trioxide, much discussion has arisen as to whether the atomic weight of silicium be 21 or 28; or the formula for its oxide SiO_3 , or SiO_2 . Chemists are now generally agreed upon the latter formula for silicic acid, and have accordingly classified silicium with titanium, as the oxide SiO_2 , agrees with titanitic acid TiO_2 . Now, if silicium were the true analogue of titanium, the oxides of these elements should be isomorphous, whereas the crystalline form of quartz is hexagonal, while rutile, anatase, brookite, zirconia and tinstone (similar

oxides of members of the series $H4n$), are tetragonal; consequently, silicium does not belong to the series $H4n$.

By assigning to silicium the atomic weight 35, it forms with nitrogen and iron a triad similar to the first three members of Hn , $H2n$, $H3n$, $H5n$. The position of $Si=35$ as the second member of the series $H7n$, not only throws new light on the disputed atomicity of this element, but also explains the anomalous atomic heat which has been assigned to it.

Through the classical researches of Regnault the specific heat of silicium was found to be 0.176.* The determination was made with specimens of the metal of considerable size, and in a state of compactness and purity, to receive a polish which formed a perfect mirror. The above number multiplied by 28, the highest atomic weight assigned to Si, gives the product 4.93, while the law of Dulong and Petit, requires the value 6.25.

In discussing the cause of the anomalous atomic heat of silicium, Regnault pointed out that in order that it might enter into the law of the specific heat of other elements, it would be necessary to write the formula of silicic acid Si_2O_5 ; it would then resemble that of nitric, phosphoric, and arsenic acids. The atomic weight of silicium would then be 35, and the product of this number and the specific heat would be nearly 6.25, which agrees with the analogous products which other simple bodies give. By assigning to silicium a higher atomic weight and a polybasic character like that of phosphorus or nitrogen, Regnault remarked that it is easy to explain the existence of the great number of silicates which nature presents in well defined and beautiful crystals, and to understand the existence of the natural hydro-silicates.

Whichever view chemists may ultimately adopt in regard to the constitution of silicic acid, or whether its atomic

* *Annales de Chimie et de Physique*, tome lxxiii., 24-31, 1861.

weight be fixed at $3H7$, $4H7$, or $5H7$, silicium will still retain its positions as the second member of the series $H7n$. The chief properties which distinguish the elements of the series $H7n$ are their high fusing point; their occlusive affinity for hydrogen; and their passivity in the presence of ordinary reagents, to which iron, under peculiar conditions, forms no exception. In regard to their occlusive affinity for hydrogen; the relation of nitrogen to iron and palladium may explain the existence of the ammonium amalgam, in which nitrogen and hydrogen are held together in the nascent state by means of mercury. The formation of silicium hydride by electrolysis, in a manner analogous to that of the ammonium amalgam, would also indicate for silicium a similar occlusive affinity for hydrogen to that possessed by nitrogen.

Although gold in some recent classifications of elements has been separated from the platinum metals, yet, in its primary qualities, it exhibits closer analogies with them than with the members of any other series, and there is no other place vacant in the groups which an element with the atomic weight and physical properties of gold would fit. The constant association in nature of quartz, hematite and specular iron ores, with gold and platinum is a fact fully recognised by chemical geologists,* and confirms the positions assigned for Si, Fe, and Au, in the table as forms of $H7n$.

The remarkable resemblance which the members of the iron group have to one another, while their atomic weights are nearly if not exactly the same, has long been a subject of much interest to philosophical chemists, and if the views which I have enounced respecting the formation of elementary species by condensation be correct, the cause of these resemblances admit of a possible explanation. From

* *Bishcoff's Chemical and Physical Geology*. Vol. iii., 534. Cavendish Soc. Works. *Murchison's Siluria*. Chap. xvii., 433-439.

the great abundance and wide distribution of iron in nature, it is probable that the vapour of this element would form a zone of considerable depth; the upper and lower regions of which, by differences of pressure and temperature might produce allotropic varieties before a definite change to the next higher members in the series occurred. When once varieties of an element were formed, these varieties would be propagated through successive condensations into the next higher members of the series, just as they are found in the palladium and platinum groups of metals. Chemists have already observed that each of the metals of the palladium group appears to be more especially correlated with some particular member of the platinum group, and all are found associated together naturally in the metallic state. If the four members of the platinum group be considered the analogues of the corresponding members of the iron and palladium groups, it will be seen that one of the members of the latter group is missing. M. Sergius Kern, a Russian chemist, has recently discovered a new metal which he classifies with the platinum group, and has given to it the name of davyum.* The specific gravity of the new metal was found to be 9.39, and preliminary experiments on its equivalent show that it is greater than 100 and supposed to be 150—154. Now the specific gravity and atomic weight of the new metal exclude it from the platinum group, and also from the iron group of metals; davyum is therefore the missing element in the palladium group, and will have a specific gravity of about 11, and an atomic weight of 105; or the same density and equivalent as the other members of the group. The state of aggregation of the small quantity of the new metal obtained by M. Kern, may have prevented the same specific gravity being found for it as for the other members.

Although I have designated the highest members of the series $H7n$, as the platinum group, yet if the slight differences

* Comptes Rendus, tome lxxxv., 72, 623, 667. 1877.

in their atomic weights and physical properties admit of explanation by the assumption of their being allotropic varieties of each other, then gold, palladium, and iron, may stand at the head of their respective groups, and determine the species to which the varieties belong. It is no objection to the theory of the members of the respective groups being varieties of each other, that they cannot by any known power of analysis be resolved into their primaries, as the same objection would apply to the natural varieties of organic species determined by naturalists.

We have seen that the quantivalence of most of the members of the preceding groups Hn , $H6n$, is in some way correlated or dependent on the construction of the typical molecules at the head of each series; but in the series $H7n$, the only element which is known to be septivalent is manganese; but the relation which this metal has to the iron group; and bearing in mind that the determination of the highest quantivalence of elements is limited by the knowledge of chemists at particular times, and is only arrived at after much research, the septivalency of manganese indicate a much higher quantivalence for the other members of the series $H7n$, than has up to this time been accorded to them.

I have hesitated to introduce hypothetical elements alternating with the iron, palladium, and platinum groups, as the regular sequence of elementary forms is broken by varieties, and from the density of the typical molecule $H7$, it may be that the members of this series are limited to those shown in the table. The density of the typical molecule $H6n$ may also explain the absence of members alternating with Cr, Mo, and W, and I have therefore only introduced one hypothetical element in this series, the analogue of Cr with the atomic weight = 144.

Considering how nearly the numbers representing the molecular constitution, and atomic weights of the members in homologous positions in the higher groups approximate,

the idea occurs that the subsequent condensations of these higher groups are in some way influenced or determined by the antecedent condensations of homologous members of the lower groups, and may be the cause of the departure in the higher groups from the simple ratios and multiple relations observed amongst the elements of the series Hn , and $H2n$. Such perturbations would appear to be similar to those which the planetary bodies exercise on each other to produce modifications in the forms of their orbits, but I leave this question to the further consideration of physicists and astronomers.

The complete parallelism of the halogens and oxygens to each other, and their intensely electro-negative character, point irresistibly to the conclusion that at one period of their history these elements existed in a state of isolation from all the others. How, and under what conditions, they acquired their electro-negative properties can in the present state of knowledge be only a matter of conjecture; but it may be conceived that these elements may have existed originally in the form of a zone or zones revolving within the moon's orbit, but high above the incandescent terrestrial surface; probably before the lunar substance changed from the annular to the globular form. These intra-lunar zones may have gradually acquired their electro negative properties by lunar and terrestrial induction, and by the loss of their primitive heat by radiation into space. Their orbits being too near the earth to permit the zones to assume the spheroidal form, they would upon rupture become incorporated with the positive terrestrial elements, and remain dissociated till the temperature of the mass was sufficiently reduced to enable chemical combination to take place. If Draper's discovery of oxygen in the sun be confirmed, the hypothesis of the existence of an intra-mercurial zone of negative elements which subsequently united with the solar positive elements is at least as probable as the assumption of an intra mercurial planet which has recently been discussed by astronomers. May not the sudden increase in the brightness of variable stars like T Coronæ, Nova Ophiuchi, 1848,

and Nova Cygni, 1876, be due to the intense heat generated by the union of zones of negative elements with the central bodies round which they revolve, or by the condensation of lower into higher forms of elementary species ?

All the positive forms of $H2n$, except glucinum and lead, are well ascertained solar elements, and the remarkable relations which the members of this group have to those of Hn render it highly probable that, besides sodium and copper, other members of Hn are present in the solar atmosphere. From the fact that aluminum, titanium, chromium, and the irons are solar species, higher forms of these elements may also be expected to be found in the sun.

The numerical relations of the atomic weights to which I have directed attention, and the brief outline of a theory of the origin of elementary species which I have founded upon them, give new force to the doctrine of the transmutable nature of elementary substances. But when the synthetical formation of organic compounds is regarded as the greatest triumph of modern chemical science, the problem of building up the higher elements from the lower, may well be deemed insoluble, as they have been formed under cosmical conditions of which we have little or no acquaintance. Very different, however, is the aspect of the problem of resolving the higher elements of each series into their respective types or into hydrogen. For just as by the application of heat, the higher members of homologous series are resolved, through their lower members, into their ultimates, so may it be expected that the elements themselves will, in their turn, give way to more powerful instruments of analysis.

When it is considered, that through the investigations of Dumas, Cooke, Odling, Mendeleeff and others, nearly all the mathematical relations of the atomic weights to each other have been unfolded during the brief interval of thirty years, so that but few steps are now required to render the natural classification of the elements complete, the resolution of elementary species into their primordial ultimates would not appear to be far off.

1	+ H_n —		+ H_{2n} —		H_{3n}	H_{4n}	H_{5n}	H_{6n}	H_{7n}	
2	Li = 7		Cl = 8		C = 12	— = 16	B = 10	— = 18	N = 14	
3	Na = 23	F = 19	Mg = 24	O = 16	Al = 27	— = 32	P = 30	— = 36	Si = 35	
4	K = 39	Cl = 35	Ca = 40	S = 32	— = 42	Ti = 48	V = 50	Cr = 54	Fe = 56	56
									Mn = 56	55
									Ni = 56	58
5	Cu = 62		Zn = 64		— = 69	— = 72	As = 75		Co = 56	58
6	Rb = 85	Br = 81	Sr = 88	Sr = 80	Ga = 96	Zr = 92	Nb = 95	Mo = 96		
7	Ag = 108		Cd = 112		Y = 123	Sn = 116	Sb = 120		Pd = 105	106
									Rh = 105	105
									Ru = 105	105
8	Cs = 131	I = 127	Ba = 136	Te = 128	In = 150	La = 140	— = 140	— = 144	Da = 105	—
9	— = 154		— = 160		E = 177	— = 165	— = 165			
10	— = 177		— = 184		Tl = 204	D = 188	Ta = 185	W = 186	Au = 196	196
									Pt = 196	197
									Ir = 196	197
11	Hg = 200		Pb = 208		Th = 231	U = 240	Bi = 210		Os = 196	198

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 26th, 1878.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section,
in the Chair.

The following gentlemen were elected officers of the
Section for the ensuing year :—

President.

JOSEPH BAXENDELL, F.R.A.S.

Vice-Presidents.

E. W. BINNEY, F.R.S., F.G.S.

ALFRED BROTHERS, F.R.A.S.

Treasurer.

JAMES BOTTOMLEY, B.A., D.Sc.

Secretary.

THOMAS MACKERETH, F.R.A.S., F.M.S.

April 23rd, 1878.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section,
in the Chair.

“On some Colorimetical Experiments,” by JAMES BOT-
TOMLEY, B.A., D.Sc.

If a white disc be sunk in a coloured solution, the in-
tensity of the colour perceived will be a function of the
depth of the disc and the quantity of colouring matter
present. If, in any set of experiments, we make the
intensity of colour constant, then the depth of the disc will

be a function of the quantity of colouring matter only. Occasionally in chemistry we have to deal with quantities so minute that we can no longer use the balance for their determination, and yet those small quantities when they yield coloured solutions are evidently comparable among themselves. In a short communication which I made to this Society (vol. xv., 63), I suggested that such a process as above indicated might be applied for the measuring of such traces, assuming that the depth of the disc would be inversely as the quantity of colouring matter present. Some experiments were made on colouring matter in solution. Glass cylinders were used about 10in. high and 2in. diameter; to one side of each cylinder was pasted a strip of paper marked with an arbitrary scale (each degree of the scale I afterwards found to be 27mm.), the white surfaces used were crucible lids; a piece of glass rod resting on one of the cylinders, having the string of the moveable disc attached to it, served as a windlass for raising or depressing it. To avoid the use of many ciphers I have taken .0001 grm. as the unit of measurement. In the following experiments column A denotes the amount of the colouring salt present, B the depth of the disc, and C the amount of the colouring salt thence derived by calculation. Standard solution 35.2 of permanganate of potash in 500 cc. of water depth of disc, 8.3.

A		B		C
53	6.1	48
70	4.1	71

Experiments were made with still stronger solutions but the differences were considerable.

Standard solution 24 of permanganate of potash in 500 cc. of water, depth of disc, 8.3.

A		B		C
36	5.4	37
48	3.6	55
60	2.8	71

Stillstronger solutions were used with this standard, but the discrepancies between the true and the calculated quantities were considerable. Standard solution 5·8 in 500 cc. of water, length of column, 8·3.

A		B		C
7·2	5·7	8·4

Some experiments were made with solutions of permanganate of potash, using a shorter column for the standard solution. Standard solution 2·4 in 214·6 cc., length of column 3·8.

A		B		C
3·6	2·2	4·1

An experiment was also tried with a still weaker solution. Standard solution 1·4 in 214·6 cc. of water, length of column 3·8.

A		B		C
2·4	2·6	2·1

Some experiments were also made by looking through the column of fluid at a white surface external to the cylinder. Standard solution 12 in 500 cc., length of column 8·4.

A		B		C
14·4	6·5	15·5
28·8	3·1	32·5
16·8	5·3	19

I prepared a fresh standard solution of the permanganate and compared the last solution with it. The new standard was similar to the last in composition.

A		B		C
16·8	5·7	17·6

Thus the result was somewhat more favourable with a newly-prepared solution. I used the permanganate on account of its great tinctorial power, but it is a salt of considerable instability, especially in contact with organic matter. The water used in the experiments was obtained by distilling Manchester town's water from a copper still lined with tin, and condensing in a tin condenser. Now such

water would not be wholly free from oxidizable matter; hence in a little while there will be a depreciation in the tint of the permanganate solution. If the tint be strong the error arising from this cause might be neglected, but when the tint is feeble the error is appreciable. If the depreciation in tint in the two solutions compared were proportional to the depth of tint in each, this source of error might be neglected; but inasmuch as the same quantity of water has been used in making each solution, an equal amount of depreciation is introduced into each. Some of the foregoing experiments will be affected by this source of error. Small sources of error which may be neglected when we deal with ponderable quantities become of sufficient magnitude to vitiate results when we deal with traces.

For experiments with a yellow solution I took a solution of bichromate of potash. Standard solution 10 in 500 cc. of water, depth 8·3.

A		B		C
15	5·4	15·4
20	4·4	18·8
30	3·5	23·7
40	2·5	34·0
50	1·7	48·8
60	1·5	55·3

Still stronger solutions of the bichromate were compared with this standard, but the discrepancies were considerable. Some of the results also given in the last table are not very good; this more especially applies to the third and fourth results. On another occasion I repeated some of these experiments with the following results—standard solution same as last:

A		B		C
30	3	27·6
40	3·2	37·7
50	1·6	51·8
60	1·4	59·3

Another series of experiments was made with the bichromate. Standard solution 50 in 500 cc., length of column 8·3.

A		B		C
60	6·7	62
70	5·3	78
80	4·5	92
90	4	104
100	3·7	112
110	3·4	122
120	3·2	130
130	3	138
140	2·8	148
150	2·6	160

In some of these determinations I felt considerable uncertainty at what depth equality of colour was obtained, and some of the results were likely to vary on repetition, In the table I put 5·3 as the depth after some hesitation, and with the intention to repeat the experiment on some future occasion. On repetition I made the depth of the disc 5·8, which would indicate the amount of the salt used as 71. In some other instances I felt uncertainty when working with a solution of the bichromate in determining equality of tint, so that whether the disc were elevated above or depressed below the position finally chosen the difference in colour was insensible. Perhaps with bright yellow colours the eye requires more experience to detect differences of shade than with colours not so glaring. Moreover, when there is a considerable difference between the quantities of the colouring salt in the two cylinders there is another difficulty to deal with. The colours to be compared differ not only in intensity but also in kind. A very dilute solution of bichromate gives a pale yellow with a slightly greenish shade, which passes, as the amount of salt is increased, into full yellow, orange yellow, and finally orange. A similar observation holds with regard to several other colouring salts. Thus a very dilute solution of permanganate of potash is pink, passing, as the strength of the

solution is increased, into crimson, and finally into violet. I also made some experiments with a blue solution. For this purpose I used an ammoniacal solution of sulphate of copper. The tinctorial power of this salt is not as great as I expected, and I made no experiments with a standard solution containing less than 150. On diluting 15 cc. of an ammoniacal solution with sufficient water to make 500 cc., I noticed after a little time that the solution was somewhat turbid. This was owing to the copper forming an insoluble compound, either hydrate or a subsalt; further addition of ammonia was necessary to clarify the solution. Standard solution 150 in 500, depth 8.3.

A		B		C
200	5.7	218
220	5.3	235
240	5.1	244
260	4.8	259
280	4.5	277
300	4.1	304
350	3.4	366
400	3.0	415
450	2.6	479

Some experiments were made with solutions of known strength, by sinking the disc to the depth assigned by theory, to see if the given tint was attained, instead of moving the disc about until the tint was estimated to be the same and then reading off the result. A solution of bichromate of potash containing 50 in 500 cc., and depth 8.3, was used as a standard. Solutions containing 60, 70, 80, 90, gave similar tints at the calculated depths. Solutions containing 150, and increasing quantities gave tints a little too deep at the calculated depths. Also a standard solution of permanganate of potash, containing 4.8 in 500 cc. of water, and depth 8.3, was taken. Solutions containing 6 and 7.2 gave a similar tint at the depths assigned by the calculation. Solutions containing 9, 9.6, and higher quantities, gave tints a little too deep at the calculated depths.

Some of the foregoing results are fair approximations, and probably better results might be obtained with a more perfect form of apparatus than I employed. The surfaces used were not perfectly flat. Moreover, there was in some of the experiments the indecision arising from the coexistence of several distinct tints on the surface of the disc due to the formation of caustic curves. This I afterwards remedied by working behind a screen of lawn. The effect of this is to render the illumination of the disc much more uniform. Instead of a screen we might also use glass cylinders ground internally. Another difficulty is the indecision respecting the depth at which equality of colour takes place. The degree of indecision will be different for different persons, and I suppose represents want of sensibility and want of skill in detecting shades of colour. So far as it arises from want of skill it is likely to be diminished by more perfect experience with colours. It seems to me that it would be well to have some definite method for exercising the eye, or for testing one's skill in this respect. This might be done by having a series of cylinders filled with coloured solutions, the quantity of colouring matter increasing in arithmetical progression and endeavouring to arrange these solutions in their order, the arrangement being made according to the judgment of the tints. The various success attending such exercises would afford the experimenter some method for testing his skill from time to time; it would also show him under what conditions he could use neither this nor any other method of colorimetry; for when he has arrived at such a stage that he can perceive no difference of tint, notwithstanding an increase in the colouring matter, it seems as useless to try to do so as to use a balance of coarse sensibility to distinguish between two weights which are very nearly the same. By frequent exercise and concentrated attention I think the power of perception of difference of shade may be increased. If such a series of cylinders were taken containing, say

quantities of bichromate of potash, increasing from 1 to 30, the first members of the series would be easily arranged towards the middle of the series, the number of successful discriminations would diminish and few would be able to detect differences in the higher members of the series; for an increment of the salt which produces a considerable difference in intensity when a small quantity of the salt is present produces a small difference when the quantity is large. This would follow from the approximate formula used, for $\frac{dt}{dQ} = -\frac{c}{Q^2}$ ($t = \text{depth}$, $Q = \text{quantity of colouring matter}$, $c = \text{constant}$). So that for a given small difference in the quantity the difference in the depth would be nearly proportional to the inverse square of the quantity. Solutions whose quantities of colouring matter are so similar as to be beyond discrimination by colorimetry may be again brought within its range by the following device: in each of the two cylinders destroy by some appropriate method an equal quantity of the colouring matter; then their differences will be increased relatively to the quantities of colouring matter remaining, and may be sufficiently sensible to be estimated. When I suggested this method of colorimetry I had not seen the article on Light by Sir John Herschel in the *Encyclopædia Metropolitana*. He there gives a probable formula for the intensity of light which has passed through an absorbing medium. His reasoning is as follows. Let the intensity of the incident light be unity, and its intensity on emerging from a unit thickness of the medium, k ; then its intensity at incidence on a second layer will be k_1 and its intensity at emergence k^2 ; hence if t be the thickness of the medium the intensity will be finally k^t , and if the original intensity were a the final intensity would be ak^t . Now, light consists of several species; hence if the composition of the incident light be $a_1 + a_{11} + a_{111} + \&c.$ the composition of the emergent light will be $a_1 k_1^t + a_{11} k_{11}^t$

$+ a_{III} k_{III}^t + \&c.$ If we use the letter I to denote incident light, T to denote transmitted light, and A absorbed light, Herschel's formulæ may be written more briefly as follows:

$$\Sigma a = I, \quad \Sigma a k^t = T,$$

k will be a function of the quantity of colouring matter present in a unit thickness. If g be that quantity we may write $k=f(g)$, or if we expand $k=f(0)-f'(0)g +$ higher terms in g . Now, g is very small. Let then the terms higher than the second be neglected. The first term will be the light transmitted when no colouring matter is present. If the colouring matter were dissolved in an absolutely transparent medium, the first term would be unity. In practice water or some colourless medium is used; but the amount of light absorbed by a thin layer of pure water is very small, so that we shall make only a very small error in writing $f(0)=1$. Hence $k=1-f'(0)g$. Substituting this value in Herschel's formula and expanding, we have

$$\Sigma a - \Sigma a f'(0)gt + \&c.$$

Now, as g is very small, we may neglect terms beyond the two first (unless t be very large). Hence

$$\Sigma a f'(0)gt = I - T = A.$$

If we assume the colour to be constant, A will be a constant term. Now, let Q denote the whole quantity of colouring matter in the cylinder, and L the whole length of the column; then $g = \frac{Q}{L}$ and the formula may be written

$$\Sigma a f'(0) \frac{Q.t}{L} = A, \text{ or } Qt = \frac{A.L}{\Sigma a f'(0)} = \text{constant.}$$

MICROSCOPICAL AND NATURAL HISTORY SECTION.

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CHARLES BAILEY, Esq., in the Chair.

The Treasurer's Account and Report of the Council were read and passed.

The Election of Officers for the ensuing year then took place as follows :—

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