PROCEEDINGS

OF THE

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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PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 1st, 1872.

Rev. William Gaskell, M.A., Vice-President, in the Chair.

Among the donations announced were a beautiful photographic copy of a fine portrait of the late Mr. John Dawson, of Sedbergh, by Mr. Westall, A.R.A., and a fine photographic portrait of the Rev. Canon Sedgwick, M.A., F.R.S., Honorary Member of the Society, both presented by Canon Sedgwick.

On the motion of Mr. Baxendell, seconded by Mr. Kipping, the thanks of the Society were unanimously voted to the Rev. Canon for his interesting and valuable donations.

"On the Composition of Ammonium Amalgam," by R. Routledge, B.Sc.

The substance now known as ammonium amalgam appears to have been first obtained by Seebeck* in the beginning of the year 1808, immediately after Davy had announced his brilliant discovery of the isolation of potassium and sodium by means of the Voltaic battery. Seebeck prepared the amalgam by placing mercury which formed the negative pole of a battery in contact with moistened carbonate of ammonia. About the same time Berzelius and Pontin† obtained the like result with solution of ammonia.

* *Annales de Chimie*, LXVI. 191.

† Gilb., VI. 260, and *Bibliothèque Britannique*, No. 323, 324, p. 122.

This discovery they communicated to Davy early in June, 1808, declaring their conviction that ammonia, like potash and soda, must be an oxide, and that the new substance was a combination of its metallic constituent with mercury. Davy* immediately commenced a series of elaborate experiments on the production and properties of the amalgam, and in an account of these experiments laid before the Royal Society in the same month he first uses the name ammonium to indicate the supposed metallic basis of ammonia. So convinced was Davy that the substance united with mercury in the amalgam was of a metallic nature, and that by combining with oxygen it constituted ammonia, that he was inclined to view nitrogen and hydrogen, if not as oxides of metals, at least as metallic gases.

Davy discovered that the ammonium amalgam was readily produced when an amalgam of potassium was made to act on moistened sal-ammoniac. He found that the electrically prepared amalgam when introduced into a tube rapidly evolved gas, which he describes as consisting of "about two-thirds to three-fourths of ammonia, and the remainder hydrogen." In another experiment, amalgam obtained by potassium was moistened with strong liquid ammonia, and when heated in a tube generated gas which was proved to consist of two-thirds ammonia and one-third hydrogen.

In the following year Gay Lussac and Thénard† investigated the ammonium amalgam, and were led to regard it as a triple compound of mercury, ammonia, and hydrogen. They found on putting some of the amalgam prepared by potassium into a tube which was filled up with mercury and then inverted in a vessel of that liquid, that the amalgam gave off, in decomposing, ammonia and hydrogen gases in the proportion of $2\frac{1}{2}$ volumes to 1. But the electrically prepared substance gave off the gases in quite another pro-

* *Phil. Trans.*, 1808, p. 355.
† *Recherches Physico-Chimiques*, I. 52.

portion, the ratio in four different experiments being nearly as 28 volumes of ammonia to 23 of hydrogen. These results were obtained by first drying the amalgam with bibulous paper, then introducing it into a tube containing a little mercury, closing the tube with the finger, agitating it for some minutes with the enclosed air, opening the tube after inversion in mercury, measuring the ammonia by absorbing with water, and determining eudiometrically the hydrogen mixed with the residual air. The amalgam was afterwards described by Thénard, in his *Traité de Chimie*,* under the name of "ammoniacal hydride of mercury."

It is interesting to observe that in 1816 Ampère,† in the passage where the now universally received views on the constitution of ammoniacal compounds are first propounded, refers to the amalgam. Speaking of the difficulty of assimilating the constitution of ammoniacal to metallic salts, he remarks — "This difficulty would disappear if we admit that, just as cyanogen, although a compound body, exhibits all the properties of the simple bodies which are capable of acidifying hydrogen, so the combination of one volume of nitrogen and four volumes of hydrogen which is united to mercury in the amalgam discovered by M. Seebeck, and to chlorine in the hydrochlorate of ammonia, behaves in all the compounds which it forms like the simple metallic substances." This theory was more fully developed by Berzelius and was soon generally received, except as regards the amalgam, concerning which various conflicting opinions were entertained. Daniell,‡ for example, speaks of it as a mere mixture of mercury and gases resulting from the cohesion of the mercury and the adhesion to it of the gases, and he cites the absorption of oxygen by melted silver as a similar case.

* Vol. II. p. 162, 3me ed.

† *Annales de Chimie et de Physique*, II. 16, Note.

‡ *Chemical Philosophy*, p. 420.

Grove,* in 1841, made a few experiments on the amalgam, and advanced the idea that it is a chemical compound of mercury and nitrogen, merely swelled up with hydrogen.

In 1864, Dr. Wetherill† performed several ingenious experiments on the amalgam, without however attempting any quantitative estimate of its composition. He concludes that it is not an alloy of mercury and ammonium, and that the swelling up of the mass is due to the retention of gas bubbles by virtue of some unexplained action which he somewhat vaguely refers to catalysis.

In the *Annalen der Chemie u. Pharmacie* for 1868‡ is a paper by Landolt, in which, after pointing out the discordance of the quantitative results obtained by Davy, and by Gay Lussac and Thénard, he describes a method by which he attempted a new determination of the relative quantities of ammonia and hydrogen. He prepared the substance from a solution of sal-ammoniac, separated from the mercury, which formed the negative pole, by a porous cell. The amalgam, when removed from the circuit, was washed in a stream of water to get rid of the adhering solution of sal-ammoniac, which always contains free ammonia. It was then immediately plunged into dilute hydrochloric acid of known strength, and the hydrogen evolved was received in a graduated cylinder placed over it, while the ammonia was estimated by determining the amount of unneutralised acid in the liquid. Two experiments gave results corresponding respectively to 2·15 and 2·4 volumes of ammonia to 1 of hydrogen. These figures of Landolt's cannot be considered satisfactory, neither nearly agreeing with each other, nor approximating to the ratio 2 : 1 sufficiently closely to justify his conclusion that they "completely confirm the results formerly obtained by Davy." Indeed Landolt points out a serious defect in his process, namely, that however rapidly

* *Phil. Mag.*, United Series, vol. xix., p. 97.

† *Silliman's Amer. Journal* [2], xl., 160.

‡ Supp. Bd., vi., p. 346.

the amalgam may, after washing, be transferred into the acid, the adhering water will nevertheless take up some more ammonia from the continuously decomposing substance while the hydrogen escapes.

It must be observed that Davy himself appears to have found a difficulty in obtaining consistent results, for he does not seem to have ever entirely satisfied himself as to the proportions of the two gases. These are the words in which he sums up his observations :—" As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. *If* this result is taken as accurate . . . . ", &c.*

This statement of Davy's being apparently the only authority for the assertion that the decomposing amalgam gives off the gases in atomic proportions, and yet being in conflict with Gay Lussac and Thénard's results, it appeared to me desirable to attempt to obtain more exact determinations.

I used amalgam prepared by electricity in the manner described by Landolt.

A simple mode of eliminating the disturbing effect produced by the attraction of ammonia for moisture suggested itself. A U-shaped glass tube was provided, open at both ends, about 1·4 centimetres in diameter and having its shorter limb 40 centimetres long. At the bottom of the longer limb, just above the bend, there was an outlet tube to which was attached a piece of caoutchouc tubing closed by a pinch-cock. Mercury was poured into the tube until it filled

* Bakerian Lecture, 1809.

about two-thirds of the shorter limb, into which was then introduced the amalgam after the latter had been wiped with filtering paper. Then into the end of the limb containing the amalgam, a caoutchouc stopper, perforated with a small opening, was immediately thrust so far that its upper surface came a little below the rim of the tube. The decomposition of the amalgam was then allowed to proceed for a few minutes, during which period any moisture adhering to the amalgam or present in the tube would become completely saturated with ammonia, and then the two gases would begin to escape through the perforation in the stopper in the proportions in which they are really evolved. Mercury was now poured into the open end of the longer limb until the amalgam just made its appearance at the top of the hole in the stopper, which was then closed by pushing in a piece of glass rod. The evolved gases being now retained in the tube pressed up the mercury in the longer limb, and it was from time to time drawn off by the outlet tube to prevent undue pressure on the stopper. When the decomposition was complete, which usually occurred in about $1\frac{1}{2}$ hours (but in one case more than $2\frac{1}{2}$ hours were required) the mercury was brought to the same level in both limbs and the space occupied by the gases was marked on the tube. A little mercury was then let out so as to make the pressure on the gas somewhat less than that of the atmosphere, and the space above the stopper was filled with hydrochloric acid diluted with a little water. The glass rod was then carefully withdrawn for an instant so that a few drops of the acid might enter the tube. The ammonia gas present was of course immediately absorbed, and the mercury having been again brought to the same level in both limbs, the space occupied by the residual hydrogen was marked on the tube. The volumes occupied by the gases were determined by finding the quantity of water required to fill them from a burette.

The following are the results of four experiments :—

No. of Experiment	Volume of the mixed gases.	Volume of residual hydrogen.	Volume of ammonia absorbed.	Volumes of ammonia found for one volume of hydrogen.
	c.cm.	c.cm.		
1	20·8	7·0	13·8	1·97
2	18·2	6·2	12·0	1·93
3	12·8	4·3	8·5	1·98
4	13·6	4·6	9·0	1·95

I believe these figures are as nearly accordant with the atomic proportions as could be expected from the means employed, where the possible error in determining the volumes might amount to perhaps ·2 c.cm.

In another similarly conducted experiment, in which it was sought to obtain as much gas as possible, the tube was closed too soon, and the result showed a deficiency of ammonia, but is otherwise interesting:—

Experiment 5.

Volume of Mercury in the amalgam.	Volume of amalgam.	Volume of the mixed gases.	Volume of residual hydrogen.
c.cm.	c.cm.	c.cm.	c.cm.
11·8	30·5	49·0	18·0

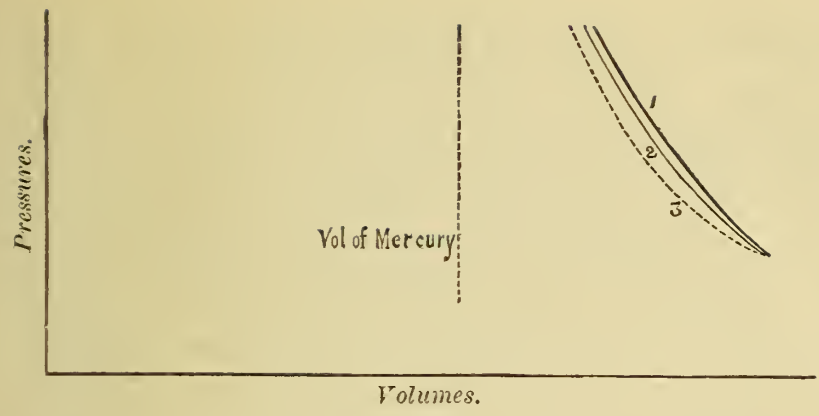
A new observation on the amalgam has recently been made in America by Professor C. A. Seeley,* who found, by subjecting it to varying pressure that its volume changes, apparently in accordance with Mariotte's law. He employed simply a glass tube fitted with a plunger, and did not measure the pressures or volumes. His conclusions were that the amalgam is a mechanical or physical mixture of liquid mercury with the gases ammonia and hydrogen, and that its semifluid consistence is due to the mixture having the nature of a froth.

Being desirous of submitting Seeley's remark on the compressibility of the amalgam to the test of direct measurement, I subjected the electrically formed amalgam to pressure in a glass tube 48 centimetres long and 1·3 centimetres diameter. The pressure was applied by connecting the tube with a syringe, by which air could be forced into

* *Chem. News*, June 10th, 1870.

the apparatus, and the amount of the pressure was measured by a column of mercury in an open manometer. There was some difficulty in measuring the volume owing to the occasional escape of bubbles of gas, which caused abrupt alterations of the level. The results obtained are given in the following table, which also contains a column of volumes calculated on the supposition that the amalgam is a mere mixture of fluid mercury and gas, allowance being made for the pressure on the gas due to the column of mercury in the amalgam itself. The extreme case was assumed, namely, that this additional pressure is represented by a column of mercury half the height of the amalgam.

No. of Experiment	Volume of mercury in the amalgam.	Atmospheric pressure in centimetres of mercury.	Volume of amalgam under atmospheric pressure.	The increased pressure in centimetres of mercury.	Observed volume of amalgam under increased pressure.	Calculated volume of amalgam under increased pressure.
	c.cm.		c.cm.		c.cm.	c.cm.
6	14·5	76·2	21·0	152·4	18·0	17·9
7	11·9	76·8	23·0	188·2	17·5	17·1
8	11·9	76·8	22·7	200·9	17·0	16·4
9	24·4	76·2	36·2	152·4	31·6	30·9
10	24·4	76·2	31·6	152·4	28·0	27·4
11	13·2	76·2	28·7	152·4	23·0	21·6
12	13·2	76·2	22·5	152·4	18·5	17·2
13	10·4	76·2	18·0	186·3	14·7	13·7
14	10·4	76·2	16·0	186·3	12·8	12·8
15	23·8	76·2	40·4	178·7	33·6	31·9
16	23·8	76·2	42·0	176·1	33·6	32·7
17	23·8	76·2	42·8	152·6	35·0	34·7
18	23·8	76·2	40·4	177·4	33·3	31·9
19	23·8	76·2	42·2	102·6	38·8	38·5
20	23·8	76·2	42·2	153·6	34·0	34·0
21	23·8	76·2	42·2	177·4	33·0	32·7
22	23·8	76·2	42·0	201·5	32·2	31·6
23	23·8	76·2	40·2	177·4	32·2	32·1
24	23·8	76·2	40·6	201·5	31·2	30·6
25	23·8	76·2	36·2	149·5	32·6	30·6
26	29·2	76·2	42·0	177·4	36·8	35·4
27	29·2	76·2	42·0	200·2	36·2	34·7
28	29·2	76·2	40·6	173·6	36·0	34·7
29	29·2	76·2	39·5	198·9	34·4	33·4
30	24·6	76·2	32·0	155·9	29·7	28·4
31	24·6	76·2	34·0	177·4	30·4	28·7



Five points deduced from the mean results of experiments 15 to 24 having been laid down in relation to rectangular axes, the curve (1) which passed through them is represented in the diagram, which shows also the curve (2) through five points representing the calculated volumes, and a line (3) representing volumes corresponding to the pressures which were applied to the top of the columns of amalgam.

The diagram and figures sufficiently show that the compressibility of the amalgam agrees nearly with the supposition of its being a mixture of gas and mercury, but that it is, however, somewhat less compressible. This no doubt is owing chiefly if not entirely to its want of fluidity.

I think that from these experiments I am warranted in drawing the two following conclusions, viz. :—

1. In the fact of the gases being evolved in atomic proportions, we have the clearest proof that the ammonia and hydrogen are chemically combined.

2. The compressibility of the mass proves that the enlarged volume or swelling up is due mainly, if not entirely, to free gases entangled in it.

In connection with the first of these conclusions arises the further question whether the NH_4 is combined with the mercury. That it is so combined appears in the highest degree probable from the apparently uniform diffusion of the NH_4 throughout the mass, and from the fact that such a union would be only one additional instance of the innumerable cases in which this radical plays the part of a metal. Seeley says, that if the radical NH_4 be contained in the amalgam at all, it must be in the state of gas. But the figures furnished by my fifth experiment show, that if this supposed NH_4 gas had the normal molecular volume, and existed in the amalgam from the beginning, a force of two atmospheres would be required to compress it within the amalgam. The decomposition therefore is progressive, and points to the existence of a real compound of NH_4 with the mercury. We may therefore admit, that such a compound is originally formed, and decomposes rapidly into mercury, ammonia, and hydrogen, while the gases becoming entangled in the mass impart to it that remarkable turgescence, which is not however a property of the original compound (or ammonium amalgam), but merely an accidental result of its decomposition.

As to the cause of the retention of the gases, I am not prepared to offer an opinion, further than that its explanation would probably involve physical rather than chemical considerations.

I have to express my obligation to the kindness of Dr. Roscoe for the use of the appliances of the laboratory at Owens College, where the experiments were carried out, and I am also indebted to him for valuable suggestions.

Ordinary Meeting, October 15th, 1872.

E. W. Binney, F.R.S., F.G.S., Vice-President, in the Chair.

Ordinary Meeting, October 29th, 1872.

Edward Schunck, Ph.D., F.R.S., Vice-President, in the Chair.

Dr. R. Angus Smith, F.R.S., described a remarkable fog which he saw in Iceland. It appeared to rise from a small lake and from the sea at about the same time, when it rolled from both places and the two streams met in the town of Reykjavik. It had the appearance of dust, and was called dust by some persons there at first sight. This arose from the great size of the particles of which it was composed. They were believed to be from $\frac{1}{400}$th to $\frac{1}{300}$th of an inch in diameter. They did not show any signs of being vesicular, but through a small magnifier looked like transparent concrete globules of water. They were continually tending downwards, and their place was supplied by others that rolled over.

Ordinary Meeting, November 12th, 1872.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Charles Anthony Burghardt, Ph.D., and Henry Arthur Smith, F.C.S., were elected Ordinary Members of the Society.

"Additional Notes on the Drift Deposits near Manchester," by E. W. Binney, V.P., F.R.S., F.G.S.

In my classification of the Drift Deposits of Manchester, printed in Vol. VIII. (second series), is given a fourfold division of the beds, No. 4, or the lowest under the till, being termed Lower Gravel, and described as a bed of sand or coarse gravel having the pebbles contained in it, consisting of the same kind of rocks as those found in deposits Nos. 1, 2, and 3, well rounded, sometimes but not always occurring under the till or brick clay.

Professor Hull, F.R.S., in a paper printed in Vol. II. (third series) of the Memoirs of the Society, states, "Another modification which we found it necessary to make had reference to the lower sand (No. 4) underlying the till in Mr. Binney's classification. We have nowhere been able to discover such a bed *in situ* during our examination; and it is remarkable that in the section of the drift which was furnished by Mr. Binney as having been proved at St. George's Colliery, Manchester, and where it is stated that this sand and gravel (No. 4) is 10ft. 6in. in thickness, there is no appearance whatever of it in the neighbouring quarries of Collyhurst, where the till may be seen directly reposing on the Permian sandstone. I do not however wish to deny that there are occasional patches of sand or gravel underlying the lower till, because such bands occur in the till itself. My only object is to remove this member from the dignity of a distinct subdivision of the drift series, at least till there is some better evidence of its existence than the reports of well sinkers, the elasticity of whose system of nomenclature is unhappily proverbial." He then gives his fourfold division. In a paper of my own, printed in the same vol. as Mr. Hull's, a list of eleven drift sections is given in which the lower gravel (No. 4) appears in ten found in Manchester.

No doubt, as Mr. Hull states, it is quite true that on the

Permian sandstone in the Vauxhall delph at Collyhurst the till is seen resting upon that rock without any intervening bed of sand or gravel; but if any one considered the exposed position of the rock at the last named place when compared with the sheltered locality at St. George's Colliery, there would be no difficulty in conceiving that a bed of sand or gravel might be removed by denuding causes in the former, while it would be preserved in the latter. Certainly this deposit was not given on the authority of an ignorant well sinker, but on that of the late Mr. Thomas Hill, an intelligent colliery manager, who was not likely to be deceived in the change of a bed of till to 10ft. 6in. of sand and gravel.

In my first paper previously referred to ten other instances were given of the occurrence of the lower gravel under the till in and near Manchester, and in the Additional Notes on Drift printed in the last two vols. of the Proceedings of the Society other cases are given of the bed having been found under.

In the present communication more sections are brought forward, the first three of which are from my own observation.

In Dantzic-street near the corner of Wells-street, Shudehill, the following beds were met with:

	ft.	in.
Till	18	0
Coarse Gravel	3	6
Broken Rock—Trias	3	6
	25	0

The gravel contained rounded pebbles of the size of a man's head, and is of a coarser description and a duller colour than I had ever previously observed in the neighbourhood of Manchester.

At the south end of George-street near Oxford-road, opposite Mr. Jackson's warehouse, the following section was met with:

	ft.	in.
Till	26	0
Red Gravel and Sand resting on Trias	4	0
	30	0

In a shaft shown me by my friend Mr. Mellor at Lime-kiln-lane, Ardwick, there was:

	ft.	in.
Till, about	25	0
Coarse Gravel resting on Upper Coal Measures	18	0
	43	0

At Levenshulme Printworks, in Mr. Aitken's bore-hole:

	ft.	in.
Till	70	0
Sand and Clay	4	0
Sandy Gravel—Trias	5	0
	79	0

By the kindness of Mr. Alfred Waterhouse I am enabled to give three sections of the drift deposits met with in excavating the foundations of the new Town Hall in Albert-square.

At the south-west angle of Lloyd-street, Albert-square:

	ft.	in.
Till (hard dry clay)	16	3
Red Loamy Sand	3	0
Running White Sand	0	9
Loam and Sand on Trias	1	6
	21	6

At the north-east angle:

	ft.	in.
Till	17	0
Soft Sand	0	3
Trias	7	0
	24	3

At the north end Albert-square corridor:

	ft.	in.
Till	13	6
Light Loam	2	0
Running Sand	0	7
Rough Clay, mixed	2	0
Fine Red Sand	1	6
Shaly Rock—Trias	1	3
	20	10

All the above sections show that the lower gravel and sand is a very variable deposit. Up to the present time, to my knowledge, no organic remains have been found in it, and the rocks met with have not been so carefully examined to speak with certainty as to whether or not they are of the same description as those found in the till and upper gravels. It may be the remains of a much greater deposit, which has been denuded before the formation of the till. Up to this, so far as I know, no scored or striated pebbles have been observed, although there are plenty of well rounded rocks in it.

Whenever any excavations are being made through the till it is desirable that parties present should carefully examine the sands and gravels lying under it as well as the broken rock so often met with on the upper portions of Triassic, Permian, and Carboniferous beds found near Manchester.

The classification of the drift in this district may still be conveniently divided into, in the descending order:—1. Valley sands and gravels. 2. Beds of sand and gravel containing layers of clay and till. 3. Thick bed of till containing beds of sand and gravel. 4. Lower sands and gravels.

"An Account of some Experiments on the Melting Point of Paraffin," by B. Stewart, F.R.S.

The following experiments were made with the view of ascertaining

1st, Whether the melting point of different specimens of paraffin is the same.

2nd, Whether that of the same specimen remains the same.

The method of observation adopted in these experiments was as follows. The thermometer had its stem fitted into the cork of a colourless glass flask so that when the flask was corked the bulb was in the centre of the flask, the extremity of the mercurial column appearing during the experiment slightly above the cork. The flask was kept heated to a point slightly below that of the melting point of paraffin. The bulb of the thermometer was then dipped for a few seconds into some melted paraffin a few degrees above its melting point, and while covered with a fluid coating of paraffin was replaced in the centre of the flask. The flask being only a very little colder than the bulb, the cooling was then very slow.

The instrument was placed so that the reflected image of the bar of a window was seen distinctly in the mercury of the bulb through the liquid paraffin. One observer carefully scrutinised this reflected image by a lens, while another watched the downward progress of the column of mercury in the stem of the thermometer. As soon as the observer scrutinising the image observed a want of definition produced by incipient freezing, he noted the circumstance to his colleague watching the column, and thus the exact reading at which freezing began was ascertained. It was found easily possible to ascertain this point to one tenth of a degree Centigrade. Four or five separate observations were generally taken, before each of which the thermometer was re-dipped into the melted paraffin.

In case of any change taking place in the zero of the thermometer while the experiments were in progress, the instrument was tried in melting ice before each experiment.

The thermometer employed was a standard, constructed at Owens College, No. 3.

The coating of paraffin surrounding the bulb was sometimes kept from one experiment to another, being always carefully dried after the bulb was plunged in melting ice, and sometimes it was removed, but this circumstance did not appear to affect the results.

It was soon seen that different specimens of paraffin had very different melting points, so that the research was directed to the second question, namely, whether the same specimen retains the same melting point, after being frequently melted and solidified.

The following is a record of the various experiments made :—

1872.		
Feb. 29	Paraffin melted	at 45·05.
Mar. 6	,, ,,	(thermometer not observed).
,, 13	,, ,,	at 44·90.
,, 21	,, ,,	(thermometer not observed).
,, 26	,, ,,	at 44·9.
April 11	,, ,,	(thermometer not observed).
,, 19	,, ,,	,, ,, ,,
,, 26	,, ,,	at 45·00.
May 3	,, ,,	(thermometer not observed).
,, 10	,, ,,	,, ,, ,,
,, 16	,, ,,	at 45·00.
,, 23	,, ,,	(thermometer not observed).
June 1	,, ,,	,, ,, ,,
,, 6	,, ,,	,, ,, ,,
,, 13	,, ,,	at 44·90.

The paraffin was melted without an observation of the thermometer at the following dates — June 19, 27; July 3, 19, 25; Aug. 1, 9, 16, 22, 31; Sept. 6, 14, 21, 27; Oct. 8, 17.

Observations with the thermometer were then resumed with the following results :

Oct. 24 ……	Paraffin melted at 44·60.		
„ 31 ……	„	„	(thermometer not observed).
Nov. 7 ……	„	„	at 44·70.
„ 11 ……	„	„	at 44·75.

The experiments now described have been made chiefly by Mr. F. Kingdon, assistant in the Physical Laboratory of Owens College. The most probable conclusion to be deduced from them appears to be that the melting point of this specimen of paraffin has become somewhat lowered since the experiments began.

It is proposed to continue these experiments for some time longer; but in the meantime it has been thought desirable to describe the method of research, as this may be of interest to observers of melting points.

Ordinary Meeting, November 26th, 1872.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Dr. R. Angus Smith, F.R.S., said that he, like others, had observed that the particles of stone most liable to be in long contact with rain from town atmospheres, in England at least, were most subject to decay. Believing the acid to be the cause, he supposed that the endurance of a silicious stone might be somewhat measured by measuring its resistance to acids. He proposed therefore to use stronger solutions, and thus to approach to the action of long periods of time. He tried a few specimens in this way, and with most promising results. Pieces of about an inch cube were broken by the fall of a hammer and the number of blows counted. Similar pieces were steeped in weak acid; both sulphuric acid and muriatic were tried, and the latter preferred. The number of blows now necessary was counted. Some sandstones gave way at once and crumbled into sand, some resisted long. Some very dense silicious stone was little affected; it had stood on a bridge unaltered for centuries, in a country place however. These trials were mere beginnings; he arranged for a very extensive set of experiments to be made so as to fix on a standard of comparison, but has not found time.

"On some some points in the Chemistry of Acid Manufacture," by H. A. Smith, F.C.S.

The author endeavours to throw some light on the interior economy of the lead chamber as at present used in the manufacture of sulphuric acid, by making first:—

An experimental examination of the causes which determine the action, inter se, of the gases in the lead chamber.

The conclusion come to differed from that generally received. He believes that action can take place between dry sulphurous acid and nitric acid gases, without the use of steam, and showed by several experiments that if action be commenced between the above mentioned gases it continues, even in the absence of air, till all the available oxygen present in the nitric acid has been made use of.

He also comes to the following conclusions:—

1. That the volume of steam introduced should be less than the combined volumes of the two gases.
2. That the volume of steam introduced should increase in proportion to the increase of temperature.
3. That the greatest amount of action between the two gases (and therefore the greatest yield of vitriol) takes place near the surface of previously formed sulphuric acid, and that therefore in 'starting' the

working of a chamber sulphuric acid should be run upon the bottom in preference to water, as at present generally done.

4. That the upper part of the chamber is of use principally as a 'reservoir,' and that little or no action takes place between the gases at that part.

The next point claiming attention was:—

The distribution of the gases in the lead chamber.

The following tables will show the results arrived at:

SULPHUROUS ACID.—TABLE I.

Length of Chamber in feet.	10	20	30	40	50	60	70	80	90	100	110	120	130	140	Length of Chamber in feet.
No. 2. ft. in height, 15 (Entrance.)	72%	70to 72%	46%	31to 33%	25%	26%	30%	22%	29to 30%	22%	23%	13%	18%	18%	15 ft. in height. (Exit.)
No. 1. ft. in height, 3 (Entrance.)	3%	8%	16%	29%	28%	18%	19%	20%	17%	17%	14%	13%	8%	16%	3ft. in height. (Exit.)
	10	20	30	40	50	60	70	80	90	100	110	120	130	140	

Length of Chamber in feet.

No. 1 represents the percentage of acid at 3 feet from bottom of chamber.
No. 2 ,, ,, 15 ,, ,,

SULPHURIC ACID.—TABLE II.

Length of Chamber in feet.	10	20	30	40	50	60	70	80	90	100	110	120	130	140	Length of Chamber in feet.
No. 2. ft. in height, 15 (Entrance.)	0%	0%	6%	18%	23%	20%	18%	16%	19%	12%	12%	7%	7%	10%	15 ft. in height. (Exit.)
No. 1. ft. in height, 3 (Entrance.)	81%	89%	76%	70%	68%	67%	60%	56%	48%	30%	38%	30%	36%	33%	3 ft. in height. (Exit.)
	10	20	30	40	50	60	70	80	90	100	110	120	130	140	

Length of Chamber in feet.

No. 1 represents the percentage of acid at 3 feet from bottom of chamber.
No. 2 ,, ,, 15 ,, ,,

Nitric Acid.—Table III.

Length of Chamber in feet.	10	20	30	40	50	60	70	80	90	100	110	120	130	140	Length of Chamber in feet.
No. 2. ft. in height, 15 (Entrance.)	25%	18%	13%	13%	8%	7%	14%	13to 14%	16%	20%	7%	3%	6%	6%	15 ft. in height. (Exit.)
No. 1. ft. in height, 3 (Entrance.)	8%	3%	6%	4%	4%	12%	8%	17%	20%	26%	26%	15%	12%	3%	3 ft. in height. (Exit.)
	10	20	30	40	50	60	70	80	90	100	110	120	130	140	

Length of Chamber in feet.

No. 1 represents the percentage of acid at 3 feet from bottom of chamber.

No. 2 „ „ 15 „ „

Ordinary Meeting, December 10th, 1872.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"Observations of the Meteoric Shower of November 27th, 1872."

1.—By E. W. Binney, F.R.S., F.G.S.

On the 27th November last, at Douglas, in the Isle of Man, my attention was called by an inmate of my house to numerous meteors in the sky. On going out of doors about 7.45 p.m., they were seen radiating from a point in Andromeda and falling in all directions towards the horizon, some not proceeding far down before they disappeared, whilst others travelled to a much greater distance. The sky was perfectly clear for three hours, during which time I observed them, and they appeared in all directions to be equally numerous except during the last hour. Some were as large as a star of the first magnitude and others were only just perceptible. Nearly all of them appeared to leave tails in their course, which were generally straight, but some of them were curled. In colour most of them were white or yellowish white, but some of the larger ones were of a reddish tinge. At about 7.45 p.m. six were noticed at one time. At 8.45, on looking at about a quarter of the space of the heavens, towards the west, I counted during a minute 21, 11, 24, and 12 respectively. This would give an average of 17 per minute; assuming that the other three portions of the heavens afforded as many, and to me the meteors appeared to be about equally dispersed, so there would be probably about 68 per minute during the two first hours I observed them. At eleven o'clock they were still falling, but not so numerously. The early part of the evening was rainy, but it cleared up shortly before seven, and I am informed that meteors were then observed.

On the 3rd December inst., at 8.45 p.m., there was visible an aurora in the form of a beautiful arch of a yellowish white colour, extending from east to west and reaching up to the lower parts of *Ursa Major*. A slight trace of streamers was seen on the top of the arch.

2.—By Joseph Baxendell, F.R.A.S.

The early part of the evening of the 27th of November was cloudy, and the meteors were not seen till about 10 minutes to 7, when a partial clearing occurred. It soon became evident that they belonged to a distinct meteoric stream, and my attention was therefore chiefly directed to the determination of the position of the radiant point. The observations were however frequently interrupted by clouds, and at no time was the sky entirely cloudless. The intervals of observation and the number of meteors whose tracks were observed with sufficient precision to be of use in the determination of the position of the point of divergence were as follows :—

h. m.		h. m.		Number of Meteors.
6 53	to	7 9	G. M. Time	65
7 21		7 51		54
8 1		8 15		80
8 31		8 34		9
8 49		9 2		31
11 21		11 27		7
11 33		11 54		15
12 7		12 19		10

The total number was 271, and of these 266 had the points of intersection of their paths in an elliptical area of 12 degrees long and 8 or 9 degrees broad, the centre of which was in right ascension 22½ degrees, and north declination $44\frac{1}{2}$ degrees, near the small star Chi Andromedæ. Three of the remaining five had their radiant point in the constellation Cassiopeia.

The average brightness of the meteors was equal to that of a star between the 3rd and 4th magnitudes; many, however, were equal to stars of the 1st magnitude, and several of the finest exceeded the planets Jupiter and Venus when in their positions of maximum brilliancy. The colour for the most part was white; in many, however, it was yellow or orange, and in several of the brightest it was at first white and then a deep red immediately before extinction.

Most of the brighter meteors left luminous trains, but these seldom remained visible for more than a few seconds.

The apparent velocity of movement was decidedly less than that of the 13th of November meteors.

The paths of many of the meteors were more or less curved, and many of them formed curves of double curvature.

It was observed that the radiant point appeared to move to the eastward during the progress of the shower, so that the mean position, from the observations made up to 8h. 34m., was about 3 degrees to the west of the position derived from the observations made afterwards.

The mean position of the radiant point, as given above, shows that the course of the stream coincides almost exactly with the orbit of Biela's comet.

3.—By Alfred Brothers, F.R.A.S.

The sky at Wilmslow appears to have been less clouded than at Cheetham Hill, and I may therefore have had a better view of the display than Mr. Baxendell. From about 5.50 to 8.30 there was very little cloud, and during that time the meteors were falling very nearly at the same rate. There was no difficulty in determining the radiant point—γ Andromedæ being about the centre.

Probably few meteor showers have ever been seen more favourably for determining the radiant than this one. The result of careful counting by myself and Mr. Wilde was that from 1800 to 2000 per hour were visible to the naked eye. The N.W. horizon was distinctly illuminated about 8 o'clock by auroral light, and the whole sky was more or less luminous during the whole time.

Mr. W. Boyd Dawkins, F.R.S., brought before the notice of the Society some remarkable forms of stalagmites which he had obtained from some caves near Tenby. In one cave the calcareous deposit had taken the form of small mushrooms standing close together with a stem not much thicker than a hair, that covered every part of the surface, and in some places had their tops of a dull red colour, and in others of a snow white. In a second every pool was lined with most beautiful crystals of dog-tooth spar, while from the roof there descended slender stalactitic pillars, some snow white and others of a deep red, and most of the thickness of a straw. They stood almost as closely together as the stems of wheat in a wheat field. In a few pools where the drip caused constant agitation of the water, pea-like rounded concretions of carbonate of lime were formed, some of which, polished by friction, were almost as lustrous as pearls, and might fairly be termed 'cave-pearls.'

"On the date of the Conquest of South Lancashire by the English," by W. Boyd Dawkins, M.A., F.R.S.

The most important event in the history of Lancashire, the conquest by the English, has been either lightly touched upon by the county historians such as Baines and Whittaker, or so interwoven with the Arthurian legends as to be almost unintelligible. The date, so far as I know, has been altogether ignored.

What, however, the modern writers have passed by or misunderstood, may be gathered from certain events recorded in the History of Nennius, Bæda's Life of St. Cuthbert, and the Anglo-Saxon Chronicle. It is possible to fix the date and the circumstances of the conquest of Southern Lancashire with considerable accuracy, and to make out the latest possible time at which any part of the county was under Welsh, and not English rule, or in other words, was within the boundary of Wales and not of England. To examine these points properly we must see what relation existed between the English on the one hand and the Brit-Welsh on the other.

In the year 449, the three ships which contained Hengist and his warriors landed at Ebbsfleet in Thanet, and the first English colony was founded among the descendants of the Roman provincials, who were known to the strangers as Brit-Welsh. From that time a steady immigration of Angle, Jute, and Frisian set in towards our eastern coast, as far north as the Firth of Forth, until in the first half of the 6th century the whole of the eastern part of our island was occupied by various tribes, whose names for the most part still survive in the names of our counties. The principal rivers also offered them a free passage into the heart of the country, and the kingdom of Mercia gradually expanded from the banks of the Trent until it reached as far as the line of the Severn. The river Humber afforded a base of operations for the Anglian freebooters who founded the kingdom of Deira, or modern Yorkshire, while the rock of Bamborough was the centre from which Ida, who landed with 50 ships in the year 547, conquered Bernicia, or the region extending from the river Tees to Edinburgh. The tide of English colonization rolled steadily westward until at the close of the 6th century the Pennine chain, or the stretch of hills, heath, and forest extending southwards from Cumberland and Westmoreland, through Yorkshire and Derbyshire, as far as the line of the

Trent, formed a barrier between the English and Brit-Welsh peoples. The Brit-Welsh still held their ground as far to the east as the district round Leeds, which constituted the kingdom of Elmet, while the kingdom of Strathclyde extended from Chester as far north as the valley of the Clyde.* The point which immediately concerns us is the time when that portion of the latter kingdom which comprises southern Lancashire fell under the sway of the English.

The two kingdoms of Deira and Bernicia had united to form the powerful state of Northumbria at the beginning of the 7th century, under the greatest of her warriors, Æthelfrith. In the year 607 Æthelfrith advanced along the line of the Trent through Staffordshire, avoiding by that route the difficult country of Derbyshire and east Lancashire, and struck at Chester, which was the principal seat of the Brit-Welsh power in this district.† There he fought the famous battle by which the power of Strathclyde was broken, and that is celebrated in song for the death of the monks of Bangor who fought against him with their prayers. By this decisive blow the English first set foot on the coast of the Irish Channel, and Strathclyde and Elmet on the one hand were cut asunder from Wales on the other. Chester was so thoroughly destroyed that it remained desolate for two centuries, until it was restored by Æthelred and Æthelflæd, the Lady of the Mercians, and the plains of Lancashire lay open to the invader. In all probability south Lancashire was occupied by the English at this time, and the nature of the occupation may be gathered from the treatment of the city of Chester. A fire, to use the metaphor of Gildas, went through the land, and the Brit-Welsh inhabitants were either put to the sword or compelled to become the bondsmen of the conquerors. It is impossible to believe that the

* See Freeman, Norman Conquest, vol. i., p. 35—map of Britain in 597. In this map Elmet is placed in Deira, although it did not pass away from the Brit-Welsh till 616 according to Nennius and the Annales Cambriæ.

† Bæda Eccles. Hist. Lib. II. c. ii. Anglo-Saxon Chronicle, A.D. 605–607.

Brit-Welsh of Strathclyde, after such a defeat as that at Chester, could have maintained any position in the plains of Lancashire. The hilly districts, however, of the middle and northern portions of the county, would offer positions from which a defence might be successfully maintained. We may therefore infer that the boundary of the English dominion in Lancashire, after the fall of Chester, was marked by the line of hills extending from Bury and sweeping round to join those in the neighbourhood of Oldham and the axis of the Pennine chain.

This western advance of the Northumbrians was completed by the conquest of Elmet in 616,* by Eadwine, the successor of Æthelfrith, and in all probability then, or about that time, not merely the valley of the Aire, but also Ribblesdale and the hills of Derbyshire and the district extending between Elmet and Chester became subject to Northumbria.

The remaining fragment of Strathclyde in the north still unconquered, embracing Cumberland and Westmoreland, was finally subdued by Ecfrith, about the years 670—685,† and with its fall the whole of this county was absorbed into the Northumbrian kingdom. A passage in the Anglo-Saxon Chronicle under the year 923 proves that the south Lancashire was called Northumbria. "In this year after harvest King Eadward went with his forces to Thelwal and commanded the 'burh' to be built and occupied and manned, and commanded another force also of Mercians, the while he sate there to take possession of Manchester (Mameceaster) in North-Humbria, and repair and man it." This passage is of particular interest, because it presents us with the first notice of Manchester that is to be found in any English record. At that time it was clearly not so important as the town of Thelwal near Warrington.

From these notices it may fairly be concluded that south

* Nennius, c. 66, circa 616, 633 A.D. Annales Cambriæ, A.D. 616.

† Bæda, Vita St. Cuthbert, c. 37. For this notice I have to thank the Rev. J. R. Green.

Lancashire was occupied by the Northumbrians immediately after the battle of Chester, and that the Northumbrian dominion embraced mid-Lancashire shortly after the fall of Elmet, and finally that the Welsh occupying the more northern portions were subdued about the years 670–685 A.D. And it must be remarked that the cause of the Celtic population of Strathclyde remaining to this day in the portions latest conquered, in Cumberland and the south-west of Scotland, while it has disappeared from south Lancashire, is due to the change in the religion of the conquerors on the interval between the two conquests. When the battle of Chester laid south Lancashire at the feet of Æthelfrith, the English were worshippers of Thor and Odin. When Carlisle was taken by Ecfrith, they were Christians warring against men of their own faith. In the one case the war was one of extermination, in the other merely of conquest.

"On some Human Bones found at Buttington, Montgomeryshire," by W. Boyd Dawkins, F.R.S.

Among some papers which have lately demanded my attention, there is one relating to the discovery of human bones in Buttington Church-yard, a hamlet near Welshpool, Montgomeryshire, which is worthy of being placed on record, and being brought into relation with history. In the year 1838 the late Rev. R. Dawkins, the incumbent of the parish, made a most remarkable discovery of human remains while digging the foundations for a new schoolroom at the south-west corner of the church-yard, and in making a path leading from it to the church door. He discovered three pits, one containing two hundred skulls, and two others containing exactly one hundred each; the sides of the pits being lined with the long bones of the arms and the legs. Two other pits contained the smaller bones, such as the vertebræ and those of the extremities. All the teeth were wonderfully perfect, and the condition of the skulls showed that the men to whom they belonged had perished in the full vigour of manhood. Some of the skulls had been fractured, and the men to whom they belonged had evidently come to a violent death. A jaw bone of a horse and some teeth were found in one of the pits, and among the circumstances noted at the time was the fact that the root of an ash tree, growing in the church-yard, had found its way through the nutrient foramen of a thigh-bone, into the cavity which contained the marrow, and had grown until it penetrated the further end of the bone, and finally burst the shaft: the bone and root were compacted together into one solid mass. These remains were unfortunately collected together and reinterred on the north side of the church-yard, without being examined by any one interested in craniology, the few fragments which escaped reinterment being merely the teeth, which were sold at sixpence and a shilling apiece by the workmen, as a remedy against tooth-ache; for the possession of a dead man's tooth was supposed, by the people in the neighbourhood at that time, to prevent that malady.

The interest in this discovery died away, and, so far as I know, there was no attempt made to bring it into relation with history, although it offers a striking proof of the accuracy of the Anglo-Saxon Chronicle. In the year 894 we read that the Danes, probably under the command of Hæsten, left Beamfleet, or Benfleet, in Essex, and, after plundering Mercia or central England, collected their forces at Shoebury in Essex, and gathered together an army both from the East Anglians and the Northumbrians. "They then went up along the Thames till they reached the Severn; then up along the Severn. Then Ethered the ealdorman, and Æthelnoth the ealdorman, and the Kings-thanes who were then at home in the fortified places, gathered forces from every town east of the Parret, and as well west as east of Selwood, and also north of the Thames and west of the

Severn, and also some part of the North-Welsh people. When they had all drawn together then they came up with the army at Buttington on the bank of the Severn, and there beset them about, on either side, in a fastness. When they had now sat there many weeks on both sides of the river, and the King was in the west in Devon, against the fleet, then were the enemy distressed for want of food, and having eaten a great part of their horses, the others being starved with hunger, then went they out against the men who were encamped on the east bank of the river and fought against them, and the Christians had the victory. And Ordheh a kings-thane was there slain; and of the Danish men there was very great slaughter made, and that part which got away thence was saved by flight. When they had come into Essex to their fortress and the ships, then the survivors again gathered a great army from among the East-Angles and the North-Humbrians before winter, and committed their wives and their wealth and their ships to the East-Angles, and went at one stretch, day and night, until they arrived at a western city in Wirral, which is called Legaceaster (Chester).

It is evident from this passage that a most desperate battle was fought at Buttington, between the Danes and the combined English and Welsh forces. And when we consider the position of the church-yard, which is slightly above the level of the fields on the east side, and which stands out boldly above the stretch of alluvium on the north side, there can be but little doubt that the battle was fought on the very spot where the bones were discovered. In the Chronicle we read that the Danes were compelled to eat their horses. The jaw of a horse was discovered in the excavations, together with many horse's teeth. It is therefore almost certain that these human remains belong to the men who fell in this battle. We cannot tell who arranged the bones in the way in which they were found; nor do we know whether they belonged to Danes, English, or Welsh, but it is hardly probable that the victors would knowingly give Christian burial to their heathen adversaries. The commanding position offered by the camp caused it to be chosen by the monks of the neighbouring Abbey of Strata Marcella for the site of the present church, and it is very probable that they discovered the relics of the battle, and arranged them in the pits in the church-yard, after the same fashion as is seen in many crypts and catacombs.

There is another point of interest in this passage of the Chronicle. Buttington is said to be on the east bank of the Severn. Since that time the river course has passed to the westward, at a distance of about a quarter of a mile. Its ancient course however is still marked by a small brook running close under the churchyard, and which finds its way into the Severn by "the main ditch." In connexion with this I may remark that Col. Lane Fox and myself, when examining Offa's dyke in the year 1869, lost all trace of it in passing from Forden northwards, when we arrived at this stream. The Severn, flowing at that time close to Buttington Church, would form a natural barrier between the Mercians and the Welsh, and render the erection of a dyke unnecessary. There is no material fact added to this account in the Chronicle of Ethelwerd, or in that of Florence of Worcester, or Henry of Huntingdon.

It is quite possible to trace at the present time the boundaries of the Danish camp. It was defended on the north-west by the river Severn; on the east by a rampart running parallel, or nearly so, with the road to Forden; on the north-east by the church-yard wall; and on the south by the depression which runs down from the present line of the Forden road behind the Vicarage garden down to what was then the old course of the Severn. It may also have included the site of the out-buildings, opposite to the Green Dragon Inn.

"On the Electrical Properties of Clouds and the Phenomena of Thunder Storms," by Professor OSBORNE REYNOLDS, M.A.

The object of this paper is to point out the three following propositions respecting the behaviour of clouds under conditions of electrical induction, and to suggest an explanation of thunder storms based on these propositions and on the assumption that the *sun is in the condition of a body charged with negative electricity*: an assumption which I have already made in order to explain the Solar Corona, Comets' Tails, and Terrestrial Magnetism.

1. A cloud floating in *dry* air forms an insulated electrical conductor.

2. When such a cloud is *first* formed it will not be charged with electricity but will be ready to receive a charge from any excited body to which it is near enough.

3. When a cloud charged with electricity is *diminished* by evaporation, the tension of its charge will increase until it finds relief.

I do not imagine that the truth of these propositions will be questioned, but rather, that they will be treated as self evident. However, as a matter of interest I have made some experiments to prove their truth, in which I have been more or less successful.

Experiment 1 was to shew that a cloud in dry air acts the part of an insulated conductor. The steam from a vessel of hot water was allowed to rise past a conductor, the apparatus being in front of a large fire, so that the air was very dry. When the conductor was charged the column of vapour was deflected from the vertical to the conductor both for a positive and negative charge.

Experiment 2 was made with the same object as Experiment 1. A gold leaf electrometer was charged so that the leaves stood open and then a cloud made to pass by the insulated leaves. As the cloud passed they were both attracted.

This experiment was attended with considerable difficulty, as the moisture from the steam seemed to get on to the glass shade over the gold leaves and so form a charged conductor between the leaves and cloud. The cloud was first formed by a jet of steam from a pipe, then by the vapour from a vessel of boiling water, and lastly by a smoke ring or rather a steam ring. By this latter method an *insulated* cloud was formed, which, as it passed was attracted by the charged leaf.

Of the two latter propositions I have not been able to obtain any experimental proof. I made an attempt, but failed, through the bursting of the vessel in which the cloud was to be formed. I hope, however, shortly to be able to renew the attempt, and in the meantime I will take it for granted that these propositions are true. Faraday maintained that evaporation was not attended by electrical separation unless the vapour was driven against some solid when the friction of the particles of water gave rise to electricity. So that unless there were some free electricity in the steam or vapour before it was condensed none could be produced by the condensation, and hence the cloud when formed would be uncharged.

In the same way with regard to evaporation, unless, as is very improbable, the steam into which the water is turned retains the electricity which was previously in the condensed vapour; the electricity from that part of the cloud which evaporates must be left to increase the tension of the remainder. So that, as a charged cloud is diminished by evaporation the tension of the charge will increase, although the charge remains the same.

I will now point out what I think to be the bearing which these propositions have on the explanation of thunder storms. In doing this, I am met with a great difficulty, namely, ignorance of what actually goes on in a thunder storm. We seem to have no knowledge of any laws relating to these every-day phenomena; in fact we are where Franklin left us—we know that lightning is electricity and that is all. It is not, I think, decided whether the storm is incidental on the electrical disturbance or *vice versa, i.e.*, whether the electricity causes the clouds and storm or is a mere attendant on them. Nor can I ascertain that there is any certain information as to whether, when the discharge is between the earth and the clouds, the clouds are positive and the earth negative, or *vice versa*. Such information as I can get appears to point out the following law: that in the case of a fresh-formed storm, the cloud is negative and the earth positive; whereas, in other cases, the cloud is positive and the earth negative.

Again, thunder storms move without wind or independently of wind; but I am not aware whether any law connecting this motion with the time of day, &c., has ever been observed, though it seems natural that however complicated by wind and other circumstance, some such law must exist. In this state of ignorance of what the phenomena of thunder really are it is no good attempting to explain them. What I shall do, therefore, is to shew how the inductive action of the *Sun* would necessarily cause certain clouds to be thunder clouds in a manner closely resembling, and for all we know identical with, actual thunder storms.

In doing this I assume that the thunder is only an attendant on the storm and not the cause of it; and that many of the phenomena such as forked and sheet lightning are the result of different states of dampness of the air and different densities in the clouds, and really indicate nothing as to the cause of electricity. In the same way, the periodicity of the storms is referred to the periodical recurrence of certain states of dryness in the atmosphere. Thus the fact that there is no thunder in winter is assumed to be owing to the dampness of the air which allows the electricity to pass from and to the clouds quietly. What I wish to do is to explain the cause of a cloud being at certain times in a different state of electric excitation to the earth and other clouds, and of this difference being sometimes on the positive side and sometimes on the negative, that is to say, why a cloud should sometimes appear to us on the earth to be positively charged, sometimes negatively, and at others not to be charged at all.

The assumed condition of the sun and earth may be represented by two conductors S and E acting on one another by induction, the sun being negative and the earth positive. The distance between these bodies is so great that the inductive action would not be confined to those parts which are opposed, but would in a greater or less degree extend all over their surfaces, though it would still be greater on that side of E which is opposite to S than on the other side.

The conductor E must be surrounded by an imperfectly insulating medium to represent damp air. The formation of a cloud may then be represented by the introduction of a conductor C near to the surface of E. Such a conductor at first having no charge would attract the positive electricity in E and appear by reference to E to be negatively charged. If it was near enough to E, a spark would at once pass, which would represent a flash of forked lightning. If it were not near enough for this it would obtain a charge through the imperfect insulation of the medium. Such a charge might pass quietly or by the electric brush. When the cloud had obtained a charge it would not exert any influence on the earth, unless it altered its position. But if the heat of the sun caused part of the cloud to evaporate the remainder would be surcharged and appear positive. Or if C approached E then C would be overcharged, and a part of its electricity would return, and on its return it might cause positive lightning. Thus, suppose that after a cloud had obtained its charge part of it came down suddenly in the form of rain. As the rain came lower its electric tension would increase until it got near enough the ground to relieve itself with a flash of lightning, almost immediately after which the first rain would reach the ground. It has often been noticed that something like this often takes place; it often begins to pour immediately after a flash of lightning, so much so that it seems that the electricity had been holding the rain up and it was only after the discharge that it could fall. This, however, cannot be the case, for the rain often follows so quickly after the flash that there would not have been time for it to fall from the cloud unless it had started before the discharge took place. If on the other hand C receded from E, it would again be in a position to accept more electricity, or would again become negative. In this way, a cloud in forming, or when first formed, would appear negatively charged; soon after it would become neutral, and then if it moved to or from the earth it would appear positively or negatively charged.

If the air was very dry, as it is in the summer, any exchange of electricity between the earth and the cloud would cause forked lightning, in the winter it would take place quietly, by the conduction of the moist atmosphere.

In this way then there would sometimes be positive, sometimes negative lightning; sometimes the discharge would be a forked flash or spark, sometimes a brush or sheet lightning. And if clouds are formed in several layers, as would be represented by another conductor D outside C, then in addition to the phenomena already mentioned, similar phenomena would take place between C and D; and if in addition to this we were to assume that there are other clouds in the neighbourhood, the phenomena might be complicated to any extent.

And if, further, the motion of the sun is taken into account; as the conductor S moves round E the charges in D and E would vary, accordingly as they were more or less between S and E and directly under the induction of S; *i.e.*, the charge in a cloud would appear to change owing to the motion of the sun; thus a cloud that appeared neutral at midday would, if it did not receive or give off any electricity, become charged positively in the evening.

With regard to the independent motion of the clouds, there are several causes which would effect it. For instance, a cloud whether it appeared on the earth to be negatively or positively charged would always tend to follow the sun, though it is possible this tendency might be very slight. Again, one cloud would attract or repel another, according as they were charged with the opposite or the same electricities; And in the same way a cloud would be attracted or repelled by a hill, according to the nature of their respective charges.

Such, then, would be some of the more apparent phenomena under the assumed conditions. So far as I can see they agree well with the general appearance of what actually takes place, but as I have previously said, the laws relating to thunder storms are not sufficiently known to warrant me in doing more than suggesting this as a probable explanation.

In these remarks I have said nothing whatever about what is called atmospheric electricity, or the apparent increase of positive tension as we proceed away from the surface of the earth. I do not think that this has much to do with thunder storms. If the law is established it seems to me that it will require some explanation, besides merely that of the solar induction acting through the earth's atmosphere on to the surface of the earth. It would rather imply that the sun acts on some electricity in the higher regions of the earth's atmosphere, and that electricity in these regions acts again on the surface of the earth; but, however this may be, the effect of the assumptions described in this paper would be much the same.

Ordinary Meeting, December 24th, 1872.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

The President drew attention to the increasing number of cases of hydrophobia. There was every reason for believing that this dreadful disorder was communicated from one animal to another by a bite, and seldom if ever was spontaneously developed. Inasmuch therefore as the effects of a bite nearly always occurred within four months, it would only be necessary to isolate all dogs for that period in order to stamp out the disease. That was the opinion of Dr. Bardsley, whose elaborate paper will be found in the 4th volume of the Memoirs of the Society, and probably gave rise to the practice of confining dogs at certain periods of the year, which has unfortunately been rendered to a great extent nugatory in consequence of having been only partially adopted.

Ordinary Meeting, January 7th, 1873.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. Julius Allmann was elected an Ordinary Member of the Society.

The President referred to the great loss which the Society had experienced by the death of one of its most distinguished Honorary Members. Dr. Rankine was one of the earliest investigators of the dynamical theory of heat, and contributed eminently in the work of bringing that theory to its present advanced condition. Besides this, he was perhaps more successful than any other man in applying his own discoveries, and those of his fellow labourers in abstract science, to practical use. His treatises on the Steam Engine and other Prime Movers, Applied Mechanics, Machinery, &c., form what may justly be termed an Encyclopædia of Civil Engineering. Called away in the prime of life, his loss is one of the most severe that could have befallen science.

Mr. William H. Johnson, B.Sc., called attention to the action of sulphuric and hydrochloric acids on iron and steel.

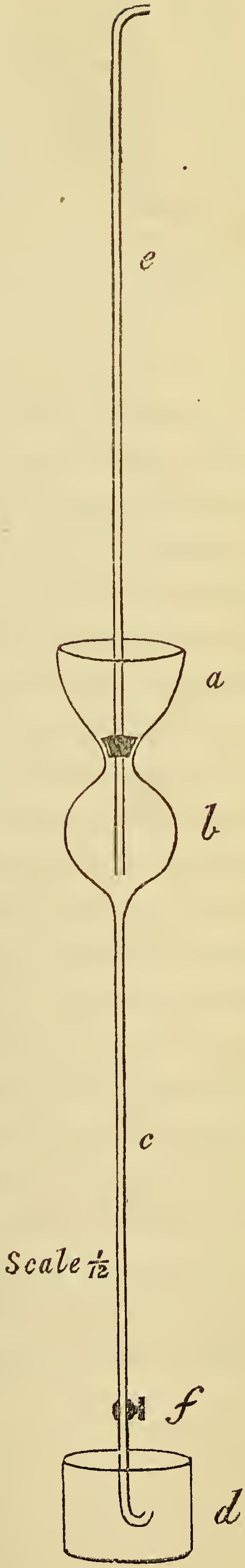
If after immersion for say ten minutes in either of these acids a piece of iron or steel be tested, its tensile strength and resistance to torsion will be found to have diminished. Exposure to the air for several days or gentle heat will however completely restore its original strength. On breaking a piece of iron wire after immersion in sulphuric acid and gently moistening the fracture with the tip of the tongue, bubbles of gas arise causing the wetted portion to appear to boil. The most careful washing and coating with lime after being dipped in the acid, and even its subsequent drawing, in which process it is reduced in diameter by passage through a die, does not interfere with either of these phenomena; which only gradually disappear by exposure to the air, or more quickly by gentle heat.

Prolonged immersion in acid has a tendency to produce a crystalline structure in even the best wrought iron.

Ordinary Meeting, January 21st, 1873.

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

The PRESIDENT explained a simple apparatus by means of which a very high degree of rarefaction of air could be produced with much facility, and which might in some circumstances be found preferable to the common air-pump or even the Sprengel. It consists of a glass funnel *a* surmounting a globe *b*, from the lower part of which a tube *c* descends to a jar of mercury *d*. The tube *e*, in connexion with the receiver to be exhausted, is furnished with a vulcanised indiarubber plug which fits into the neck of the funnel. In using the apparatus the stopcock *f* is shut and the funnel filled with mercury. Then by lifting the tube *e* with its plug, the mercury fills the globe *b* and the pipe *c*. The tube *e* is then replaced, and the stopcock being opened, the mercury descends in *c* emptying the globe. By returning the mercury into the funnel by means of a pump, or more simply, by lifting the jar *d*, the process is repeated until the requisite degree of rarefaction is produced.



E. W. Binney, V.P., F.R.S., stated that during the last session he had exhibited specimens of *Zygopteris* and *Stauropteris* found in the lower coal measures of Lancashire, short notices of which appeared in the Proceedings of the 9th January and the 20th February, 1872. He now brought some drawings of other specimens of petioles from the same localities, which appeared to belong to the genus *Anachoropteris*. One of them given to him by his friend Mr. Whitaker of Watersheddings, Oldham, was closely allied to *Anachoropteris Decaisnii* of Renault. It was of an oval form, measuring half an inch across its major and four tenths of an inch across its minor axis.

Another singular fossil was from his own cabinet, and procured from the Lower Brooksbottom seam of coal. It was of a circular form and about one tenth of an inch in diameter. Its central axis was bounded by three crescent-shaped lines which joined together, and at their points of junction proceeded in three rays, which at their extremities diverged in numerous curved lines towards the circumference. These rays bore some resemblance to the five rays in an *Anachoropteris* figured by Renault in plate 10, fig. 2 of tome xii. of the Annales des Sciences Naturelles, but in the place of being embedded in cellular tissue as in the French specimen, they appeared to traverse a mass of reticulated tissue arranged in a series of curved lines so as to appear like three quadrants arranged within a circle with the central axis in the form of a spherical triangle in the midst of them. It is nearly impossible to describe the fossil without the aid of a figure. He considered that it would have to be placed in a new genus, and he had already found five or six different species.

Ordinary Meeting, February 4th, 1873.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

E. W. BINNEY, V.P., F.R.S., said that the Society had lost one of its most illustrious Honorary Members by the death of the Rev. Adam Sedgwick, F.R.S., Woodwardian Professor of Geology in the University of Cambridge, a great and good man, whose loss it will be hard to replace. All who had the pleasure of his acquaintance have to deplore the removal of one of the kindest and heartiest of friends, as well as one of the most eminent geologists of this century. His published papers in the Royal Society's Catalogue, sole and joint, amount to 58. The part of his labours which I have been best acquainted with are the memoirs on the Magnesium Limestone and Lower Portions of the New Red Sandstone now known as Permian strata in the North of England. For patient research and sound conclusions they are models for all future workers in the same field. Never was a more generous or willing friend to the humble worker in science. Many years since, on the death of that excellent naturalist the late Samuel Gibson, of Hebden Bridge, blacksmith, the deceased Professor with other friends, lent a ready hand in raising a fund for the widow and family. During a long illness poor Gibson had been compelled to part with his collection of British insects in thirty-four cases to a neighbour for as many shillings. In order to make as much money as possible by a sale of what was left of his things, the purchaser of the insects was asked to return them on repayment of what he had paid. After a lengthened correspondence the matter was referred to Professor Sedgwick, who settled it by writing the following letter, which by its tact and conciliatory language proved quite effectual:

Norwich, June 25, 1849.

My Dear Sir,

I am extremely sorry that you have appealed to me about the disposal of poor Mr. Gibson's insects, especially as I am at this moment confined to my bed by illness. It pains me to write while propped up in bed, as I feel so much lassitude that I cannot long attend to anything. Surely no blame, in the first instance, attaches to the Rev. Mr. ——. You are bound to accept his statement without any reserve, viz., "That he was not desirous of obtaining the insects, but having been applied to, and thinking that purchasing them might be a little benefit to Gibson's family, he did so, giving the amount that was required." I am truly sorry that you have not written to the Rev. Mr. —— with a little more caution, for he has, not unnaturally, taken offence at an expression in your letter of June 4th. The case is a very plain one, he and you are both anxious for the benefit of poor Gibson's family. He appears not to have had any idea of the value of the collection, and if he resolve to keep it he would not surely object to the valuation of some good entomologist. Between the amount of such a valuation and the sums he has already advanced he would not, I should think hesitate to pay the difference to Mr. Gibson's family. If this plan be not adopted I think the value of the collection should be ascertained in the way you propose, by public auction at Manchester, or by any method that promises to raise the largest sum for the widow and children. I must, in conclusion, say that I do not by any means approve of the plan of making up to the family for the loss of the insects by occasional acts of pecuniary help. They appear to have parted with the collection under the pressure of dire necessity, and this should not be turned against them. I write with pain and labour, and fear I hardly make myself understood.

Very truly yours,

A. SEDGWICK.

E. W. Binney, Manchester.

The insects, when sold by the late Mr. Capes, at his auction rooms in Manchester, realized the sum of £44 10s., and are now in the Peel Park Museum, Salford. Altogether nearly £150 was obtained for the widow. The last letter I received from the Professor was in the past summer, when he presented to the Society photographic portraits of himself and his old friend the late Mr. Dawson, the mathematician of Sedbergh, which are placed in our meeting room. In the early days of the British Association he was probably the most eloquent and humorous speaker amongst its members, and few who had the pleasure of listening to his reply to Dean Cockburn in the Geological Section at York will ever forget it.

Professor WILLIAMSON, F.R.S., stated that the second fossil plant described by Mr. Binney at the last meeting of the Society, on January 21st, and of which a notice appeared in the Society's Proceedings, does not belong to some new genus, as Mr. Binney supposed, but is one that he has already described on two or three occasions as being the stem or branch of the well-known genus *Asterophyllites*, In his description of the *Volkmannia Binneyi*, published in the Society's Transactions in 1871, respecting which Professor Williamson showed that it possessed a vascular axis exhibiting a triquetrous transverse section, the author gave his reasons for believing that the strobilus was the fruit of *Asterophyllites*. In a letter addressed to Dr. Sharpey on Nov. 16, 1871, and published in No. 131 of that Society's Proceedings, Professor Williamson gave a brief description of a stem having a similar triangular vascular axis, with lenticularly thickened nodes, and which he again referred to the same verticellate leaved genus. In a second letter to Dr. Sharpey, dated May 3, 1872, the author confirmed the above conclusions by stating that he had "got an additional number of exquisite examples showing not only the nodes but verticils of the linear leaves so characteristic of the plant. These specimens place the correctness of my previous inference beyond all possibility of doubt, and finally settle the point that asterophyllites is not the branch and foliage of a calamite, but an altogether distinct type of vegetation having an organisation peculiarly its own." The author said that he had obtained the plant in almost every stage of its growth, from the youngest twig to the more matured stem, and that the genus would be the subject of his next, or fifth, of the series of memoirs now in course of publication by the Royal Society.

"On a large Meteor seen on February 3, 1873, at 10 p.m.," by Professor OSBORNE REYNOLDS, M.A.

On the 3rd of February (that is yesterday), at 10h. 7m. (as afterwards appeared) by my watch (which was 7 minutes fast), I was walking from Manchester along the east side of the Oxford Road (which there runs 30° to the east of south), I had just reached the corner of Grafton-street, when I saw a most brilliant meteor. I first became aware of it from the brightness of the wall on my left, *i.e.*, on the north-east, which caused me to turn my head in that, the wrong, direction; the first effect was that of a flash of lightning, but it continued and increased until it was equal to daylight. On lifting my head I saw directly in front of me, what had previously been hidden by the brim of my hat, a bright object, apparently fixed in the sky, as though it were coming directly towards me; immediately afterwards it turned to the west, and passed just under the moon (which it completely out-shone). I was very much startled when I first caught sight of it, owing doubtless to the rapidity with which it was increasing in size, and the directness with which it seemed to be coming. The next instant I saw that it was only an extraordinary meteor. It passed the moon, falling at an angle of I should say 20°, and then ceased suddenly, having traversed a path of about 90°, from the south to the east. The colour of the light was that of a blue-light, or rather burning magnesium. The sky was cloudy, but there was no appearance of redness about either the head or the train. I endeavoured to fix its course by the stars, but it was too cloudy, although I could see here and there a star. The conclusions I came to, there and then, were that its course must have been nearly parallel with the road, which by the map runs, at that point, 30° to the west of north; that when I first saw it it was about 40° above the horizon and due south; and that it passed about 20° to the north of the moon. (This would make its line of approach from Pegasus.) While I was thinking of its course I heard a report, not very loud, but which I connected with it. I judged it was about 30″ after the display. I then looked at my watch, it was 10h. 7m. I then walked along, talking to a fellow-traveller who had not quite recovered his alarm. Presently we heard a loud report, like a short peal of thunder or the firing of a large cannon; I immediately looked at my watch, it was then 10h. 10m., so that this second report was from three to four minutes after the display. I have no doubt that this was the report of the meteor, for compared with the other it was like the firing of a cannon to a musket. The time of the second report would make the distance 30 or 40 miles, so that it would have passed over Chester and burst over Liverpool. In this case it must have been a tremendous affair, for the sky was cloudy, and I do not think I exaggerate when I say that at one instant it was as light as day; the train was very long and the speed great. It ceased suddenly, as when a ball from a Roman candle falls into water; there were no fragments, as from an explosion.

"Note on Meta-Vanadic Acid," by Dr. B. W. GERLAND. Communicated by Professor ROSCOE, F.R.S.

A solution of copper vanadiate in aqueous sulphurous acid, after part of the latter is removed by boiling, deposits brilliant yellow crystals, the description and analysis of which I gave in the *Journ. of Pract. Chem.*, 1871, page 97. These crystals are quite uniform in appearance and contain cupric oxide, vanadic acid, and sulphurous acid. They rapidly change under the influence of air, their beautiful metallic lustre soon disappears, and the colour becomes a dark green. Although formed in a solution of sulphurous acid, they nevertheless decompose when treated, after separation from their mother liquor, with fresh sulphurous acid, so that two kinds of crystals, brown and orange yellow, now appear mixed together. An excess of sulphurous acid dissolves the the former and leaves the latter intact. After filtration, washing, and drying, they form microscopic scales of beautiful lustre and a deep yellow orange colour; they are free from copper and sulphur, and perfectly unalterable in the air. Heated to 100° C. and even to 130°, they lose no weight, but at a low red heat water is given off, and the residuum consists of vanadium pentoxide, which fuses and crystallizes after cooling.

The composition of the substance, previously dried over vitriol, is according to analysis the following:

Water (loss by heating)	8.73
Vanadium pentoxide	91.06
Impurities	0.21
	100.00

These numbers correspond to the formula of the meta-vanadic acid VHO_3, which requires—

Water	8.97
Vanadic pentoxide..........................	91.03
	100.00

In some instances I obtained the same bronze or gold-like substance by treating copper vanadiate suspended in water with sulphurous acid gas, and in many others the effect of the gas was formation of vanadic oxide in solution. I intend to elucidate this point by further experiments.

The copper vanadiate was prepared by precipitation of ammonium vanadiate with copper sulphate. The mother liquor contained both copper and vanadic acid. After evaporation the latter is found in the residue as meta-vanadic acid, with the same metallic appearance as that just described, and can be obtained by washing with water. The crystals obstinately retain copper, sometimes as much as 12 per cent, which is best removed by repeated treatment with aqueous sulphurous acid. A sample of the substance so prepared was analysed by Professor Roscoe with the following results :

Weight of substance taken	0.4505 gram.
Loss on ignition	0.0411 ,,

Hence the per centage composition is found to be

Water	9.12
Venadium pentoxide	90.88
	100.00

The samples of vanadium bronze obtained by these three different methods had the same composition, the same appearance, and the same chemical properties. It is essentially distinguished from the amorphous brick-red hydrated vanadic acid by its indifference to reagents. Sulphurous acid scarcely acts on it, neither does ammonia, and even a solution of sodium carbonate dissolves it only after very long continued boiling. In the air it is perfectly permanent. It is very probable that this meta-vanadic acid will become a favorite bronze, valued even higher than gold.

I trust that at some future time I shall be able to render a more satisfactory account of this interesting substance, and particularly of its formation.

Macclesfield, January, 1873.

Dr. William Roberts exhibited some preparations and experiments bearing on the question of biogenesis. He stated that in the last two and half years he had performed over 300 experiments. His results supported the conclusion that the fungi, monads, and bacteria which make their appearance in boiled organic mixtures are not due to spontaneous evolution, but arise exclusively under the influence of pre-existing germs or ferments introduced from without. His method of experimenting consisted chiefly in exposing organic solutions and mixtures to a boiling heat in glass flasks whose necks had been previously tightly plugged with cotton wool. Two modifications of the experiment were adopted.

I. In the first modification a 4-ounce flask was employed, and the heat applied directly by means of a gas flame.

II. In the second modification—after the introduction of the materials to be operated on— the elongated neck of the flask was sealed hermetically by the blowpipe above the plug of cotton wool; the flask was then weighted with a collar of lead and immersed in a large can of water; the can was then put on the fire and the water boiled for 20 or 30 minutes. During the process of boiling the flask was maintained in an upright or semi-upright position, in order to prevent any wetting of the cotton-wool plug by the contents of the flask. When the can was cold the flask was removed and its neck filed off above the cotton wool, so as to permit free ingress and egress of air.

Flasks thus prepared were maintained at a warmth varying from 50° to 90° Fahr. for long periods — many weeks and months — some in the dark and some exposed to the light, with the following results.

I. Simple filtered infusions of animal or vegetable tissues —a very considerable variety were tried—boiled over the flame for five or ten minutes, in flasks previously plugged with cotton wool, remained permanently barren. This result was absolutely invariable.

II. More complex mixtures—milk, neutralized or alkalized infusions of vegetable and animal tissues, similar albuminous and gelatinous solutions, mixtures containing fragments of animal or vegetable substances or cheese — yielded variable results. In none of them did fungoid growths make their appearance — but monads and bacteria frequently appeared in abundance.

This seemingly contradictory result was inferred to be due to the ineffective application of the heat in the process of direct boiling over a flame. It was found that many of these more complex mixtures frothed excessively when boiled — *brisk* ebullition could not therefore be maintained —particles were spurted about on the sides of the flask, and, in this way, apparently escaped effective exposure to the heat. Even when the boiling was prolonged for 20 or 30 minutes the results were still uncertain — sometimes the flasks remained barren — sometimes they became turbid and swarmed with bacteria.

III. By the second modification of the experiment much more constant results were obtained — the flasks remained almost always permanently barren—and the few exceptions were found to be due to some imperfection in the conduct of the experiment. No exceptions occurred with milk, nor with substances, however complex, which were in actual solution, but when considerable pieces of vegetable or animal substances were introduced into the flasks, bacteria and monads with putrefactive changes occasionally made their appearance in abundance. In these exceptional cases, when the experiments were repeated with the pieces finely comminuted, or introduced in some other way more favourable to the diffusion of the heat, the flasks remained permanently barren.

Dr. Roberts called attention to the crucial significance of experiments on this subject made in flasks whose necks are plugged with cotton wool. A plug of cotton wool acts as an absolutely impervious filter to the solid particles of the atmosphere, while it permits a free passage to the gaseous constituents.

When one of these experiments is effectively performed, the fluid or mixture in the flask may be exposed to the full influence of light, of warmth, and of air, and yet it remains permanently barren. As slow evaporation takes place the liquid passes through all grades of concentration, possibly chemical changes of various kinds take place within it, and still no organic growth makes its appearance for months and even years; but if the plug of cotton wool be withdrawn for a few minutes, or a single drop of any natural water, however pure and well filtered, be introduced, then all is changed—in a few days the clear solution becomes turbid from bacteria and monads, or a mass of mildew covers its surface and soon half fills the flask.

In the face of these experiments it was impossible to doubt that the biogenic power of the atmosphere resides in its dust, and not in its gaseous ingredients; but as to the exact nature of that biogenic power—whether it be a specific germ or a ferment—no sufficient evidence has yet been adduced. Dr. Roberts did not find that diminished pressure of the atmosphere, obtained by sealing flasks hermetically in ebullition, after the mode suggested by Dr. Bastian, materially affected the results.

Dr. R. Angus Smith, F.R.S., said that he was glad to see such uniformity of results. His own experiments, which were very numerous on a similar point, were made differently, but were without exception proving the same. As to the name of the substances in the air, he preferred *germ*: it involved no theory. A germ may be considered that which germinates. *Dust* is an equivocal expression, which may cause a popular error. *Polarity* introduces a theory which is so entirely without basis that in our present state of knowledge we may call the inference it presupposes decidedly false.

"P.S. To Dr. Joule's description of a Mercurial Air-pump."

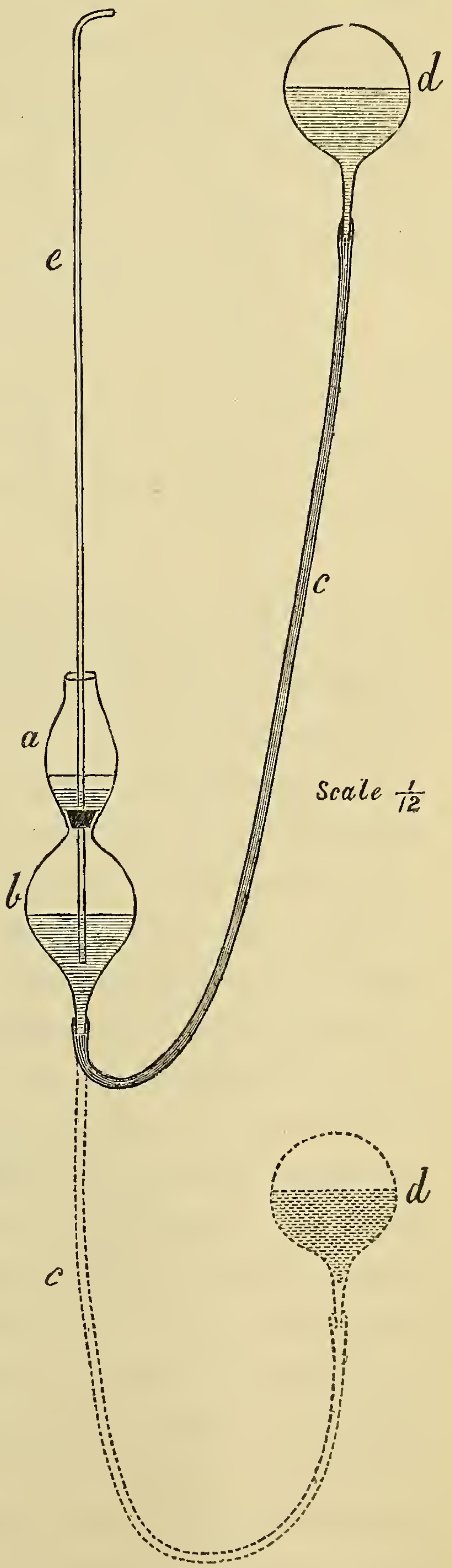
The exhauster described in the last number of the Proceedings has been further improved by dispensing with the glass tube *e*, and its stop-cock *f*. This is effected by attaching the base of the globe *b* to a strengthened indiarubber pipe, connected at the other end to a glass vessel of rather larger capacity than *b*. This vessel has only to be successively raised and lowered in order to exhaust the receiver. The mercury in the vessel may be either under atmospheric pressure or relieved therefrom. In the former case it must be alternately raised and depressed from 30 inches below *b* up to that level. In the latter it must be raised and depressed from the level of *b* to 30 inches above it. Castor oil is a useful medium to prevent the passage of air between mercury and the glass vessels.

It is important to add a little sulphuric acid to the mercury, in order to remove the film of water which adheres to the inside of the globe *b*. On this account it would, perhaps. be desirable to substitute a plug of glass for the indiarubber one between *a* and *b*.

Ordinary Meeting, February 18th, 1873.

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

Dr. JOULE, F.R.S., gave some further account of the improvements he had made in his air exhausting apparatus. As stated in the last Proceedings, he had substituted a caoutchouc tube attached to the neck of a glass vessel, for the original perpendicular pipe with its stop-cock. This is seen in the adjoining sketch *c* and *d*. The two positions, viz. when *b* is being filled, and when it is being emptied, are shown by the full and the dotted drawing. It is convenient to introduce no air into *d* except that required to act as a cushion to avoid a shock when filled in the lower position. Sulphuric acid may be introduced into the receiver to be exhausted, but it is perhaps more convenient to place it over the mercury in *a*, whence it may occasionally be drawn into *b*, to effect the drying of the internal parts of the apparatus. Dr. Joule has met with some difficulty in using mercury gauges to ascertain the residual pressure, inasmuch as



he finds that mercury thoroughly boiled in clean glass tubes does not show a convex surface, but adheres strongly to the glass. However he has confidence in giving the following results in working with his apparatus, with acid of various strength, obtained by successive dilutions of sulphuric acid, of sp. gr. 1.845 by volume.

Sulphuric Acid.		Water.		Pressure in Inches of Mercury.
3	+	0	....	Inappreciable.
3	+	1	....	Inappreciable.
3	+	2	....	0·01 at 70°
1	+	1	....	0·03 at 63°
1	+	2	....	0·15 at 63°
1	+	4	....	0·30 at 55°
0	+	1	....	0·37 at 47°

"Notes on supposed Glacial Action in the Deposition of Hematite Iron Ores in the Furness District," by WILLIAM BROCKBANK, F.G.S.

The hematite iron ore deposits in the Furness district are of two very distinct varieties—(1) Those filling hollows in the limestone, covered only by the post tertiary gravels and clays, and (2) Those occurring in the carboniferous limestone in veins, and large irregular cavities, or "pockets."

The summit of the mining district of Dalton-in-Furness is High Haume, which rises about 508 feet above the level of the sea, and is of Silurian age; Coniston limestone, grits and flags; upon whose flanks rests the carboniferous limestone. The uplifting of this central cone tilted the limestones, so that they dip very quickly towards the S.E., and broke them up into a succession of reefs, the outcrops forming a parallel series of ridges from W. to E., each marked out on the surface by lines of iron ore workings.

The source of the hematite ore appears to have been, here as elsewhere, at or about the junction of the silurian slates with the carboniferous limestone; and it found its way into the fissures and caverns with which the latter abounds, and wherein it is now so largely worked. The surface of the country is remarkable for the absence of brooks on the limestone area, the only two, viz., Powka Beck and Dragley Beck, running along the base of the clay slates. The brooklets elsewhere find their way through the fissures in the limestone and into the curious tarns which dot the surface.

The regular veins (2) are thus pretty easily accounted for, being similar to those of the Whitehaven district.*

The superficial deposits (1) are more especially the subject of the present communication, as they afford, in the writer's opinion, undoubted evidence of glacial action, and of the mode in which the iron ore has been transported by its agency.

John Bolton, the Ulverston geologist, published in his "Geological Fragments" several sections of bore holes and open workings in this neighbourhood, from which the following has been compiled as illustrative of the district. It is not taken from any single example, but adapted from several instances, to show the general aspect of the whole.

	ft.	in.
Soil	2	0
Gravel and clay	4	0
Yellow clay, mixed with iron ore	4	0
Black mould	4	0
Iron ore (dark coloured)	2	0
Black mould, mixed with iron ore	6	0
Iron ore	8	0
Decomposed limestone	7	0
Black woody deposit	12	0
Decomposed limestone	6	0
Black mould and wood	2	0
Yellow clay, mixed with ore	6	0
Black mould, mixed with iron ore	10	0
Black mould	4	0
Black mould, mixed with iron ore and limestone	3	0

* See Proceedings, Dec. 10, 1867, pp. 59—61, and Dec. 1, 1868, pp. 51—56.

Mr. Bolton was unable to give any clue to the manner in which such remarkable sections as the above had obtained.

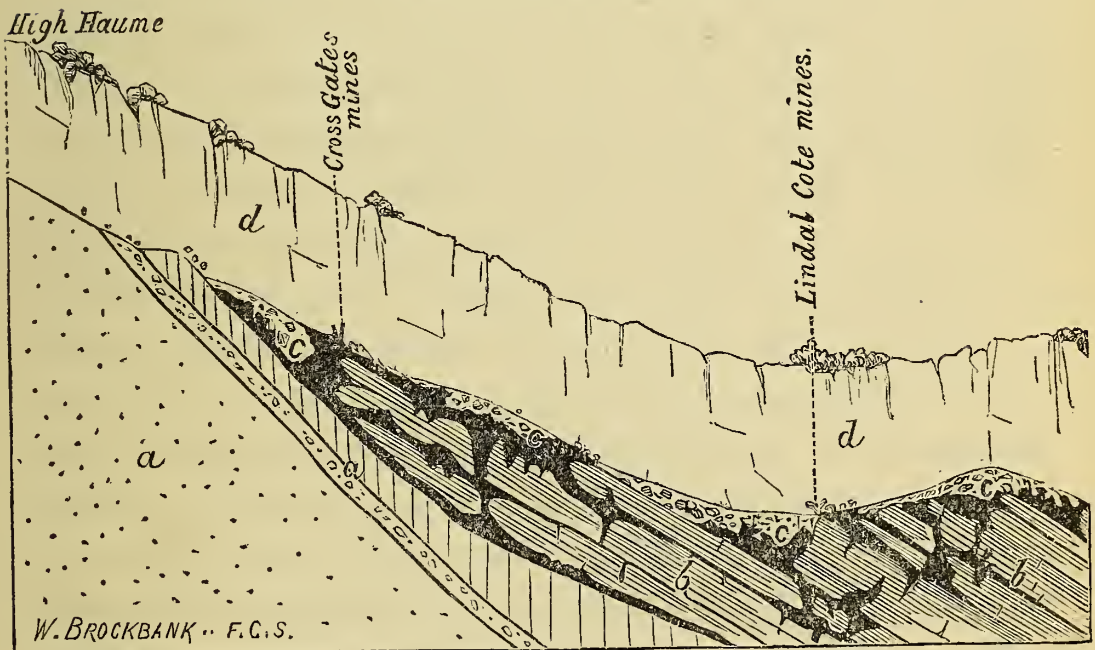
The occurrence of the superficial deposits, as shown in the foregoing section, is, I believe, to be explained by the theory of glacial action, and is evidently a part of the great change wrought upon the surface, by the agency of ice, during the "glacial epoch"; coeval with the boulder drift. The great ice sheet, which then covered all the north of England, descended from the lake mountains, grinding down the surface rocks, and depositing the clays and gravels in its course. The evidence of this is most strikingly displayed in the above section, each line of which apparently marks out a period, and a pause, in its course.

The iron ore occurring in these deposits is of a dark colour, and of much lighter specific gravity than that from the veins of limestone; and it has the appearance of having been all ground to powder. After exposure to atmospheric influence it soon falls again into that state. The clays are of a bright yellow colour, and of exceedingly fine grain, being evidently the "flour of rocks," ground down by the glacier in its passage over the clay-slates. The unfossilized wood is in a remarkable state of preservation, occurring in large fragments, as if it had been rudely broken up and crushed, probably also by the ice. It is principally birch, and some of the trees have been found of 2ft. diameter. In one of the pits there was also a layer of peat, giving evidence of a long period of rest and stagnation.

The iron ore was thus, by glacial agency, transferred from its original place of occurrence, from the outcrop of one reef to another, and redeposited as drift; covered up by clays and the debris of rocks, wherever there was a cavity to receive it. The water resulting from the thaw of the ice would carry the ore down with it into the crevices and caverns of the limestone, where it is now found as soft or "puddling" ore. Aggassiz points out in his work on glaciers that ice does not sink into all the hollows, but frequently bridges over large cavities; and these hollows would be just of such a class as to escape contact with the moving mass above; so that the successive deposits would be preserved from time to time, as the ice passed away and returned.

The following diagram will illustrate the above description, showing the geological structure of the district and the mode of occurrence of the hematite iron ores, and also of the ice covering, by which I suppose the superficial deposits to have been formed.

SECTION NEAR DALTON-IN-FURNESS.



a. Silurian (Coniston Grits and Flags).
b. Carboniferous (Limestone, with Hematite Iron Ore in veins and "pockets").
c. Drift Deposits (Hematite Iron Ore, with Boulder Clay, Wood, and *débris* of older rocks).
d. Supposed Glacier (by which the deposits (*c*) have been formed).

"The Results of the Settle Cave Exploration," by W. Boyd Dawkins, M.A., F.R.S.

Since the results of the exploration of the Settle Caves were brought before the British Association at Liverpool, in 1870, considerable progress has been made in the further investigation of the remarkable contents of the Victoria

Cavern. Up to that time our researches had revealed, perhaps, the most remarkable collection of enamelled jewellery which had ever been discovered in one spot, along with broken bones of animals and the implements of every-day life, which afforded a pointed contrast to the culture implied by the workmanship of the articles of luxury. The Roman coins, and the style of workmanship of the implements, pointed out that the cave was occupied during the troublous times when the Roman Empire was being dismembered by the invading barbarians, and when Britain, stripped of the Roman legions, was falling a prey either to the Picts and Scots on the one hand, or to the Jutes, Angles, and Saxons on the other. If we stretch the limits of the occupation to the latest they cannot be held to extend nearer to our own times than the Northumbrian conquest of Elmet (or Kingdom of Leeds and Bradford) by Eadwine, in the year A.D. 616, that was preceded in 607 by the march of Æthelfrith on Chester, and the great battle near that Roman fort, celebrated in song for the defeat of the British and the slaying of the monks of Bangor. At that time the Northumbrian arms were first seen on the shores of the Irish Channel, and the fragment of Roman Britain—which had extended on the western part of our island, from the estuary of the Severn uninterruptedly, through Derbyshire and Lancashire into Cumberland—was divided, never again to be united. The Roman civilization, which had up to that time been maintained in that district disappeared, and was replaced by the civilization which we know as English. The traces therefore of Romano-Celtic ornaments and implements from the Victoria Cave must be assigned to the period before the English conquest, before the Northumbrians conquered West Yorkshire and Mid-Lancashire.

Underneath the stratum containing the Romano-Celtic or Brit-Welsh articles, at the entrance of the cave, there was a thickness of about six feet of angular stones, and at the bottom of this a bone harpoon or fish-spear, a bone bead, and a few broken bones of bear, red deer, and small short-horned ox prove that in still earlier times the cave had been inhabited by man. A few flint flakes probably imply that these remains are to be referred rather to the Neolithic age than to that of Bronze.

Below this was a layer of stiff clay, into which the committee sank two shafts, respectively of twelve and twenty-five feet deep, without arriving at the bottom. They have, however, at last penetrated it, and have broken into an ossiferous bed, full of the remains of extinct animals, similar to those which have been discovered at Kirkdale and elsewhere; consisting of the cave bear, cave hyæna, woolly rhinoceros, mammoth, bison, reindeer, and horse. The bottom has not been reached, and the area exposed is so small that it is impossible to say whether man was living in the cave at this time or not.

The clay immediately above it is considered, both by Mr. Boyd Dawkins and Mr. Tiddeman, to be of glacial origin, and in that case this cave is the only one in Great Britain which has offered clear proof that this group of animals was living in the country before the glacial age. It may be that the remains of man may be discovered here, as in the caves of Wookey Hole, Kent's Hole, and Brixham; but this problem can only be solved by an exploration on a larger scale, which the committee hope to be able to carry on by the aid of further subscriptions, and which the British Association has thought sufficiently important to aid by a grant of £50. The problem which they are attempting to solve, is not merely of local interest, but one which is worthy of the aid of all who care for the advancement of knowledge.

"The explorations of the Victoria Cave," writes Mr. Tiddeman, "carry with them more than common interest, from the probability of making out in this district the relation of the older cave mammals (and perhaps of man) to the Glacial period. The complete absence of this fauna from the river gravels and other Post-Glacial deposits of this district, taken with the former existence of a great development of ice over the northern counties, renders it highly probable that the latter was the agent which removed their remains from all parts of the country to which it had access, leaving them only in sheltered caves.

"In this cave we find, above the beds containing the older fauna, a deposit of laminated clay of great thickness, differing so much from the cave-earth above and below it as to point to distinct physical conditions for its origin. Clay in all respects similar, but containing scratched stones, has been found intercalated with true glacial beds in the neighbourhood, thus rendering the glacial origin of that in the cave also highly probable.

"Moreover, at the back of a great thickness of talus at the entrance glaciated boulders have been found, resting on the edges of the beds of lower cave-earth containing the older mammals. All points considered, there is strong cumulative evidence pointing to the formation of the lower cave-earth at times at any rate prior to the close of the Glacial period and probably earlier. It is to be hoped that further investigations may settle these and other most important questions."

The objects found in the Victoria Cave will not be removed from the county, but will be placed in a museum attached to the Grammar School at Giggleswick.

Mr. Brockbank, F.G.S., differed from Mr. Dawkins as to the mode in which the "talus" before the Victoria cave, and the earth with which it is filled, were deposited, and consequently as to the basis upon which his estimates of time were based. He believed this cavern had been filled by the agency of running water, which flowed through it in rainy seasons, as is the case in the numerous other similar caves, such as the Ingleborough and Peak caverns. He did not believe that the "talus" had been made up of debris which had entirely fallen from the face of the cliffs, and which would have thus been altogether of limestone "breccia"; but on the contrary that a great part of it had been washed out from the interior of the cave in times of flood, carrying with the earth any loose bones or other light objects which lay in the cave. The proximity of the Craven fault might account for the presence of Silurian rocks in the debris, without the necessity of supposing glacial action for their conveyance. He did not consider it possible for the cavern to have been filled with debris washed in through its entrance, but rather the reverse.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 4th, 1872.

Professor W. C. WILLIAMSON, F.R.S., President of the Section, in the Chair.

The PRESIDENT delivered an address of which the following is an abstract:—

Your secretary has intimated that a few remarks would be expected from me on the occasion of our entry upon the work of a new session and of my occupying once more your presidential chair. Under these circumstances I would direct your attention for a few moments to a question that vitally affects us as Lancashire naturalists. We live in a district that has long been celebrated for the multitudes of men who have devoted their leisure to the study of nature in some one or other of its varied aspects. It was the home of Hobson and of Caley, of Crowther and of Buxton, and the race is still perpetuated by a large number of men like Butterworth, Nield, and Whittaker, to whose field-labours, as active collectors, the special investigations upon which I have long been engaged owe so much of their success. The energetic spirits of a kindred society—the Scientific Students' Association—are in like manner taking a fair share in the work of sustaining the reputation of Lancashire for the earnestness of its practical naturalists. We have much reason for being thankful that we are surrounded by so many men who are able and willing thus to carry on this labour of love.

But from amidst these grounds for congratulation there looms out, but too distinctly, a fact of an opposite kind—a fact which does not affect us alone, but the responsibility for which is shared, I fear, by the entire nation. I would not for a moment be deemed capable of unduly depreciating the *systematic* study of the animal and vegetable kingdoms, to which as Englishmen we are so addicted. On the contrary, I know too well that such studies are essential to us; they constitute the indispensable foundations upon which those who aim at erecting loftier edifices must build. But whilst making this admission in the most unreserved manner, I cannot hide from myself, or from you, the fact that there are yet higher subjects of thought and research than those involved in the discrimination of genera and species, or in the study of the systematic positions which objects should occupy in the human classifications. It is eminently characteristic of the present age that men have become alive to this truth; hence we find them in various parts of the world grappling with the loftiest of problems. The sneers with which "Peter Pindar" saluted Sir Joseph Banks for impaling butterflies and boiling fleas are no longer possible. Goethe, Oken, and Owen have stimulated us to the study of animal and vegetable homologies; Darwin has removed many of the difficulties that beset the Lamarckian ideas respecting the origin of species; by sending us along what I believe to be the right track he has opened the way to new lines of enquiry so vast as to demand the greatest of intellects to trace their ultimate ramifications and to reach the grand generalisations towards which they will finally conduct us. Then there is the wide field of detailed physiological research, in which so much has already been done, but so much of which is yet uncultivated. We are surrounded one very hand by myriads of plants and animals of whose life-history we know little, but which invite our study. To this end we must make the microscope our primary instrument, with the auxiliary appliances of chemical reagents to which of late years so much attention has been paid. These remarks suggest but a few of the problems which are awaiting a thorough solution. With the remembrance of the importance of these problems fresh in our minds we may ask ourselves what are we individually doing as our contribution towards the attainment of the desired results.

With a few noble exceptions I fear the answer to this question is alike unsatisfactory to us as men of Manchester and as Englishmen. We do not pursue wide and prolonged researches and work them out to their ultimate issues, in the way that is done by the naturalists of France and Germany. This remark is especially applicable to the subject of Vegetable Physiology. When I take up a number of the *Annales des Sciences Naturelles* and see such magnificent physiological memoirs as have been supplied by men like Mohl and Trecul, Van Tieghem and Nägeli, Hofmeister and Tulasné, I cannot but ask myself what have we Englishmen to show as our contributions to this series. I do not forget that our countryman Robert Brown was the grandest figure in the group of pioneers in these researches; but upon whom has his mantle fallen? We fear that no one has risen up amongst us capable of receiving it. The defective standard of which I complain is further shewn in the Physiological text-books with which we Englishmen are satisfied. Excellent and useful as the Manuals of Henfrey, Balfour, and Oliver may be, they bear no comparison to the noble "Lerbuch" of Sachs; a volume which is as rich in the facts which it records as it is profound in the philosophy which it seeks to expound. I know not what the cause of this unsatisfactory state of the higher departments of study in England may be. Something is doubtless due to the fact that we are all more or less engaged in a feverish race after the material comforts of life, which do not, in the same degree, tempt our Continental brethren from the quiet retirement of their studies. Many of them are content with a less share of worldly things than satisfies us; hence we find amongst them a much larger number of men who make scientific research the business of a life than is to be found here. We have around us an earnest band of amateurs who turn from their special callings at the close of the day to such branches of natural science as they severally select for the recreations of the evening and of the holiday; but such interrupted and superficial studies, invaluable as they are to the students themselves—and I believe that we can scarcely exaggerate that value—are insufficient to supply the deeper want upon which I have dwelt. I can only trust that we shall all be roused during the coming session to grapple with some of the profound biological questions that are now before the world asking for solution; and that we may thus contribute, in some humble degree, to remove the reproach which I fear deservedly rests upon us, of being satisfied with the more easily followed and superficial lines of enquiry, instead of striving boldly to sink our plumb-lines into the deepest abysses of the vast ocean of undiscovered truth.

Mr. H. A. Hurst read a Paper "On the Flora of Alexandria (Egypt)," illustrated by a series of specimens collected by himself.

"On the Destruction of the Rarer Species of British Ferns," by Joseph Sidebotham, F.R.A.S.

The object of the writer was to protest strongly against the destruction of many of the rare species of our native ferns. He mentioned four districts in Lancashire, Derbyshire, Westmorland, and Wales, and gave lists of ferns which he had found abundantly in them 25 years ago, all of which have now entirely disappeared, or have become exceedingly rare. Since fern collecting became a sort of fashion a few years ago, a class of people has sprung up who gain a livelihood by collecting and selling fern roots to tourists; these are exposed for sale in the markets during the summer season, and it is pitiable to see cartloads of them torn from their native rocks and glens, and to think that not one root in a hundred will grow when carried away and planted on rockwork; and the few plants that do survive are but miserable representatives of their respective species. There are laws to protect the small birds from being exterminated, but none can be framed to protect our ferns and wild flowers. The only suggestions the writer could make to preserve them was to appeal to tourists on no account to purchase roots of ferns from these dealers, and not to dig up rare specimens when they find them, but content themselves with the fronds. He then enumerated the various native species of ferns, and showed how few of them were suitable for cultivation in ordinary gardens and rockeries, and that for such a purpose the common species were really more suited in every way than the rarer, being handsomer and more easily grown. He also strongly advocated the growth of varieties from spores, and spoke of the pleasure he had experienced in examining the extensive collection of those raised by E. J. Lowe, F.R.S., &c., of Highfields, near Nottingham.

Mr. Hurst mentioned that the Madeira *Dicksonia Culcita* had been eradicated from its sole Spanish habitat, near Algeziras, by collectors.

Ordinary Meeting, March 4th, 1873.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. Francis Nicholson, F.Z.S., was elected an Ordinary Member of the Society.

T. T. WILKINSON, F.R.A.S., communicated the following "Monthly Fall of Rain, according to the North Rain Gauge at Swinden, as measured by Mr. James Emmett, Waterworks Manager, Burnley, from January 1st, 1866, to December 31st, 1872":—

	1866	1867	1868	1869	1870	1871	1872
January	5·17	3·12	4·08	5·12	3·19	1·17	4·77
February	3·65	4·45	3·74	6·75	0·78	2·26	3·16
March	2·24	1·48	4·55	0·80	1·70	0·99	3·92
April	0·99	4·75	2·23	2·00	1·33	2·25	4·29
May	1·23	2·75	1·50	3·03	1·54	1·30	2·95
June	4·25	1·75	0·45	1·19	3·62	2·38	6·60
July	5·59	4·92	0·68	1·52	1·31	2·83	3·40
August	7·60	2·06	4·34	2·70	0·58	1·35	4·05
September	12·07	2·94	2·72	5·21	0·96	1·50	6·75
October	2·71	4·27	5·33	3·50	7·08	3·06	5·88
November	6·86	1·26	2·27	3·75	2·64	2·10	6·58
December	5·88	4·55	10·00	4·70	1·31	1·85	3·61
Total in inches..	58·24	38·30	41·89	40·27	26·04	23·04	55·96

NOTE.—The height of the Rain Gauge is about 750 feet above the level of the sea, and about 18 feet above the ground.

Mr. BAXENDELL read the following communication from Mr. S. BROUGHTON :—

It appears there is some doubt as to the existence of ball discharge in thunderstorms. At the request of Mr. Baxendell I communicate an observation of such, seen during the approach of a storm, in 1854 or 1855, when walking from Altrincham to Timperley.

Over the edge of a cloud near the east horizon a flash of lightning was seen, and a ball *apparently* the size of one from a Roman candle shot upwards through an arc of 20° or

30°. I cannot say that it went to another cloud, but that would most likely be so, as my attention was taken up watching the progress of the electric ball.

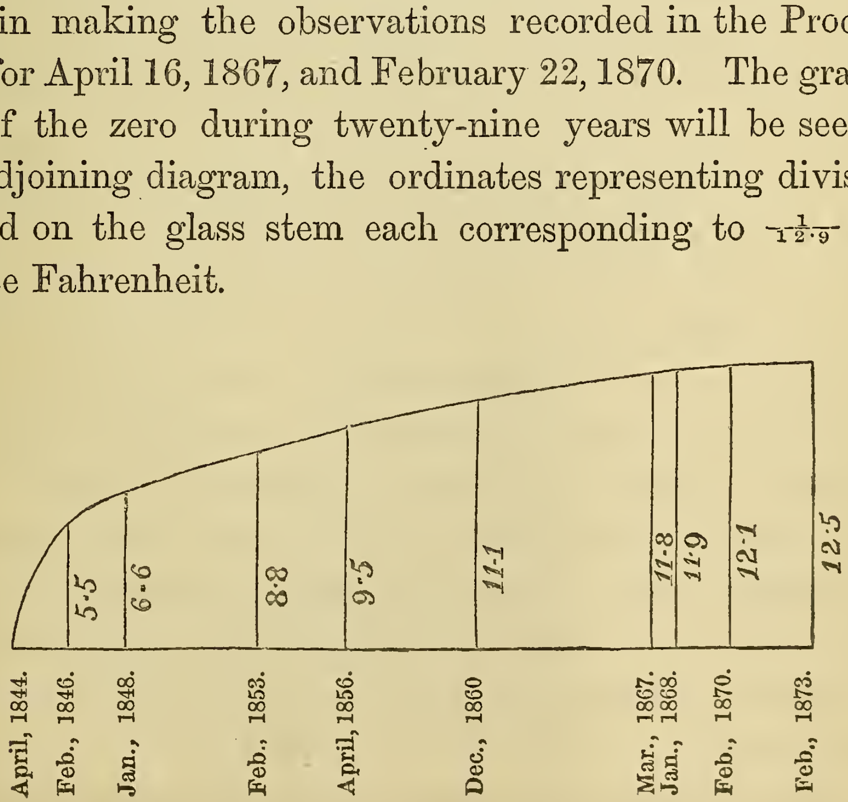
E. W. Binney, V.P., F.R.S., said that shortly after the meeting of the Society on the 21st January last, when he exhibited the singular fossil plants, which were quite new to him at the time, and which he thought would have to be placed in a new genus, he had received excellent transverse and longitudinal sections of similar specimens from Professor Renault of Cluny, which were if possible in a more beautiful state of preservation than those found in the carboniferous strata of Lancashire. On the 4th February Professor W. C. Williamson, F.R.S., stated that these specimens were the branches or stems of the well-known genus *Asterophyllites*, and he had communicated his views to the Royal Society so early as November, 1871, wherein he expressed his opinion "that *Asterophyllites* is not the branch or foliage of a Calamite, but an altogether distinct type of vegetation having an organisation peculiarly its own."

Now the distinguished French Professor in his letter to me states that he had described this fossil plant in a memoir read before the Academy in 1870, and that in his opinion it belonged to *Sphenophyllum*, and an abstract of the communication appears in the *Comptes Rendus* for 1870. I am not in possession of the facts from which the two learned professors came to such different conclusions, but I am inclined to consider the singular little stem as belonging to a new genus until the leaves of *Sphenophyllum* or *Asterophyllites* are found attached to it. When this comes to pass of course there can be no doubt on the matter.

Mr. Brockbank, F.G.S., exhibited specimens of iron manufactured by the old Bohemian process from hematite ores in the south of Europe. Similar iron has also recently been sent to England from Japan, the high prices now ruling having attracted supplies of iron from distant countries.

Finished bar iron is produced at the present time in countries where labour is cheap and charcoal plentiful at an exceedingly low price as compared with present values in England. The specimens now exhibited cost only £6 per ton for the bloom and £8 per ton for the finished bar. The sizes of the bars are however very small, but it is a remarkable fact that on so small a scale iron of the very highest quality can be made and sold at half the price of English bars made on the largest scale with all the advantages of our modern machinery and appliances. It is believed that this iron is made by a similar process to that followed by the Romans in Britain, the remains of furnaces or "bloomeries" on Ennerdale lake being of this class.

THE PRESIDENT said that he had made another observation of the position of the freezing point in the thermometer used in making the observations recorded in the Proceedings for April 16, 1867, and February 22, 1870. The gradual rise of the zero during twenty-nine years will be seen by the adjoining diagram, the ordinates representing divisions etched on the glass stem each corresponding to $\frac{1}{12 \cdot 9}$ of a degree Fahrenheit.



"On the Influence of Acids on Iron and Steel," by William H. Johnson, B.Sc.

I.—*General Effects of Acid.*

Pieces of iron and steel wire of various qualities were immersed in sulphuric or hydrochloric acids for spaces of time varying from 10 minutes to 12 hours, and then well washed with water and dried, and the following experiments made:

1. On breaking one of the pieces of wire and moistening the fracture, still warm from the effort of breaking it, bubbles were seen to rise through the water from the *whole* surface of the fracture, even when the piece was ·412 inch diameter. Further, pieces of wire that had been immersed in acid, washed, coated with lime, dried, and drawn to a smaller diameter, thus removing any trace of acid on the surface, gave bubbles in the same manner. The bubbles are most abundant if the iron has been immersed in sulphuric acid, and may be seen several days after the iron has been removed from the acid. If steeped in hydrochloric acid the bubbles are seen with difficulty and only after long immersion.

Bubbles are not apparent with steel, even after prolonged immersion, except the steel be very mild.

Test paper was not sensibly altered in colour by the water on the fractures.

By exposure to the atmosphere, or more quickly by steeping in water, the above phenomena, as well as those to be mentioned later on, decrease in intensity until at length they are no longer visible, and the iron is quite restored to its original state. Gentle heat greatly aids this. They also cease to be visible sooner if hydrochloric acid be employed than if sulphuric acid is used, doubtless because the latter is less volatile.

2. The fracture of a piece of iron or steel immersed for

one hour or more in either acid is somewhat darker in colour than before. After several hours the fracture may be black in the centre and more or less crystalline in appearance.

3. Pieces of iron or steel heated in a confined space after immersion in acid become slightly rusted. If air has free access during the application of heat, this is not the case.

It thus appears that heat expels the dilute acid from the interior of the iron, which if not carried away with sufficient rapidity by the surrounding air attacks the surface of the iron, forming an oxide or oxychloride of iron.

Sometimes instead of a uniform coating of rust the iron is simply spotted. The acid will in some cases, after lapse of time, find its way to the surface of the iron and spot it with rust, even without the application of heat; this is particularly the case with iron which has been soaked in sulphuric acid.

It is this power which iron possesses of absorbing acid and afterwards giving it off, which accounts for the difficulty hitherto experienced of coating iron with copper, tin, or any other metal in acid solutions. For the acid on coming to the surface of the iron is unable to make its way through the impervious coating of metal, and consequently combining with the iron at the surface, forces the copper or tin off.

4. The universal effect of acid on iron and steel is to decrease its toughness. This brittleness is most marked with steel. Sometimes a coil of steel wire after immersion in acid will break if allowed to fall on the ground. And I have seen hardened steel and steel containing a large percentage of carbon fly in pieces as soon as it was immersed in acid without being touched at all.

II.—*Effect on the Weight.*

Pieces of iron and steel were immersed in acid for different periods of time, well washed in water, and weighed. They were then heated in a kitchen oven and again weighed. The results are given in the table below.

TABLE SHOWING THE INCREASE OF WEIGHT AFTER IMMERSION IN ACID.

			HYDROCHLORIC ACID.					SULPHURIC ACID.			
	QUALITY.		Weight in Grams.		Loss by Heating.	Gain °/° by Immersion.	REMARKS.	Weight in Grams.		Loss by Heating.	Gain °/° by Immersion in Acid.
			Before Heating	After Heating				Before Heating	After Heating		
1	Steel......	·124	49·81525	49 81500	·00025	·000502	Appearance of fracture crystalline, speckled and white; after heating, finer and greyer.	50·56990	50·55516	·01474	·029156
2	Mild Steel.	·126	47·36490	47·36020	·00470	·009923		43·85970	43·84990	·00980	·022350
3	Best Iron..	·122	47·48030	47·47495	·00535	·011260	Annealed.	43·25005	43·23965	·01040	·024052
4	Char. Iron.	·125	43·20994	43·20020	·00974	·022540		42·34002	42·32974	·01028	·024285
	Total..		187·87039	185 85035	·02004	·010659		180·01967	179·97445	·04522	·025126
	In acid 5 hours, then washed several times in water and heated 18 hours in an oven.										
5	Mild Steel.	·165	78·69240	78·65170	·04070	·05187		71·36530	71·32490	·04040	·05664
6	Best Iron..	·165	81·68530	81·67220	·01310	·01604		85·98500	85·94000	·03500	·04072
7	Char. Iron.	·165	78·69240	78·65170	·01595	·02028		84·09020	84·07515	·01505	·01796
	Total....		239·07010	238·97560	·06975	·02918		241·44050	241·34005	·09045	·03747
	In Acid 3½ hours, then well washed in water. Heated 18 hours.										
8	Steel......	·165	80·08010	80·06770	·01240	·01548					
	In Acid 12 hours. Heated 30 hours.										
9	Steel.[1].....	·180					Very slightly rusted after heating.	79·10020	79·09005	·01015	·01283
10	Mild do. ..	·182						77·56980	77·56990	—·00010	
11	Best Iron..	·155						74·92055	74·91722	·00333	·00440
12	Charcoal..	·158						61·42040	61·41990	·00050	·000814
13	Ditto	·420	87·45715	87·45500	·00215	·00245					
	In Acid 12—13 hours, then steeped in water for 10 hours. Heated 24 hours.										

In all cases except one they were found to have lost in weight, and the exception was probably owing to the increased weight caused by a slight coating of oxide overbalancing the loss occasioned by heating.

The gain in weight by immersion in H^2SO^4 is greater than by immersion in HCl.

In experiments 1—4 the gain per cent is:

For immersion in HCl...... = ·010659
Ditto in......... H^2SO^4 ..... = ·025126

or almost as 2 to 5, more accurately as 1 : 2·357.

In experiments 5—7 the gain per cent for

HCl .... = ·02918
H^2SO^4.............. = ·03714

as 1 : 1·284.

Experiments 9—13 show how rapidly steeping in water removes what the iron has taken up by immersion in acid; the loss in weight on subsequent heating being only about 1-10th of that in previous experiments where the iron had not been immersed in water any length of time.

III.—*Effect on the Breaking Strain and Elongation.*

The effect of immersion in acid on the breaking strain and elongation of iron wire naturally suggested itself as an interesting subject for inquiry. Accordingly a number of pieces of iron wire were immersed in hydrochloric acid for one or more hours, and then carefully tested for elongation and breaking strain. The pieces were then heated on a hot plate for some hours and again tested with the following general results.

1. That immersion in acid diminishes the breaking strain of iron wire from ½ to 3 per cent, and steel wire about 4·76 per cent.

2. That immersion in acid appears in some cases to diminish, in others slightly to augment, the elongation of iron wire; and to augment the elongation of steel wire about 30 per cent.

Subjoined are the results of a few of the experiments on iron wire.

QUALITY.	No.	ELONGATION.		BREAKING STRAIN.	
		Immersed in Acid 1 Hour.	Heated.	Immersed in Acid 1 hour.	Heated.
Annealed Iron Wire, ·164in. diam.	1	15%	22%	1176	1168
	2	19	20	1176	1162
	3	22	19	964	1008
Average......		18·6%	20·3%	1105·3	1112·6
Annealed Iron Wire, ·150in. diam.	4	24%	22%	908	944
	5	24	21	908	930
	6	22	25	896	946
	7	21	23	914	908
	8	22	22	926	924
	9	24	24	926	924
	10	22	23	934	896
	11	22	21	930	928
	12	21	20	924	906
Average......		22·4%	22·3%	918·4	922·8
Hard Iron Wire, ·136in. diam.	13	·5%	2%	1230	1218
	14	2·5	3·5	1146	1230
	15	2	3	1200	1232
Average......		2%	2·83%	1192	1226·6

IV.—*Effect of Pyroligneous Acid.*

The effect of pyroligneous acid on iron and steel appears to be exactly similar to that of hydrochloric and sulphuric acids, causing it to become more brittle, &c., though the effects are perhaps somewhat less intense. As in their case, heat restores the iron to its original toughness.

V.—*Effects of Acids on Copper and Brass.*

Sulphuric acid appears to have no effect whatever on copper. After 18 hours' or longer immersion in sulphuric acid copper is as tough as ever, the action being confined to the surface only.

Brass becomes rotten after long immersion in vitriol, doubtless because the zinc of which it is partly composed is attacked by the acid, and, as might be expected, heat does not restore it to its original condition. Prolonged exposure to a moist damp atmosphere appears to make brass brittle just as acid does.

VI.—*Effect of Zinc on Iron.*

A piece of galvanized iron of good quality, which when cold several times resisted bending to and fro at right angles to itself, was raised to a red heat with such rapidity that only a small portion of the coating of zinc was vaporised. On then attempting to bend it, it broke off sharp, the fracture being short and crystalline. When cold, this piece broke with all its former toughness, the fracture showing a long fibre. The same piece was then heated till all the coating of zinc was driven off; it was then found impossible to break it. This clearly shows that the iron was not red short except when rendered so by the zinc.

The same experiments were tried with iron coated with lead and with tinned iron, but without the above results.

Some kinds of iron do not appear to be rendered red short by zinc.

Possibly the above phenomenon may have some connection with the fact that zinc forms an alloy with iron at a red heat, containing from 2 per cent to 6 per cent of iron, and having a melting point which is higher as the proportion of iron is greater, while lead and tin do not alloy with iron at this temperature. But still the iron appears to absorb the liquid zinc in a similar way to that in which it appears to take up acid on immersion in it, and with similar results.

Hitherto I have spoken of iron absorbing and occluding acid as though this something which increases the weight of the iron, alters its tensile strain, &c., had been definitely proved to be acid; but in the face of my having been unable to obtain any reaction to test paper, this is very uncertain. Though the fact that the immersion of iron which has been soaked in an alkaline fluid greatly hastens its restoration to its original state, and the rusting of the surface of iron soaked in acid when heated in a confined space, all lead to the belief that acid is absorbed, though other bodies, such as gases, may be occluded at the same time.

The experiments of Professor Graham in 1867, and more recently those of Mr. Parry, show that hydrogen, carbonic oxide and carbonic acid, and nitrogen are evolved from wrought iron, cast iron, and steel, when heated in vacuo. Therefore it seems probable that a part of the hydrogen produced by the action of the acid on the iron may be absorbed by the iron, its nascent state facilitating this. And when the iron is heated by the effort of breaking it, the gas may bubble up through the moisture on the fracture.

In Mr. Parry's experiments while one vol. of iron evolved two vols. of gas when heated strongly in vacuo; one vol. of mild steel evolved only ·13 of a vol. of gas. If from a small evolution of gas during heating of steel in vacuo we may argue a very small evolution of gas in steel soaked in acid, then we are led to suppose that the bubbles evolved from the hot moist fracture of a piece of steel will be very small or imperceptible, which experiments amply confirm.

Ordinary Meeting, March 18th, 1873.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. James Cosmo Melvill, M.A., F.L.S., was elected an Ordinary Member of the Society.

E. W. Binney, F.R.S., V.P., said that during the last week an interesting controversy had been going on in this city between the Town Clerk and the Professor of Chemistry at the Royal Institution as to the quality of the water supplied to Manchester. These disputants are well able to wage their own warfare, therefore it is not my intention to interfere with them. In these days no one doubts the blessings of a constant supply of pure and good water; but the latter quality is determined in a great measure by the purpose for which it is intended to be used. If for manufacturing and washing then a pure soft water is no doubt most desirable, but it is very questionable if such a water when conveyed any considerable distance in leaden pipes is the best for the drinking purposes of a town population.

In the Report of the Commissioners for Inquiring into the State of Large Towns and Populous Districts, Dr. Lyon Playfair, the Commissioner who reported on the then supply of Manchester appears to have directed little attention to the quality of drinking water for a town population which had to a great extent left off using the milk, porridge, brown bread, and oatcake of our forefathers, and resorted to sloppy tea, white bread, butter, and a little meat, for at page 411 of his Report he says:—" In considering the best means for the extension of this benefit," alluding to a constant supply, " to the working classes, or in sanctioning the formation of new waterworks, it would be highly advisable to obtain evidence as to the quality of the water, particularly with regard to its hardness. The value of attention to this point will be obvious, when the difference of consumption of soap is considered. I found by various trials in summer that the Manchester water possesses a hardness equivalent to what would be obtained if 13 or 14 grains of chalk were dissolved in a gallon of pure water." The learned Commissioner gives the water at Aberdeen at one grain of chalk per gallon, and comparing that with the 14 of Manchester and the 12 of London, he concludes "Thus the hard water of Manchester may be regarded as increasing the water rent to a family of five individuals 16s. 8d. per annum, or £49,363 per annum to the whole town, a sum nearly double that of the present water rental. But large as the cost entailed upon a town by a bad selection of water in the unnecesary consumption of soap, still greater loss is incurred in the wear and tear of clothes." This was written about thirty years since, and I have not the death-rate of Manchester in 1842. In that space of time how much money has been expended in Manchester by the public authorities in shutting up cellar dwellings, closing grave yards, removing pigstyes, altering ashpits and middens, opening new streets, and supplying pure water? I cannot tell its amount, but every ratepayer knows practically that it is very large. In looking at the rate of mortality for the week ending March 8th, as given in the *Manchester Guardian*, in the 21 leading places in the kingdom, it was at the annual rate of 28 per thousand. In London, the rate was 27; Bristol, 31; Wolverhampton, 28; Birmingham, 28; Nottingham, 27; Liverpool, 31; Manchester, 36; Bradford, 26; Sheffield, 27; Newcastle-on-Tyne, 31. Now I believe the first named five towns are supplied with hard water, and give an aggregate of 141, whilst the latter five, are supplied with soft water, and give an aggregate of 151. This is a significant fact and worthy of grave consideration. True, it is only one week, and a whole year ought to be examined, but I imagine the results if carefully gone into will give no advantage to the use of pure soft water when compared with hard, for 27 is a very high rate for London. In building up the skeleton of an adult large quantities of the phosphates and carbonates of limes are required. The well to do, who consume plenty of butchers' meat, cheese, and new milk, may manage to obtain what nature requires, but for the poor, who live on sloppy tea, fine white bread, a little butter, a trifle of meat, and plenty of soft water, where are they to get their necessary supply from? It is not my intention to assert that the high rate of mortality is all due to soft water. No doubt there are many causes which help to produce it, but good, wholesome drinking water, containing carbonate of lime, and plenty of fresh air, which is hard to get in a close and crooked-built town of high warehouses, have in my opinion much to do with it. In my own case, I put a little lime in the drinking water used in my house, and I live on a sandy hill, well exposed to the winds of heaven. In all sanitary arrangements too much attention cannot be given to providing plenty of fresh air and as much light as practicable.

"Observations on the Rate at which Stalagmite is being accumulated in the Ingleborough Cave," by W. Boyd Dawkins, M.A., F.R.S., F.G.S.

The only attempt to measure with accuracy the rate of the accumulation of stalagmite in caverns, in this country, is that made by Mr. James Farrer in the Ingleborough Cave, in the years 1839 and 1845, and published by Professor Phillips in "The Rivers, Mountains, and Sea Coast of Yorkshire," (second edition, 1855, pp, 34-35). The stalagmite of which the measurements were taken is that termed, from its shape, the jockey cap. It rises from a crystalline pavement to a height of about 2½ feet, and is the result of a deposit of carbonate of lime, brought down by a line of drops that fall into a basin at its top, and flow over the general surface. On March 13th, 1872, in company with Mr. John Birkbeck and Mr. Walker, I was enabled by the kindness of Mr. Farrer to take a set of measurements, to be recorded for use in after years.

For the sake of insuring accuracy in future observations, three holes were bored at the base of the stalagmite, and three gauges of brass wire, gilt, inserted, gauge No. 1 in the following table being that on the S.S.E., No. 2 on N.N.E., No. 3 on the W. side. The curvilinear dimensions were taken with fine iron wire, or with a steel measure; and the circumferential around the base along a line marked by the three gauges. The measurements 2, 3, and 4 of the table ware taken on the 15th of March, by Mr. Walker, and their accuracy may be tested by the fact that they coincide exactly with No. 1, which I took two days before.

The lengths of wire, properly labelled, will be deposited in the Manchester Museum, The Owens College, for future observers.

In the following table I have given my own measurements and compared them with those taken by Mr. Farrer.

TABLE OF MEASUREMENTS.

	13th Mar. 1873. Inches.	1839. Inches	30th Oct. 1845. Inches.	Increase since 1839	1845	Rate of increase per annum. Inches.
1 Basal circumference at Gauges..	128	118	120	10	8	·2941—·2857
2 Guage No. 1 to Gauge No. 2....	52·625					
3 ,, 2 ,, 3....	35·0					
4 ,, 3 ,, 1....	40·375					
5 Gauge No. 1 to hole in centre of basin at apex....	30					
6 ,, 2 ,, ,, ...	29·5					
7 ,, 3 ,, ,, ...	31·4					
8 Hgt. from Gauge No. 1..........	20·9					
9 ,, ,, 2 minimum	20·4					
10 Maximum......................	29·7					
11 Tape measurement on slope gauge No. 1 to edge of apex..	26·7					
12 ,, No. 2 ,, ,,	26·6	21·0		5·6		
13 ,, ,, Maximum ,,	36·0	32·0	35·0	4·0	1·0	
14 Roof to apex of Jockey cap	87		95·25		8·25	·2946
15 Roof to tip of stalactite			10			
16 Stalactite to apex of Jockey cap.			85·25			

Unfortunately I have been unable to identify the exact spots where the stalagamite was measured by Mr. Farrer, so that the only measurement which affords any trustworthy data for estimating the rate of increase is number 14. With regard to this the only possible ground of error is the erosion of the general surface of the solid limestone, of which the roof is composed, by carbonic acid, since the year 1845, and this is so small as to be practically inappreciable. We have therefore evidence that the jockey's cap is growing at the rate of ·2946 of an inch per annum, and that if the present rate of growth be continued it will finally arrive at the roof in about 295 years. But even this comparatively short lapse of time will probably be diminished by the growth of a pendent stalactite above, that is now being formed in place of that which measured ten inches in 1845, and has since been accidentally destroyed. It is very possible that the jockey cap may be the result not of the continuous but of the intermittent drip of water containing a variable quantity of carbonate of lime, and that, therefore, the present rate of growth is not a measure of its past or future condition. Its possible age in 1845 was estimated by Professor Phillips at 259 years, on the supposition that the grain of carbonate of lime in each pint was deposited. If, however, it grew at its present rate it may be not more than one hundred years old. All the stalagmites and stalactites in the Ingleborough cave may not date further back than the time of Edward III. if the Jockey cap be taken as a measure of the rate of deposition.

It is evident, from this instance of rapid accumulation, that the value of a layer of stalagmite, in fixing the high antiquity of deposits below it is comparatively little. The layers, for instance, in Kent's Hole, which are generally believed to have demanded a considerable lapse of time, may possibly have been formed at the rate of a quarter of an inch per annum, and the human bones which lie buried under the stalagmite in the cave of Bruniquel are not for that reason to be taken to be of vast antiquity. It may be fairly concluded that the thickness of layers of stalagmite cannot be used as an argument in support of the remote age of the strata below. At the rate of a quarter of an inch per annum 20 feet of stalagmite might be formed in 1000 years.

"On Methyl-alizarine and Ethyl-alizarine," by EDWARD SCHUNCK, Ph.D., F.R.S.

In a paper which I had the honour of reading before this Society some time ago* I gave an account of a yellow colouring matter accompanying artificial alizarine, to which I gave the name of *anthraflavic acid.* Though the substance was at the time new to me and apparently to others also, it is quite possible it may have been previously observed by those working with artificial alizarine, since the crude product is probably hardly ever quite free from it, and its presence would not be likely to escape the notice of any one endeavouring to prepare pure alizarine from the manufactured article.

My analyses of the acid and of its barium and silver salts led to the formula $C_{15}H_{10}O_4$ for the acid, and I was therefore inclined to view it as a body homologous with alizarine, or alizarine in which H is replaced by CH_3. I supposed it to be derived from a hydrocarbon higher in the series than anthracene ($C_{15}H_{12}$?) contained in the ordinary anthracene of commerce, a body which is supposed by some chemists really to exist, and which would stand in the same relation to anthracene as toluol does to benzol. It was necessary to adopt some such hypothesis, since, as Graebe and Liebermann remark, in referring to my experiments, a compound obtained from anthraquinone by the same process as that yielding alizarine cannot possibly contain 15 atoms of carbon. The conversion of the acid into alizarine by the action of fusing caustic potash would however admit of explanation in accordance with my view, since the methyl

* Proceedings Lit. and Phil. Soc., Session 1870-71.

presumed to be contained in it might be supposed to be eliminated and replaced by hydrogen during the process.

The examination of anthraflavic acid was subsequently undertaken by Mr. Perkin,* whose analyses of the carefully purified substance led to the conclusion that it is isomeric with alizarine. I do not wish to dispute the accuracy of this view of its composition, since a trifling admixture of some impurity, such as anthraquinone, might easily have given rise to the excess of carbon found in my analyses, though I may state that a specimen of the substance, prepared from some of the "by-product" of the manufacture of alizarine—kindly sent me by Mr. Perkin—and purified with great care, gave exactly the same composition as before.

Graebe and Liebermann† have also examined a yellow crystalline body accompanying artificial alizarine, which is converted into the latter by the action of fusing caustic potash. They are of opinion that it is identical with anthraflavic acid, there being, indeed, little or no difference in the properties of the two substances. They assign to it the formula $C_{14}\ H_8\ O_3$, and consider it as monoxyanthraquinone, alizarine being dioxyanthraquinone. The results of their analyses of the substance and its barium compound differ however so widely from those obtained by Mr. Perkin and myself (particularly in this respect, that in the compounds of anthraflavic acid, two atoms of hydrogen are replaced by metals, whereas in those of monoxyanthraquinone only one atom is replaced) as to lead to the conclusion either that there exists more than one body having the general properties—chemical and physical—of anthraflavic acid, or that we have not all of us been working with pure substances.

Without pronouncing any decided opinion on this point, which can only be determined by further investigation, and without entertaining any sanguine anticipation of being able to prepare anthraflavic acid directly from alizarine, it

* Chem, Soc. J., XXIV, 1109. † Liebig's Annalen CLX., 141.

seemed to me that it might be of some interest to ascertain the nature and properties of the methylic and ethylic substitution products of alizarine obtained directly from the latter.

In order to obtain methyl-alizarine I tried several methods. The first consisted in heating bromalizarine with iodide of methyl and metallic silver in closed tubes. This process yielded a small quantity of a crystalline substance, which I believed to be the compound sought for. The other method, which is one now often practised for obtaining methylic and ethylic substitution products, gave better results. Purified artificial alizarine was treated with a mixture of iodide of methyl, caustic potash, and a little methylic alcohol in closed tubes, at a moderate temperature. After heating for some days the tubes were opened and emptied, and the excess of iodide of methyl having been evaporated, the residue was treated first with hot water, to remove the iodide of potassium, and then with a little cold alcohol. The alcohol—which dissolved out a brown resinous impurity—having been filtered off, the residue was treated with dilute caustic potash lye, in which the alizarine not acted on dissolved with a violet colour. The liquid having been filtered off, the residue, which consisted of the potassium compound of methyl-alizarine—a compound very little soluble in cold water—was washed until the percolating liquid began to be of a cherry-red colour. It was then treated with hydrochloric acid, and the orange-coloured flocks left undissolved were filtered off, washed and dissolved in boiling alcohol. The alcohol, on cooling, deposited crystalline needles of methyl-alizarine.

Methyl-alizarine as thus prepared has the following properties :—When crystallised from boiling alcohol it appears in long yellow needles, having a reddish tinge, but without the semi-metallic lustre peculiar to alizarine which it generally resembles. When heated it is entirely volatilised, yielding a sublimate of yellow lustrous scales and needles. It is almost insoluble in boiling water, but dissolves easily in concentrated sulphuric acid, even in the cold, giving a cherry-red solution. It does not dissolve sensibly in caustic potash lye in the cold, but on boiling a bright cherry-red solution is obtained, which on cooling deposits dark red crystalline masses. The solution shows no trace of absorption bands, but only a general obscuration of the green part of the spectrum, and in this respect differs widely from the alkaline solutions of alizarine, which exhibit such very characteristic absorption bands. The solution in concentrated sulphuric acid does, however, show an absorption band on the border of the green and blue, just like a solution of anthraflavic acid in the same menstruum, but far less distinctly than the latter, on account of the much greater obscuration of the parts of the spectrum adjacent to the band. On adding alcoholic potash solution to an alcoholic solution of methyl-alizarine the potassium compound is deposited in dark red needles, arranged in star-shaped masses. The sodium compound, prepared in the same way, crystallises in small light red needles. A watery solution of the potassium compound gives with chloride of barium a red flocculent precipitate. The alcoholic solution of methyl-alizarine gives no precipitate with acetate of lead. When treated with boiling nitric acid methyl-alizarine is dissolved and decomposed, and the solution on evaporation leaves a white crystalline residue, probably of phthalic acid. Methyl-alizarine undergoes no change when treated with strong caustic potash lye, even at the boiling temperature. It is only when fusing hydrate of potash is employed that decomposition takes place. If the operation be carefully conducted there is obtained, on the addition of water to the fused mass, a violet-coloured solution, which shows the absorption bands of alizarine very distinctly. There is no doubt, therefore, that by the more energetic action of the alkali at the temperature of fusion alizarine is regenerated. Methyl-alizarine does not dye mordanted cloth when tried in the usual manner. It imparts hardly any colour to the mordants, and differs, therefore, in this respect from the parent substance more than in any other.

Though methyl-alizarine differs in most points very widely from anthraflavic acid, still the two substances are found to resemble one another as regards some of their properties. Both yield crystallised potassium and sodium compounds. Both are converted into alizarine by the action of fusing potassic hydrate, though both remain unchanged when treated with strong alkaline lyes. The action of both on the spectrum is very similar. Neither of them is precipitated from its alcoholic solution by acetate of lead. Both are incapable of dyeing mordants.

The analysis of methyl-alizarine gave numbers corresponding with the formula $C_{15}H_{10}O_4$. It is therefore alizarine in which one atom of hydrogen is replaced by methyl. It still remained to determine how this substitution takes place, whether it is one of the two hydroxyl atoms contained in alizarine the hydrogen of which is replaced by methyl, or whether the substitution is effected in a different manner. In the former case methyl-alizarine would contain only one atom of hydrogen replaceable by metals. The formula of methyl-alizarine being $C_{14}H_6(HO)(CH_3O)O_2$, that of the potassium compound, for instance, would be $C_{14}H_6(KO)(CH_3O)O_2$ and it would contain by calculation 13·3 per cent of potassium. Now the potassium compound prepared in the manner just described and dried first over sulphuric acid and then at 130° C., was found to contain 12·6 per cent of potassium. It is certain therefore that methyl-alizarine belongs to the class of compound ethers, being formed by the replacement of one of the hydrogen atoms of a bibasic acid by methyl. It has a similar composition to Mr. Perkin's diacetyl-alizarine. In the latter how ever two atoms of hydrogen are replaced by the compound radical acetyl. Diacetyl-alizarine seems also to be a much less stable body than methyl-alizarine.

Ethyl-alizarine may be prepared in the same way as the corresponding methyl compound, employing iodide of ethyl in place of iodide of methyl. The properties of the two substances are so nearly alike that they can hardly be distinguished from one another. The composition of ethyl-alizarine is expressed by the formula $C_{16}H_{12}O_4$.

Specimens of the two substances were shown along with some specimens sent for exhibition by Mr. Perkin, including the new colouring matter lately discovered by him, anthrapurpurine, and samples of dyed calico showing the different effects produced by alizarine and anthrapurpurine.

"On the Transition from Roman to Arabic Numerals (so-called) in England," by the Rev. Brooke Herford.

One of the collateral points of interest with which the local historian has to occupy himself from time to time, is the determination of dates. When, now three years ago, I was busy with the re-editing of Baines's History of Lancashire, left incomplete by the death of my old friend Mr. Harland, in verifying some notes about the village churches in Leyland Hundred, my attention was asked to a date on one of the beams of Eccleston church, which had been an object of curiosity to many visitors, but which no one had ever been able to decipher. The inscription was as follows:

anno dñi lhʒe

carved on the oak beam in an unusually clear, square character. For a long time I was unsuccessful in my attempts to decipher it. It was when I had got to the very last sheet of my work, and while examining some old M.SS. of the reign of Elizabeth, that I was one day particularly struck by the resemblance between the 5's of the M.SS and its h's, and at once this gave me the clue to the Eccleston date, the whole difficulty of which had lain in the very careful "𝔥" which formed the second figure. I turned to my copy of it and saw at a glance that it was in reality 1536.

The explanation of it I worked out in my mind as follows:—The inscription had evidently been cut by a very careful workman; but at that time the Arabic numerals were hardly known except to scholars, and all the associations that ordinary people had with figures were with letters used as numerals. Hence workmen tried to make the figure offered to them like the nearest letter they could find. So the workman at Eccleston, instead of imitating what seemed to him the rude h of his copy, made a beautiful "𝔥" of the period! And the same with the 3, which would be to him evidently a rough attempt at a Z; and with the 6, which, looking like an inverted e, he judiciously put what he considered the right side up. My perplexity, however, and especially the solution of it, drew my attention to the question of how long ago the Arabic numerals were introduced, and of the source from which they came to us.

Until latterly it has been generally believed that our system of decimal notation came to us from the Arabs, and hence the name Arabic numerals. It is now however generally admitted that they are originally Indian. Two lines of possible derivation from India have been traced out, each of which has been regarded as that by which their use was actually introduced into Europe. One is through the Moors. It is known that the present system of arithmetic was introduced from India into Persia at the end of the 8th century. Hence it passed into use in the north-east of Africa about the end of the 10th century, and with the Moors it would undoubtedly come into Spain. The other line is through the Latins. Boethius, in the beginning of the 6th century, in the first book of his Geometry, describes an adaptation of the Abacus which really involved the system of decimal numeration, and some of the M.SS.—and as M. Chasles proves the best and most ancient—contain a table of nine figures, which are curiously like those now in use among us,—more like our present figures indeed than are the numerals in use among the Moors. The next link in this chain of derivation is in a monkish treatise, *De Numerorum Divisione*, by Gerbert, a Benedictine monk, subsequently raised to the papal chair (in 999) as Sylvester II. This treatise (says M. Martin) does not explicitly describe the decimal numeration, but throughout takes it for granted. Whence however did Gerbert learn it? It was said, a few generations later, from the Saracens; but it appears from the arguments of M. Chasles and M. Henri Martin [to whose arguments the paper referred in detail], that this was a mistake, and it seems on the whole most probable that the abacus with nine figures has come to us from the Latins, who had it in the time of Boethius, whose ascription of it to Pythagoras doubtless arose from its having been brought from India by the Neo-pythagoreans. Preserved by Boethius, the use of these figures with an abacus of traced columns became known to the more learned monkish scholars of the middle ages, and gradually came into use in scientific calculations, the Greek cypher being supplied and the columns at length dispensed with. For generations, probably for centuries, the signs and the use of them would be confined to the learned, as little understood by the common people as are now the signs of the zodiac. It is in the popularizing of them rather than their introduction that we probably feel the value of Arab and Moorish influences.

The interesting question still remains as to the date at which they first began to make their appearance in literature, to be used for inscribing dates, and, last of all, to take their place in the transactions of the counting-house and the elementary arithmetic of schools. As might be expected, all the first traces of these figures in England were found in the old calendars and calculations with which, here and there, the monkish scholars busied themselves. Chaucer in his "Dreme" (about 1375) speaks of them as "figures newe" in a passage the tenor of which shows that he was aware of the enormous improvement which they offered upon the old use of the Roman signs. The first printed book which is known to contain the Arabic numerals is an old blackletter quarto printed at Louvain in 1476, entitled *Fasciculus Temporum.* Caxton, I believe, never uses them, in the works issued from his press; but in his *Mirrour of the World,* 1480, is a curious wood-cut representing a man sitting at a desk, and before him a board on which are drawn some rude representations of Arabic figures. The earliest authentic instances of monumental or structural inscriptions with Arabic numerals are given in the *Archæological Journal* for 1850, and were accepted by the Archæological Institute as genuine:—On a lych gate, at Bray, Berkshire, 1448; on a quarry of stained glass, at St. Cross's Hospital, Hampshire, 1497; on a stone, also at St. Cross's, 1503. I believe that nothing earlier than these is really known. There are, indeed, plenty which claim to be of greater antiquity—but one or two explanations will probably answer for them all. In several cases the bottom of the antique 4, in the hundreds, has been cut off, leaving an apparent date of the eleventh century. In still more cases a rude 5 has been read for a 1. These numerals would be used for inscriptions, as a mere fancy-lettering, long before their real importance was understood. Merchants would go on using the old figures, which had served their fathers. So we find the old system holding its place in all known public or private accounts till the beginning, and in many cases till far on into the sixteenth century. One curious exception, indeed, has been noted by that trustworthy antiquary the Rev. Joseph Hunter. At one of the meetings of the Archæological Institute, in 1850, he brought forward a fac-simile of an old warrant which he had discovered in the Record Office, in which the date (1325) is expressed in one part in Roman and in another Arabic numerals. It is a warrant from Hugh le Dispenser to Bonifez de Peruche and his partners, merchants of a company, to pay forty pounds. On the face of it, as executed by the English Chancellor, it is dated "the XIX° year" of Edward II. It bears, however, the endorsement of the Italian merchant on the back, and he has endorsed it February, 1325, in Arabic figures. I do not know that I could conclude with a better illustration of the probability of the account, which I have adopted from M. Chasles and M. Martin, of the Arabic numerals having come to Europe from India, not first by means of the Moors, but through the Italians, since we find an ordinary Italian merchant using them in an ordinary business transaction, at least two centuries before their common use in English bookkeeping and commerce.

"Notes on the Victoria Cave, Settle," by William Brockbank, F.G.S.

The discoveries of the antiquities and animal remains in the Victoria Cave have been described to the Society by Mr. Boyd Dawkins, and are very fully set forth by Mr. R. Tiddeman, F.G.S., in the *Geological Magazine* for January, 1873 (Vol. x., No. 1).

Mr. Tiddeman's views are shortly as follows. (1) He gives a section of the cave, shewing a cavern in the face of a limestone cliff, the floor of which is covered thickly over with stratified deposits, sloping inwards from the entrance, and against the edges of which rests a talus of *Breccia*, having below it a stratum of glacial drift clay with boulders. The latter he shews as just occurring above the bone bed in which the oldest remains were found, and which he therefore infers to be of preglacial age.

There is a slight but important difference between Mr. Tiddeman's statement as herein set forth, and that of Mr. Dawkins to this Society to which I took exception on the 18th of February. Mr. Dawkins gave the Society to understand that the most ancient remains, lately found, occurred outside the cave, in the talus, in which I think he was quite mistaken, and Mr. Tiddeman does not so place them. My remarks, as published in the Proceedings of that Meeting, had special reference to this very point, and as Mr. Dawkins varied his description in the published summary, they do not appear to be a reply to the context.

However, Mr. Dawkins and Mr. Tiddeman are both in accord in considering that the lower cave earth in which the oldest remains are found is immediately covered by a clay of glacial origin; and that in this case the Victoria Cave is the only one in Great Britain which has offered clear proof that the group of animals whose bones have been there found was living in the country before the glacial age.

The conclusion above stated is so important as to demand the clearest proof, and therefore the subject is one worthy of the most careful consideration, and full discussion; and as I hold the conclusion to be altogether wrong, I will proceed firstly to describe the deposits from my own point of view, and then will try to shew where I think the above gentlemen are in error.

(1) The Victoria Cave occurs in the face of a limestone crag, which appears to be much fissured, as the openings of four other caverns occur in it within a quarter of a mile, two of which are believed to be in connection with the Victoria Cave. The cliff rises from 200 to 300 feet above the cave, and beyond it is a high tract of pasture land, with numerous hollows on the surface; into which the rain sinks and finds its way through the fissures in the limestone. So

completely does all water sink away, that artificial ponds are made for the cattle to drink at in suitable places, and it is a very curious fact, that the only true clay suitable for puddling purposes, occurs in sheltered hollows on the summit of the hills, and this is a true glacial clay. No doubt this clay at one time covered the entire surface of the hill tops, as they are still dotted thickly over with huge drift boulders, or "Calliards," as they are locally called, chiefly of whinstone, black marble, and silurian flags, such as occur in the neighbouring hills northwards. The caverns all appear to have been formed on the lines of main fissures where the limestone has been much broken. The close proximity of the Great "Craven fault," (which runs at right angles to the face of the Langcliffe Scar in which the Victoria Cave occurs), will account for the great extent to which the limestone has been thus fissured.

It is therefore evident that the surface water in wet seasons, having to find its way through these fissures, from the watershed of a large area, would form great underground streams, which would wear out these caverns and carry through and into them much detritus from the surface; and very probably the whole of the drift clays, which have evidently been denuded from the surfaces where the boulders now lie, have been thus removed and carried away in the course of the long ages of time which have elapsed since their deposition, during the glacial epoch.

(2) The evidence to be gathered from the whole district points to a very considerable falling away of the face of the limestone scars during wet seasons and frosts. The day before my visit a mass of at least 100 tons had fallen from above the face of the Victoria Cave, It appears to me that the face of the scar at the cave was formerly at least 30 feet in front of its present line, and that this mass must have fallen away, at any rate since the glacial age. The limestone about the cave is so much fissured, and so constantly

permeated with water in large quantities, that its whole mass is loosened, and falls away from season to season to a very great extent. The effect of this upon our present subject has an important bearing in two particulars.

(*a*) It would entirely do away with the supposition that any part of this "talus" now lying immediately against the entrance of the cave, was existent during the glacial epoch, and hence that the boulders relied upon by Messrs. Tiddeman and Dawkins cannot be *in situ* as therein deposited, and

(*b*) That the floor level of the cave has been constantly rising, having been reformed upon the masses of limestone which had fallen from the roof. These two important deductions are amply verified by the present appearances of the cliff and cavern.

(3) In every instance with which I am acquainted the clay which fills the caverns of Yorkshire and Derbyshire has been introduced by the agency of running water, generally by "pot holes," which communicate with the surface, and which in wet seasons give passage to large volumes of water laden with detritus, a portion of which is deposited in such parts of the underground channels as are favourable to its accumulation. Such clays are likely to be laminated, because of the mode af their deposition, *at intervals,* which allowed one layer to harden before another was deposited upon it. The clay which is found filling the Victoria Cave is precisely such as we should look for under the circumstances before described. The glacial drift deposited clay of the boulder type upon the surface; and the rains of ages dissolved it away and carried it down these fissures into the cavern, where a portion of it remained. That the cave is of the precise character here indicated I can certify, for I was able to get to the end of it after going for a considerable distance through mud and water—the roof being only about two or three feet from the floor. I there found that the end

of the cave was an oval dome, which continued upwards in a circular shaft as far as my sight could reach; and I found the sides in many places dotted with clay, and the ledges, as high as I could reach, thickly covered with it, of the precise colour and appearance of that filling the cave. The surface under the dome, or "pot hole," had also many pebbles scattered over it, and these were of the same rocks as the large drift boulders occurring on the surface. Much water was coming down this shaft, as also in several other places in the Victoria Cave, and it disappeared again through the floor, and especially at a point near the entrance, where a large aperture showed that the cavern continued to a much lower level than the lowest point yet reached.

(4) Mr. Tiddeman's section and description gives the stratification of clays in the interior of the cave as regular and as consisting of (*a*) lower cave earth (*b*) bone bed containing bones of older mammals (*c*) laminated clay, and (*d*) upper cave earth.

So far as I can learn, however, I cannot agree that this correctly describes the interior of the cavern. I should adopt in preference the following description:

(*a*) Lower yellow clay, the old floor of occupation of the cave about 1 foot thick containing large quantities of coprolites, the dung of the older mammals, whose bones occur plentifully in it, and I believe this seam of clay will be found to occur throughout the cave at varying levels.

(*b*) Laminated clays above and below the large masses of limestone which have fallen from the roof and which have been deposited by water from the surface. This clay contains pebbles, and occasionally larger pieces of rocks, such as occur on the surface.

(*c*) Cave earth on the surface of (*b*), at varying levels, and which contained Roman remains. This earth occurred generally at parts of the cavern where the roof is not much fissured, and where consequently it has not fallen.

Now Mr. Tiddeman describes this upper clay or cave earth as gradually thickening from the entrance towards the rear of the cave, and he places a laminated clay between it and the lower cave earth, which he also describes as dipping gradually from the entrance towards the rear of the cavern, and he distinctly pronounces this laminated structure to be evidence of its glacial origin, and he supposes it to have been deposited in the following manner:—

"Let us imagine a glacier or an ice sheet passing by the mouth of the cave and partly blocking the entrance with its rubbish * * * * the glacier melts by day and usually (though not always) freezes by night. The moraine rubbish hinders the coarser debris from entering the cave, but gives passage to glacier water charged with fine mud. The glacier by its grinding keeps the water charged with mud, and the frequent change from daily flow to nightly inaction, gives rise to that close lamination, which is its characteristic feature."

With all respect to the opinion of so high an authority, I altogether deny the possibility of this being the true explanation, for the following reasons:—

(*a*) Glaciers do not deposit fine mud in lateral moraines 150 or 200 feet above the base of the glacier; and even if they did, it is not possible that such mud could flow into a cavern closed at its end as here described.

(*b*) The laminated clay occurs in the cave on the surface, *at a point where it can only be of most recent origin,* near the dome which terminates in a "pot hole," and by which it has evidently been only recently introduced; *and similar clays occur in other caverns, where glacial action as above described could not have obtained.*

After a most careful examination I am perfectly satisfied that Mr. Tiddeman has overrated the importance of this laminated clay, and that his theory is altogether erroneous.

Mr. Tiddeman describes the "talus" as having fallen from the cliff above, and that it continued upwards, so as formerly to close the entrance of the cave, which is so far quite correct. He afterwards describes the most recent discovery as being brought to light below all the "talus" at the mouth of the cave, viz. a bed of tenacious clay with scratched silurian and other boulders, resting on the edges of the beds containing the remains of the older mammals, and dipping outwards at an angle of 40°. Professor Hughes had suggested to him the possibility of this boulder clay not being in its original position, but that it might have fallen from the cliff; but Mr. Tiddeman thinks this impossible. He "considers that it seems likely that it is the remnant of the moraine (lateral or *profonde*) which dammed up the mouth of the cave, and prevented anything but fine sediment from entering it during the glacial period" (as before cited), and it is upon this supposition that the more important one is based, viz.; that the remains found recently are of pre-glacial age.

I am sorry again to have to differ from Mr. Tiddeman, but I am perfectly convinced he is in error, and that there is at present nothing at all resembling the boulder drift clay to be seen at the entrance of Victoria Cave. I examined the whole section very carefully, and had some of the boulders, which are very few, got out, and I believe they are fully to be accounted for without any need to assume glacial action. They are of black limestone, silurian flags, whinstone, and millstone grit, such as occur plentifully on the surface of the scar, and where they were probably deposited as drift. At the point where the animal remains so plentifully occurred is probably an old entrance of the cavern, on a much lower level than the original entrance when the cave was first discovered. Just within this, in a water-worn hollow, the remains occurred in the yellow clay or cave earth, which abounded with the dung of the animals. Mr. Jackson says there was a sill stone in front, evidently worn to smoothness by the frequent passing of the animals; and just beyond this point there is an opening into a cavern, lower still than the lowest point yet reached, and into which the drainage of the cavern now flows. Everything points to the probability of a large quantity of clay having poured out among the talus at this place in very wet seasons, and the clay itself as now found is a pasty, tenaceous mass, unlike any naturally deposited clay with which I am acquainted.

Amongst the boulders I found one which is of itself sufficient to account for the occurrence of boulders without any need of a glacial theory.

It is a smoothly rounded limestone boulder, precisely such as is formed by the rolling action of falling water in "potholes," and which cannot have had any glacial origin. This boulder occurring as it did with others of black limestone and silurian slate, is to my mind perfectly conclusive.

The point at which the last discovery of older bones was made, is at least 30 feet in advance of the original entrance, and was covered in front with talus. It is however a portion of the solid cliff, which has remained after all the rest had fallen away, and its evidence is conclusive that a very large mass has thus fallen since these remains were there deposited. The fall of this large mass, containing in its fissures clay and boulders from the glacial drift which certainly passed over it, would be amply sufficient to account for all the drift boulders which actually occur in the talus.

I visited Victoria Cave three years ago, when the operations had newly commenced, and I then found at the top of the talus precisely similar boulders to those which have recently attracted so much attention, and I believe they will be found throughout the debris. For all these reasons, therefore, I submit that there is no ground for the theory of glacial action as put forth by these gentlemen, but on the contrary that the filling of the Victoria Cave was the work of long ages, by the action of running water, and that there is no reason to suppose that the remains found in it are older than the glacial epoch.

The PRESIDENT exhibited a syphon barometer, the peculiarity of which consisted in the introduction of a small quantity of sulphuric acid over the ends of the mercurial column.

Mr. SPENCE, F.C.S., communicated to the Society the result of an experiment in heating a diamond, which will considerably modify the general impression as to that gem being combustible only at an extremely high heat.

A friend of his had brought over a number of diamonds from the African mines. Some of these were what is called "off colour," not being purely white, and he put one of these into Mr. Spence's hands to try some experiments for displacing the colour if practicable.

This diamond, the size of a small pea, was immersed in fire-clay in a small crucible, the clay being mixed with a little carbonate of soda and hydrate of lime, the crucible was then placed in a muffle, and for three days and nights exposed to a heat, which, at no time, was beyond a low cherry red. After cooling, the crucible was broken, and the lump of hardened fire-clay was carefully broken up to extract the diamond; after two or three fractures of the lump an impression or hole in the indurated clay was discovered just at the spot where the diamond should have been, but not a vestige of the precious stone remained.

The only explanation of its departure that seems feasible is, that the soda carbonate, causticised by the lime hydrate, had by its affinity for carbonic acid assisted the oxygen of the atmosphere getting through cracks in the clay, to oxidise the pure carbon of which the diamond is composed at a vastly lower temperature than would in ordinary circumstances have been required—at all events this gem was entirely volatilised at a very low red heat.

Ordinary Meeting, April 1st, 1873.

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

Mr. J. S. Kipping and Mr. J. Sidebotham were appointed Auditors of the Treasurer's Accounts.

"Note on an Observation of a small black spot on the Sun's disc," by JOSEPH SIDEBOTHAM, F.R.A.S.

As there is again some speculation as to the existence or an intra-mercurial planet, and every little fact bearing on the subject may be of value, I have referred to my diary and find that on Monday, March 12th, 1849, our late member Mr. G. C. Lowe and I saw a small circular black spot cross a portion of the sun's disc. We were trying the mounting and adjustments of a 7-inch reflector we had been making, and used an ink box between the eye-piece and the plane speculum. At first we thought this small black spot was upon the eye-piece, but soon found it was on the sun's disc, and we watched its progress across the disc for nearly half an hour. The only note in my diary is the fact of the spot being seen — no time is mentioned, but if I remember rightly it was about 4 o'clock in the afternoon.

Mr. BAXENDELL, on behalf of Mr. SIDEBOTHAM, F.R.A.S., exhibited a knife, the blade of which is steel, the bush at the handle brass, and the handle itself copper, all coated with nickel, beautifully polished. In a letter which Mr. Sidebotham had received from Professor Hamilton L. Smith, of Hobart College, Geneva, N. Y., the writer suggests the use of iron or bell metal specula, coated with nickel, for reflecting telescopes. He says, "I ground and prepared a bell metal speculum, which I coated with nickel, and this, when polished, proved to be more reflective (at least I thought so) than speculum metal. The two objects which I sought were—first to have a polished surface unattackable by sulphuretted hydrogen (this, for example, is not injured by packing with lucifer matches), and secondly, for large specula, doing most of the work by the turning-tool and lathe. I really think a large, say 3 feet, mirror, coated with nickel, but cast of iron, and finished mostly in the lathe, while it would not cost the tenth of a similar sized speculum metal, would be almost equal to silvered glass of the same size, and vastly more enduring as to polish.

Professor WILLIAMSON, F.R.S., referring to Mr. Binney's remarks at the meeting of March 4th, said that Mr. Binney, after pointing out that I had identified a certain type of stem-structure with *Asterophyllites*, and that Professor Renault had discovered the same structure in *Sphenophyllum*, Mr. Binney proceeds to say, "I am not in possession of the facts from which the two learned professors came to *such different conclusions*, but I am inclined to consider the singular little stem as belonging to a new genus *until the leaves of Sphenophyllum or Asterophyllites are found attached to it.* When this comes to pass of course there can be no doubt of the matter." I have italicised the two important points in the preceding quotation. In the first place I cannot understand how Mr. Binney has overlooked my statement, made primarily in the Proceedings of the Royal Society, and repeated in the last number of the Proceedings of your meeting of February 4th, that I *had* "got a number of exquisite examples showing not only the nodes, *but verticils of the linear leaves so characteristic of the plant.*" These leaves I have obtained attached to the stems in question in at least a dozen examples. Secondly, Mr. Binney considers that my conclusions and those of my friend Professor Renault are *different*, whereas they mutually sustain each other in the strongest possible manner. Nearly every writer who has dealt with these subjects has recognised Annularia and Sphenophyllum as genera of plants having the closest possible mutual affinity; they are invariably arranged side by side. Brongniart, in his *Tableau des genres de végétaux fossiles,* says of Sphenophyllum that "great attention is necessary in order to avoid confounding it with certain species of Asterophyllites;" and again he says of the fructification of Sphenophyllum that it "is too analagous to that of Asterophyllites to allow of any doubt as to the affinities of these two genera" (*loc. cit.* p. 52). Mr. Carruthers, in his lecture "On the Cryptogamic Forests of the Coal Period," says of Asterophyllites, Annularia, and Sphenophyllum, "it is possible they may be found to constitute three genera, but there are no characters possessed by the leaves which prevent them belonging to one well defined genus." (Proceedings of the Royal Institution of Great Britain for April 18th, 1869.) I could easily multiply similar illustrations of my statement, but I have probably said enough to prove that, so far from the "conclusions" of Professor Renault and myself on this point being opposed and "different," we have been independently and unknown to each other arriving at what are practically identical conclusions respecting the stem under consideration.

E. W. Binney, F.R.S., said that after having heard Professor Williamson's remarks his opinion expressed at the meeting of the Society on the 4th day of March last was not altered. *Sphenophyllum* and *Asterophyllites* have always been considered as distinct genera of plants, and they are so described in Professor Schimper's great work. Professor Renault writes, "Si je ne me trompe ces tiges curieuses appartiennent à des sphenophyllum, du moins c'est ce que j'ai écrit dans les comptes rendus de l'académie en 1870." And again "Je n'ai pas encore rencontré de feuilles adhérentes au rameau ce qui m'a empèché de déterminer spécifiquement ce sphenophyllum." When he (Mr. Binney) sees the leaves whether of *Asterophyllites* or *Sphenophyllum* attached to the curious little stem he will be convinced of their connection, but until then he will hold to his original opinion.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 25th, 1873.

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.
ALFRED BROTHERS, F.R.A.S.

Vice-Presidents.
JOSEPH BAXENDELL, F.R.A.S.
SAMUEL BROUGHTON.

Treasurer.
THOMAS CARRICK.

Secretary.
GEORGE VENABLES VERNON, F.R.A.S., F.M.S.

"Rainfall at Old Trafford, Manchester," by G. V. VERNON, F.R.A.S.

The total amount of rainfall in 1872 was 50·692in. against 33·288in. in 1871.

The amount which fell in 1872 was 14·883in. above the average of the last seventy-nine years, and in excess of any rainfall at Manchester between 1793 and 1872. Referring to the observations made by Mr. Walker from 1786 to 1793, we find that in 1789 he collected 50·998in., and in 1792 55·250in. Since this period the rainfalls exceeding 40in. have been 1822, 44·767in.; 1823, 42·941in.; 1828, 45·267in.; 1830, 40·861in.; 1833, 41·677in.; 1836, 45·351in.; 1841, 41·190in.; 1845, 41·415in.; 1847, 43·555in.; 1848, 45·230in.; 1852, 45·730in.

At the time Mr. Walker registered his excessive falls, the mean annual temperature was lower than it has been since, and reference to my paper, "Inquiry into the question Whether Excess or Deficiency of Temperature during part of the year is usually compensated during the remainder of the same year" (Memoirs, vol. 2, third series, p. 424), will show that between 1781 and 1791 a lower mean temperature prevailed than any we have had since. The other years in which excessive rainfall occurred, 1822, 1823, 1828, 1830, 1833, 1836, 1841, 1845, 1847, 1848, and 1852, appear to have been irregular as regards temperature; the years 1822, 1828, 1833, 1841, 1847, 1848, and 1852, had a temperature above the average, whilst 1823, 1830, 1836, and 1845, had a temperature below the average. Taking the average rainfall of each of these series it appears that the heaviest rainfall occurred during the warmer years.

Returning again to the year 1872, the rainfall rises above the average in every quarter, especially in the third, the excess in that quarter reaching 7·104in.; in the last quarter the excess was very small.

Every month excepting May, August, November, and December, had a rainfall above the average, the falls of June, July, and September being most remarkable, each of these months having a fall of more than double the average.

The very heavy fall in the middle of July was accompanied by a great flood in the Medlock here, and there is every certainty that such a rainfall again must be accompanied by a similar flood and great destruction of property.

What would have occurred if the rainfall in July had been like that of 1828, 11·480in., or 3·822in. in excess of what fell in July, 1872?

Rain fell on 40 days in excess of the average of the last 10 years (Proceedings, vol. 11, p. 184); rain fell upon the greatest number of days in January, June, September, and October, and upon the least in April.

Whatever was the disturbing cause which produced the excessive rainfall, examination of the excess of each quarterly period shows that it went on increasing until September, and then apparently declined to the end of the year, the excess in question being—March quarter, 2·808in.; June quarter, 4·794in.; September quarter, 7·104in.; and dropping down in the December quarter to 0·177in. only.

As regards the temperature of the year, it was above the average in every quarter, Greenwich giving

March quarter.........	+ 5·0°	in excesss of the average of 101 years;
June quarter	+ 0·5°	
September quarter ...	+ 1·5°	
December quarter ...	+ 1·7°	

so that in the case of last year a high temperature has accompanied the excessive rainfall.

OLD TRAFFORD, MANCHESTER.

Rain Guage 3 feet above the ground, and 106 feet above sea level.

Quarterly Periods. 1871.	Quarterly Periods. 1872.	1872.	Fall in Inches.	Average of 79 Years	Difference.	No. of Days Rain fell in 1872.	Quarterly Periods. 79 Years	Quarterly Periods. 1872.	Quarterly Periods. Difference.
Days	Days		In.	In.	In.		In.	In.	In.
38	56	January ..	4·255	2·537	+1·718	22	7·240	10·048	+2·808
		February..	3·018	2·409	+0·609	18			
		March	2·775	2·294	+0·481	16			
44	50	April......	2·975	2·062	+0·913	9	7·226	12·020	+4·794
		May	2·145	2·301	−0·156	17			
		June	6·900	2·863	+4·037	24			
52	59	July	7·658	3·557	+4·101	17	10·376	17·480	+7·104
		August....	2·784	3·501	−0·717	19			
		September	7·038	3·318	+3·720	23			
48	63	October ..	4·404	3·891	+0·513	22	10·967	11·144	+0·177
		November..	3·774	3·784	−0·014	21			
		December..	2·966	3·292	−0·326	20			
182	228		50·692	25·809	+14·883	228	35·809	50·692	+14·883

Ordinary Meeting, April 15th, 1873.

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

Mr. William Thomson was elected an Ordinary Member of the Society.

Mr. FRANCIS NICHOLSON, F.Z.S., exhibited two fine eggs of the golden eagle (Falco chrysaëtos) taken the previous week from a nest in the north of Scotland. Fortunately some of the large landed proprietors both in Scotland and Ireland are now preserving this noble bird from persecution during the breeding time, so that it is not likely to be thoroughly exterminated at present, but British taken eggs are difficult to obtain and are rare in collections.

The following letter from Mr. WILLIAM BOYD DAWKINS, F.R.S., was read:

As Secretary of the Committee of the British Association for carrying on the exploration of the Victoria Cave, I am obliged to notice the "Notes on Victoria Cave," by Mr. W. Brockbank, published in the Proceedings, March 10th, 1873, pp. 95 *et seq.* The notes in question are based partly on Mr. Brockbank's examination of the cave during two visits with an interval of two years between them, partly on the facts recorded by Mr. Tiddeman and myself, and partly on a ground plan constructed by our superintendent Mr. Jackson, for the Exploration Committee, that is not yet published. I submit that until the work of the Committee to which the cave has been handed over by the kindness of the owner be finished, and the observations, to which Mr. Brockbank has had no access, be recorded, his notes must of necessity be imperfect and liable to error. How much he is in error as to matters of fact may be estimated by the examination of the statement, p. 97 — "the day before my visit a mass of at least 100 tons had fallen from above the face of the Victoria Cave." Mr. Jackson writes me that not even a mass weighing one ton, although two blocks possibly of

10cwt. each, had fallen. The statement at p. 96, in which I am made to differ with Mr. Tiddeman as to the presence of the pleistocene mammalia inside the cave is altogether unfounded, and the inference that I "varied my description" after my paper came before the Society is negatived by the fact that the abstract in question was printed for private circulation in 1872. The remains occur at the entrance and extend both inside and outside the cave, as I pointed out in my diagram. These are merely two out of many points which have been raised, and which do not lead me to alter my conviction that the stratum containing the mammalia is of preglacial age, or to undertake any responsibility as to the views which I have *not* advanced. Were I to discuss all the points which have been raised, I should anticipate the Report of the Committee to the British Association. If these hasty and necessarily imperfect observations were not calculated to throw discredit on the Exploration, I should not trouble the Society with this note.

"On some Improvements in Electro-Magnetic Induction Machines," by HENRY WILDE, Esq.

[An abstract of this paper will appear in the next number of the Proceedings.]

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Extraordinary Meeting, December 11th, 1872.

JOSEPH SIDEBOTHAM, F.R.A.S., in the Chair.

Mr. JAMES M. SPENCE exhibited a large and interesting collection of natural history and other objects from Venezuela. Mr. Spence had lately returned from that country, in which he spent eighteen months, during which time he accumulated a very extensive collection.

The natural history collection contained a number of hunters' skins of the larger animals of prey and of the chase; but the great wealth and beauty of the fauna of the country was best illustrated by the extensive collection of birds, which is probably the best ever got together, and embraces examples of nearly all the tribes found in the Venezuelan Republic.

The economical portion of the collection was of great interest and value, chiefly from its extent and the care which had been exercised in its collection and transportation, and the valuable notes of Dr. Ernst of Caracas, which accompany it, rendered it still more valuable. Specimens of the vegetable and mineral productions of Venezuela were to be seen in great number and variety.

Among the plants exhibited was a small collection of *Characeæ* named by Dr. Ernst, but the chief interest was in a small collection of plants gathered by Mr. Spence on the summit of Mount Naiguati.

This mountain, whose altitude is nearly 9,500 feet, is the highest in Venezuela, and was regarded as almost inaccessible until Mr. Spence and five companions made a successful ascent in April, 1872. A species of grass allied to the bamboos and new to science was one of the results of this ascent.

The exhibition also included an assortment of interesting curiosities of native manufacture, recent and ancient. There were goblets, drinking cups, and flasks more or less finely carved out of cocoa nuts, some mounted in silver; and a series of delicately worked cups and bowls of calabash.

From the State of Trugillo Mr. Spence has brought three curiously shaped vessels obtained from Peruvian burial places.

The collection remained open to the public for some days, and was visited by a large number of persons.

January 27th, 1873.

Professor W. C. Williamson, F.R.S., President of the Section, in the Chair.

"Description of Minerals and Ores from Venezuela," by John Plant, F.G.S.

The collection of minerals acquired by Mr. J. M. Spence during his residence at Caracas, and on several journeys along the coast, came from the provinces of Barcelona, Bolivar, Carabobo, and Coro, with a few obtained from the regions of the River Orinoco and Lake Maracaibo. The collection contains gold in quartz of very rich character, argentiferous ores, green and blue carbonates of copper, copper pyrites, galena, iron ores of various kinds, carbonaceous minerals, calcites, silicas, and rock specimens of gneiss, mica, talc schists, kaolin, hornblendic rocks, and serpentine with a few imperfect fossil and silicified woods.

The gold quartz of the richest kind, came from the Province of Guayana, where vast regions of auriferous rocks occur ; and where also gold is found in small grains, flakes, and nuggets of all sizes from an ounce to many pounds weight, in a clay from two to eight inches thick, as well as in a red peroxidated iron earth, both probably alluvial drifts. The quartz veins are richly impregnated with gold in crystals and strings, as may be seen in specimens in the collection. Other specimens of the gold rocks come from the Isle of Aruba, and Loro Estado, Tacasumino.

The argentiferous ores are galenas and cupiferous, and are not of very great richness; they are from La Guaira, Cumaná, and Coro, where decomposed galenas are worked for silver.

The copper ores include 20 specimens from mines that have been worked with profit, one of which, the Aroa mines in the province of Yaracui, is the most famous for the superior richness of its carbonates. The specimen of cuprite from this mine or Quebrada has some long and beautiful crystals of olivenite with cubes of strontian, and from Aragua are specimens of pyrargyrite or red silver ore; others from Caracas, Coro, and the river Tui, include malachites and a native sulphate of copper, probably a crystallisation from the waters issuing from the mines. The chalcopyrites are neither numerous nor very good; the best comes from the Aroa mines, the small granular pyrites appears to be most abundant in a decomposing gneissoze rock.

The galenas are from mines at Los Teques, Aroa, and Campano, several are pseudomorphous crystals in filmy aggregations, interesting specimens for the mineralogist.

The iron ores include specimens of pyrites (mundic) which in Venezuela appears to be as abundant as in most palæozoic regions, ten of the samples are rich, and would be profitable if the cost of mining is not too expensive at Barquisimeto, Caracas, and the Aroa mines.

The hæmatites include specular, micaceous, and red iron ores, all comparable to the best European ores. The limnites comprise bog-iron ore of recent formation and a brown amorphous ore. The siderites include an aggregation of tabular crystals from Caracas, probably a carbonate of protoxide of iron valuable in making steel, and massive clay ironstones from the districts of Corui Machate, where coal is also worked. The crystallised and compact magnetites come from the same place. A thin vein of brown siliceous ironstone has its surfaces covered with minute fragments of clear quartz, singular and beautiful under the microscope.

The carbonaceous minerals are coals, graphite, sulphur, asphaltum and petroleum. The coals are from Nuevo Mundo, where Mr. Spence has proved the existence of workable coals, the Island of Toas in the Lake Maraciabo, and a cannel coal from Coro, with several black shales from these localities. These coals are undoubtedly of excellent quality, and from report can be worked economically; their age is at present unknown from the want of any proper geological survey, and in the absence of fossils of any kind in the shales in this collection; in all probability however the Venezuelan coals are of true carboniferous age.

The graphite from Caracas is an impure amorphous earthy kind, in schists of two inches thick, occurring in talcose and micaceous rocks. The sulphurs are massive and of good quality from Campano, Cumaná, and Coro. Asphaltum and its varieties are reported to be found on the coasts in great deposits and in springs: the specimens in the collection are of excellent quality.

The twelve rock specimens of quartz crystals include some of equal purity and size to those obtained from Brazil. The marbles are of inferior quality and quite devoid of colour and beauty; but in the International Exhibition of 1862 some excellent green and red marbles were shown.

The predominating rocks of the mountain ranges in Venezuela are palæozoic, metamorphosed talcose and chloritic slates, with great layers of gneiss; and within this range along the line of faults and in veins, are found an endless variety of minerals, of which the collection contains asbestos, serpentine, talc, hornblende chlorite, kaolin, felspar, and selenite.

Amongst the comparatively recent rocks are stalactites, salt, marl, alum, gypsum, and many calcareous deposits from the sea shores and fresh water lakes.

The special collection made by Mr. Spence during a visit to the Island of Orchilla is interesting to the geologist. It contains sufficient specimens to decide the main geological character of the island to be entirely metamorphic gneiss, overlaid with modern calcareous tufas.

The collection includes a number of crude guanos, phosphates of lime, alumina and *urao,* a sesquicarbonate of soda —all of commercial value and sources of prosperity if efficiently worked.

February 24th, 1873.

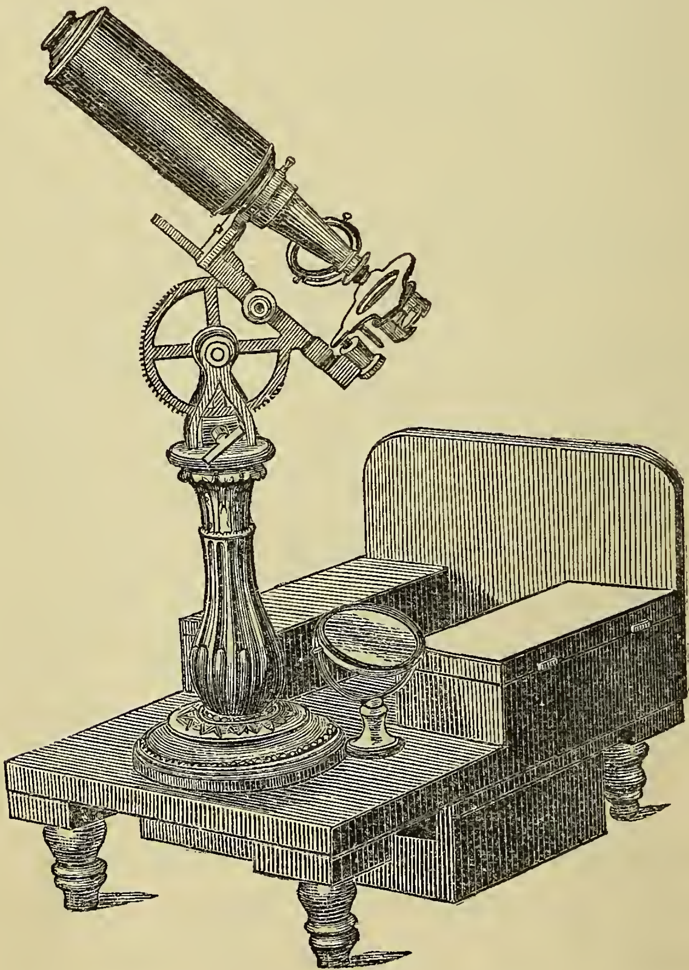
JOSEPH SIDEBOTHAM, F.R.A.S., in the Chair.

Mr. HARDY made a communication to the Section respecting the occurrence of one of the few large bivalve mollusca within the limits of the Manchester district, the species in question, Unio tumidus of authors, having been observed in considerable numbers in the canal at Barton, a little beyond the aqueduct, and in several places between there and Stretford: a few dead shells were also found in the river.

References were given to works on local conchology in which no notice of this shell as an inhabitant of the district was to be found. Allusion was also made to the record of a single living example of another species of the same genus, the U. pictorum of Linne, in the canal near Romiley; and during the conversation which followed the reading of the paper Mr. T. S. PEACE announced that this latter shell had since been collected in quantity in the same canal some short distance beyond Marple; thus establishing satisfactorily the occurrence of two out of the three British species of Unio, the third not being at all likely to inhabit any of our rivers in their present condition; although the specimens collected at Barton were many of them much larger than others of the same species collected in more southern and apparently more favourable localities, and exhibited to the meeting.

JOSEPH SIDEBOTHAM, F.R.A.S., exhibited an old microscope sent by Mr. Rideout, and explained its construction. The workmanship of the brass-work was very beautiful, and the various motions and appliances much admired; he also read a letter from Mr. DANCER, who for several reasons thought that the microscope was not more than 120 years old, and was made by the elder Adams. He said that many of these old microscopes in finish of brass-work, good fitting and screws would compare very favourably with instruments of recent construction, and that the appliances and apparatus of one of the complete microscopes would surprise a microscopist of the present day; he would find many parts and adaptations which are generally supposed to be of modern invention.

The stand of the microscope is of ebony, and is a fine specimen of geometrical turning. The optical part is of course very poor, and inferior to the very cheapest achromatic instrument of the present day.



Annual Meeting, April 29th, 1873.

E. W. Binney, F.R.S., F.G.S., Vice-President, in the Chair.

The following Report of the Council was read by one of the Secretaries: —

The Council have the satisfaction to report that a further improvement has taken place in the financial position of the Society, the Treasurer's account showing that the general balance on the 31st of March last was £407 1s. 4d. against £340 0s. 3½d. on the 31st of March, 1872.

The number of ordinary members on the roll of the Society on the 1st of April, 1872, was 174, and six new members have since been elected; the losses are, deaths, 4; resignations, 4; and defaulters, 3. The number on the roll on the 1st of April instant was, therefore, 169. The deceased members are John Francis, George Cliff Lowe, Samuel Emanuel Nelson, and Joseph Jordan.

Mr. George Cliff Lowe, whose death was the result of an accident in the United States, was known to many of our members for his general and accurate acquaintance with the natural sciences, but more particularly that of astronomy.

Possessing a love of knowledge for its own sake, and a comprehensiveness of mind to deal with other besides purely physical subjects, he took great interest in the leading philosophical questions of the present time, and his opinions were generally to be found on the side of progress. Although not a frequent contributor to the literature of science, Mr. Lowe had an acuteness of perception combined with a degree of manipulative and artistic skill which made his co-operation and judgment much valued and sought for by others.

We thus find Mr. Lowe's name associated with that of Professor F. C. Calvert, F.R.S., in a joint paper "On the Expansion of Metals and Alloys," published in the Proceedings of the Royal Society, vol. 10, 1860. Mr. Lowe was also associated in business with our member Mr. Wilde as an electrical engineer, and suggested to him the plan of exciting a number of electromagnetic machines by the current from one machine, instead of employing a separate exciting machine for each. With his philosophical attainments Mr. Lowe combined estimable moral qualities, the most conspicuous of which were the amiability of his character and the generosity of his disposition.

Mr. Joseph Jordan, F.R.C.S. Engl., was one of the oldest members of the Society, having been elected on the 19th of October, 1821. He was born in Manchester, and, with the exception of a short period when he was surgeon of the 1st Lancashire Militia, resided in Manchester all his life. He retired from active practice about nine years ago, when he was in the 76th year of his age. His name will be distinctly remembered as the founder of provincial medical schools. As early as 1814 he gave regular courses of lectures on anatomy, with demonstrations and dissections, to classes of medical pupils and students. He was the first provincial lecturer and teacher whose certificates were accepted and recognised by the examining bodies in London. The Apothecaries' Hall began to accept his certificates in 1817, and the College of Surgeons in 1821. In 1826 he built a medical school in Manchester at his own cost, and, besides its lecture hall, provided it with one of the most commodious and best-fitted dissecting rooms in England, and transferred to it his own valuable museum, containing nearly 4,000 anatomical specimens and morbid and other preparations. He subsequently placed this museum in the Manchester Royal School of Medicine. He devoted himself to the arduous duties of a public lecturer for twenty years. On his retiring from the chair a public dinner was given to him by his friends, in October, 1834, attended by almost every medical man of reputation in Manchester, and a handsome and valuable testimonial in silver plate was presented to him from his friends and pupils.

Mr. Jordan had further claims upon public regard as a large benefactor to suffering humanity by professional unpaid services. In his private practice, extending over more than fifty years, Mr. Jordan ever showed a special devotion to the relief of the sickness and suffering of the poor. His great professional skill, often unpaid, and even supplemented by a liberal purse, and that genuine kindness which ever doubles the value of a gift, won for him the blessings of thousands. Nor was his philanthropy less conspicuous in official positions. About 1819 he aided largely in founding the Lock Hospital, for unfortunate women, of which he was the surgeon or consulting surgeon till he finally retired from practice. He was always a steady benefactor to the institution, in wise counsel and liberal donations. In 1835 he was appointed an honorary medical officer of the Royal Infirmary, and long filled the honourable position of its senior surgeon with the highest credit to himself and with great benefit to the institution and the community at large. Within its walls he often performed some of the greater as well as the more delicate operations of surgery; his remarkable nerve and steadiness and precision of hand admirably qualifying him for these duties. He invented a most beautiful little lamp to obtain a magnified view of the membrane tympani and other organs, for which the Society of Arts awarded their silver medal. His clinical lectures in the hospital wards always attracted a large and attentive following of the pupils and students, and a few years ago a very numerously signed testimonial was presented to him by the pupils of the Royal Infirmary for these lectures. He was a most eloquent and interesting lecturer, and his great and long experience enabled him to illustrate his lectures with cases bearing upon the subject, which rivetted the attention and increased the knowledge of his hearers.

Mr. Jordan was a valued contributor to medical science by a new method of treating false joints. A difficult class of surgical cases is presented when the fractured surfaces of bone refuse to reunite, or else unite so badly as to cause great suffering and even loss of the use of a limb. For the cure of these so-called "false joints," and the effecting of a speedy, safe, and satisfactory reunion of the fractured bones, Mr. Jordan, in the year 1854, invented and applied a new and exceedingly simple mode of treatment. His plan was recognised not only by his professional brethren in Manchester, but in June, 1856, the eminent Paris surgeon, Professor Nelaton, in a public lecture to his class, described the method as "a happy innovation, and one capable of receiving numerous applications." The priority of Mr. Jordan's claim to this invention was beyond doubt. Finding, however, that a French surgeon was introducing the method as his own, Mr. Jordan proceeded to Paris in 1860, where he published in French a treatise, illustrated with three plates, entitled "Traitement des Pseudarthroses par l'Autoplastic Periostique," which not only effectually extinguished any rival claim, but comprised a full and clear exposition of the mode of treatment in all its successive stages, and gave to the author a European reputation.

It was at one time proposed that some mark of her Majesty's favour should be solicited by Mr. Jordan's friends, to honour one who had conferred so much credit upon his profession in Manchester, and so much advantage upon the community at large; but the modesty of the veteran self-sacrificing surgeon shrunk from this distinction, and at his instance the movement was stopped.

In the last annual report it was stated, with reference to the benefaction which the late Natural History Society provided for the promotion of the study of Natural History in Manchester, under the guardianship of the Literary and Philosophical Society, that the Owens College would at once proceed to endeavour to sell the Peter-street site, to be delivered up in June, 1873, for money or for rent, as may seem best. In the latter case it had been agreed between the commissioners and the college that the college should pay £60 per annum as interest at 4 per cent. on £1,500 until the principal shall have been paid over to the society. The Council have now to report that the Peter-street site has not yet been sold, but on the 20th of November last a letter was addressed by Mr. Darbishire to Mr. H. A. Hurst, the treasurer of the Microscopical and Natural History Section, stating that by an arrangement made on that day between the commissioners of the Peter-street Museum and the Owens College the Museum Trust in the hands of the college will pay to the Philosophical Society, for the present, interest upon the sum of £1,500 at 4 per cent. from that date. The first half-yearly payment will therefore become due on the 20th of May next.

At a meeting of the Council held on the 7th of January last, a committee was appointed to consider and report upon the desirability of incorporating the society, and of acceding to an application of the Manchester Geological Society for permission to hold its meetings and keep its library within this society's buildings. Resolutions embodying the recommendations of this committee will be submitted this evening for the approval of the members of the society.

In May of last year, Dr. R. Angus Smith, F.R.S., a vice-president of this society, attended on behalf of the society the centenary celebration of the foundation of the Royal Academy of Sciences of Belgium, and a medal has this day been received commemorative of this interesting event.

The following papers and communications have been read at the ordinary and sectional meetings of the society during the session now closing:—

October 1st, 1872.—"On the Composition of Ammonium Amalgam," by R. Routledge, B.Sc.

October 29th, 1872.—On a Peculiar Fog in Iceland, and on Vesicular Vapour," by R. Angus Smith, Ph.D., F.R.S., V.P.

November 4th, 1872.—"On the Flora of Alexandria (Egypt)," by H. A. Hurst, Esq.

"On the Destruction of the Rarer Species of British Ferns," by Joseph Sidebotham, F.R.A.S.

November 12th, 1872.—"Additional Notes on the Drift Deposits near Manchester," by E. W. Binney, F.R.S., F.G.S., V.P.

"An Account of some Experiments on the Melting Point of Paraffin," by Professor Balfour Stewart, LL.D., F.R.S.

November 26th, 1872.—"On the action of Town Atmospheres on Building Stones," by R. Angus Smith, Ph.D., F.R.S., V.P.

"On some points in the Chemistry of Acid Manufacture," by H. A. Smith, F.C.S.

December 10th, 1872.—"Observations of the Meteoric Shower of November 27th, 1872," by E. W. Binney, F.R.S., F.G.S.; Joseph Baxendell, F.R.A.S.; and Alfred Brothers, F.R.A.S.

"On some remarkable Forms of Stalagmites from Caves near Tenby," by W. Boyd Dawkins, F.R.S.

"On the date of the Conquest of South Lancashire by the English," by W. Boyd Dawkins, F.R.S.

"On some Human Bones found at Buttington, Montgomeryshire," by W. Boyd Dawkins, F.R.S.

"On the Electrical Properties of Clouds and the Phenomena of Thunder Storms," by Professor Osborne Reynolds, M.A.

December 11th, 1872.—"On a Collection of Natural History and other Objects from Venezuela," by James M. Spence, Esq.

December 24th, 1872.—"On the increase in the number of cases of Hydrophobia," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

January 7th, 1873.—"On the Action of Sulphuric and Hydrochloric Acids on Iron and Steel," by William H. Johnson, B.Sc.

January 21st, 1873.—"On an Apparatus for producing a high degree of Rarefaction of Air," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., Presiden

"On some Specimens of Anachoropteris," by E. W. Binney, F.R.S., F.G.S.

January 27th, 1873.—"Description of Minerals and Ores from Venezuela," by John Plant, F.G.S.

February 4th, 1873.—"On some Specimens of Asterophyllites," by Professor W. C. Williamson, F.R.S.

"On a large Meteor seen on February, 3, 1873, at 10 p.m.," by Professor Osborne Reynolds, M.A.

"Note on Meta-Vanadic Acid," by Dr. B. W. Gerland. Communicated by Professor Roscoe, F.R.S.

"Experiments on the Question of Biogenesis," by William Roberts, M.D.

February 18th, 1873.—"Account of Improvements in an Air Exhausting Apparatus," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

"Notes on supposed Glacial Action in the Deposition of Hematite Iron Ores in the Furness District," by William Brockbank, F.G.S.

"The Results of the Settle Cave Exploration," by W. Boyd Dawkins, M.A., F.R.S.

February 24th, 1873.—"On the occurrence of Unio tumidus in the Manchester district," by Mr. Hardy.

March 4th, 1873.—"Monthly Fall of Rain, according to the North Rain Gauge at Swinden, as measured by Mr. James Emmett, Waterworks Manager, Burnley, from January 1st, 1866, to Dec. 31st, 1872," by T. T. Wilkinson, F.R.A.S.

"On Ball Discharge in Thunderstorms," by Mr. S. Broughton.

"On Specimens of Iron manufactured by the old Bohemian Process, from Hematite Ores in the South of Europe," by W. Brockbank, F.G.S.

"On a Change in the Position of the Freezing Point of a Thermometer," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

"On the Influence of Acids on Iron and Steel," by William H. Johnson, B.Sc.

March 18th, 1873.—"On the Quality of the Water supplied to Manchester," by E. W. Binney, F.R.S., F.G.S.

"Observations on the Rate at which Stalagmite is being accumulated in the Ingleborough Cave," by W. Boyd Dawkins, M.A., F.R.S., F.G.S.

"On Methyl-alizarine and Ethyl-alizarine," by Edward Schunck, Ph.D., F.R.S.

"On the Transition from Roman to Arabic Numerals (so called) in England," by the Rev. Brooke Herford.

"Notes on the Victoria Cave, Settle," by William Brockbank, F.G.S.

"On an Experiment in Heating a Diamond," by Peter Spence, F.C.S.

March 25th, 1873.—"Rainfall at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S.

April 1st, 1873.—"Note on an Observation of a small Black Spot on the Sun's Disc," by Joseph Sidebotham, F.R.A.S.

"On the use of iron or bell metal Specula, coated with Nickel, for Reflecting Telescopes," by Professor Hamilton G. Smith, of Hobart College, Geneva, N.Y., communicated by Joseph Sidebotham, F.R.A.S.

April 15th, 1873.—"On some Improvements in Electro-Magnetic Induction Machines," by Henry Wilde, Esq.

Several of these papers have already been printed in the current volume of the Society's Memoirs, and others have been passed for printing.

No increase has taken place during the year in the number of Sectional Associates; nevertheless the Council consider it desirable to continue the system of electing such Associates during the ensuing year.

The Honorary Librarian reports that during the past year more pressing duties have prevented him from giving that attention to the Library which it requires, and he urges the early appointment of a paid servant to attend to the multifarious duties of the office. Since the last annual meeting there is no change to report in the number of learned bodies with which the Society is in the habit of exchanging transactions.

On the motion of Mr. J. A. Bennion, seconded by Mr. S. Broughton, the Annual Report was unanimously adopted.

On the motion of Mr. A. Brothers, seconded by the Rev.

JOSEPH FREESTONE, it was resolved unanimously—That the system of electing Sectional Associates be continued during the ensuing session.

On the motion of Mr. R. D. DARBISHIRE, seconded by the Rev. WILLIAM GASKELL, it was resolved unanimously—That the Council be instructed to take steps for procuring the incorporation of the Society under the provisions of the Companies Acts, and to apply to the Board of Trade for permission to omit the word "Limited" from the title of Incorporated Society.

On the motion of Mr. W. A. CUNNINGHAM, seconded by Mr. W. RADFORD it was resolved unanimously—That the application of the Manchester Geological Society for permission to hold its meetings and keep its library within this Society's buildings, in consideration of an annual payment, be acceded to, and the Council be authorised to negotiate the terms and conditions of such arrangement.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:—

President.
JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.

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.
REV. WILLIAM GASKELL, M.A.

Secretaries.
HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.
THOMAS CARRICK.

Librarian.
CHARLES BAILEY.

Of the Council.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
OSBORNE REYNOLDS, M.A.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
ALFRED BROTHERS, F.R.A.S.
REV. BROOKE HERFORD.

THOMAS CARRICK, TREASURER, IN ACCOUNT WITH THE LITERARY & PHILOSOPHICAL SOCIETY OF MANCHESTER.

Dr. FROM MARCH 31ST, 1872, TO MARCH 31ST, 1873. **Cr.**

Dr.

Date	Particulars		£ s. d.	£ s. d.
1872. April 1.	—To Balance in the Bank of Heywood Brothers & Co.		439 14 9	
	Less Balance due to the Treasurer		0 19 5½	438 15 3½
1873. Mar. 31.	—To *Members' Contributions:*			
	Arrears, 1871-2		16 16 0	
	Admission Fee, 1871		2 2 0	
	Members on the Roll, April 1, 1872, 174 at 42s.	£365 8 0		
	Deduct Compounders 4; Ditto Members deceased 1; Ditto in Arrear, March 31, 1873 8	33 12 0		
			331 16 0	
	Three Members elected, 1872	6 6 0		
	Three ditto ditto 1873	3 3 0		
		9 9 0		
	Less on Arrear	2 2 0	7 7 0	
	Six Admission Fees	12 12 0		
	Less on Arrear	2 2 0	10 10 0	
	Five Associates of Microscopical Section at 10s.		2 10 0	
	Two ditto ditto Arrears received		1 0 0	372 1 0
	To *Sale of Publications:*			
	Memoirs		2 12 9	
	Proceedings		0 8 6	3 1 3
	To *Sundry Income—Sectional Contributions:*			
	Microscopical		2 2 0	
	Physical and Mathematical		2 2 0	4 4 0
	To Interest allowed by Bankers			12 7 7
				£830 9 1½

	£ s. d.
Compound Fund	£98 15 0
General Balance	407 1 4
	£505 16 4

Cr.

Date	Particulars	£ s. d.	£ s. d.
1873. Mar. 31.	—By *Charges on Property:*		
	Chief Rent	12 13 6	
	Fire Insurance	3 7 6	
	Inhabited House Duty	6 7 6	
	Property Tax	2 16 8	25 5 2
	By *House Expenditure:*		
	Water, Gas, and Coal	21 0 7	
	Cleaning and Petty Expenses	5 8 10	
	Repairs, &c.	4 16 9	
	Tea and Coffee at Meetings	17 16 9½	49 2 11½
	By *Administrative Charges:*		
	Wages of Keeper of Rooms	57 4 0	
	Attendance on Sections	4 4 0	
	Postage and Parcels	11 18 0	
	Printing, Stationery, and Receipt Stamps	6 16 5	80 2 5
	By *Publishing:*		
	Memoirs, Printing, and Engraving	34 12 0	
	Printing Proceedings	34 9 6	
	Editing Memoirs and Proceedings	50 0 0	119 1 6
	By *Library:*		
	Periodicals, Binding Books, &c.	47 17 9	
	Subscription to Palæontographical Society	2 2 0	
	Ditto Ray Society	1 1 0	51 0 9
			324 12 9½
	By Balance in Bank of Heywood Brothers and Co.	455 17 7	
	Ditto in the hands of the Treasurer	49 18 9	505 16 4
			£830 9 1½

23rd April, 1873.
THOMAS CARRICK, TREASURER.

Audited and found correct, April 24th, 1873.
JOSEPH SIDEBOTHAM.
J. S. KIPPING.

The following paper was read at the Ordinary Meeting of the Society, held April 15th, 1873 :—

"On some improvements in Electro-magnetic Induction Machines," by Henry Wilde, Esq.

Soon after the announcement by the author (in 1866) of the discovery that electric currents and magnets, indefinitely weak, could, by induction and transmutation, produce magnets and currents of indefinite strength,* a number of electricians suggested other methods by which this principle could be exhibited and more powerful results obtained than those which the author described. The most interesting as well as the most useful of these suggestions was to augment the magnetic force of the elementary magnet, by transmitting the direct current from the armature of a magneto-electric, or an electro-magnetic machine through wires surrounding its own permanent or electro-magnet, in such a direction as to intensify its magnetism until, by a series of actions and reactions of the armature and the magnet on each other, an exalted degree of magnetism in the iron or steel was obtained.

This idea seems to have occurred to several electro-mechanicians almost simultaneously in England, Germany, and America. In a letter to the *Engineer* newspaper of July 20th, 1866, Mr. Murray, after referring to the author's experiments, writes that he wishes to point out a variety of the principles embodied in the machine the author had described, which, he says, is so obvious that it cannot fail to be hit upon by some inventor before long, and warns anyone whom it may strike against patenting the idea, seeing that he had already constructed a machine upon the plan. Mr. Murray then states that, "Whereas Mr. Wilde, "beginning with an ordinary magneto-electric machine, "uses the current obtained from it to charge a powerful

* Proceedings of the Royal Society, April 26, 1866. Philosophical Transactions, Vol. clvii., 1867. Philosophical Magazine, S. 4, Vol. xxxiv.

"electro-magnet, and from this obtains a second and more "powerful current, which, used in like manner, produces "one still more intense. I, using only a single machine, "pass the currents from its armatures through wires coiled "round the permanent magnets in such direction as to "intensify their magnetism, which, in its turn, reacts upon "the armatures and intensifies the current."

Mr. Murray's warning to inventors against patenting his idea would seem to have been disregarded, as a patent was taken out on December the 24th of the same year, by C. & S. A. Varley, for "Improvements in the means of generating Electricity," wherein is described a machine consisting of two electro-magnets and two bobbins. The bobbins are mounted on an axle, on which also a commutator is fixed; the ends of the insulated wire surrounding the bobbins are connected with this commutator and through it with the insulated wire of the electro-magnets, forming the whole into one electric circuit. Before using the apparatus an electric current is sent through the electro-magnet for the purpose of securing a small amount of permanent magnetism in the iron core of the electro-magnet. On revolving the axle, the bobbins become slightly magnetised in their passage between the poles of the electro-permanent magnets, generating weak currents in the insulated wire surrounding them. The effect of the current passing through the electro-magnets is to increase their magnetism, and to magnetise in a higher degree the bobbins when passing between the poles of the electro-magnets, and the bobbins act and react on each other causing the circulation of increased quantities of electricity.

Another patent for the same idea was taken out by C. W. Siemens, F.R.S., on January the 31st, 1867, as a communication from Dr. Werner Siemens, of Berlin. Again the same idea was communicated to the author in a letter from Mr. Moses G. Farmer, of Salem, Mass., U.S.A., who had constructed a machine to which the initial charge of magnetism was imparted by means of a thermo-electric battery.

The last instance of the repetition of this same idea is that by Sir Charles Wheatstone, in a paper "On the Augmentation of the Power of a Magnet by the reaction thereon of currents induced by the magnet itself."*

This enumeration of the instances where the idea of augmenting the force of a magnet by currents induced by itself, the author would have deemed somewhat unnecessary, were it not that the contrivance had been described as a new principle in electric science, whereas it is, as Mr. Murray justly designates it, an obvious variety of the principles embodied in the machine the author first described before the Royal Society.

At the time when this method of exciting an electro-magnet was brought prominently forward by Messrs. Siemens and Wheatstone, the author directed attention to the fact (which would seem to have escaped the notice of these electricians, as they omitted to mention it) that machines constructed as they had described them, are incapable, of themselves, of producing powerful electric currents, as the whole energy of the machine is expended in exciting its own electro-magnet.†

While the current transmitted from the armature of a magneto-electric or an electro-magnetic machine through coils surrounding its own magnet is incapable of directly producing powerful electro-dynamic effects, such current may be usefully employed to excite the electro-magnets of other machines in accordance with the author's original method. Some idea of the smallness of the quantity of electricty requisite for this purpose will be found from the fact that the full power of the 10 inch machine is de-

* Proceedings of the Royal Society, vol. xv., p. 369.

† Proceedings of the Literary and Philosophical Society of Manchester, vol. vi., p. 103.

veloped when its electro-magnet is excited by the current from four pint Grove's cells. The electro-magnet of this machine is now excited by its own residual magnetism in the following manner:—A small magnet cylinder (3·5 inches diameter and 14 inches long) is bolted to the top of the 10 inch cylinder, so that the sides and axis of the former are parallel with the similar parts of the latter. The cylinders are separated for a space of three-quarters of an inch by packings of brass, and consequently act upon each other by induction through the intervening space, instead of by contact as in ordinary methods of magnetisation.

The residual or permanent magnetism of the large electro-magnet with its cylinder is very considerable, being many times greater than that of the four small permanent magnets with which it was originally excited.

The small scale upon which the author's experiments have been repeated by physicists has, in some instances, given rise to the notion that the residual magnetism of an electro-magnet is a lower degree of permanent magnetism than that which originally formed the basis of his augmentations.

The coils of the small armature are placed in connection with those of the great electro-magnet, and when the armature is rotated the magnet cylinders act and react on each other until the electro-magnet is excited to the highest degree of intensity. By this arrangement of the armatures and cylinders the minor current for exciting the electro-magnet is kept distinct from the major current from the large armature, which may be coiled for currents of high or low tension, according to the purpose for which they are required.

So far as the author has communicated the results of his investigations on the principle of accumulative action in electro-dynamics, they have been obtained with machines designed with reference to the peculiar form of armature contrived by Dr. Werner Siemens, of Berlin. While possessing several advantages, in point of efficiency over that of Saxton, the Siemens armature requires to be driven at a high velocity to produce a succession of currents sufficiently rapid to be available as a substitute for the voltaic battery. Little inconvenience however arises from the high speed when the armatures are of small dimensions, but as the dimensions increase it becomes necessary to lower the speed, and the large machines are, consequently, not proportionately powerful with the smaller ones. Besides this, the advantages possessed by this form of armature in having the moving mass of metal near the axis of rotation is neutralised, as the dimensions increase, by the excessive heat generated by the magnetisation and demagnetisation of the iron; it would also be convenient in some circumstances to drive a machine direct from the crank or fly-wheel of a steam-engine, without the intervention of multiplying gearing.

Considerations of this nature led the author, towards the end of 1866, to propose to himself the construction of an electro-magnetic machine with multiple armatures, which should remove the inconveniences inherent in those hitherto constructed, by producing a greater number of currents for one revolution of the armature axis. Since that time he has been engaged, with more or less interruption, in carrying out this design, and has at length constructed a machine the performance of which surpasses all his previous essays in this direction, in regard to power and efficiency, and with a considerable reduction in the quantity of the materials employed.

The machine in which these results are embodied consists of a circular framing of cast iron, firmly fixed together by an iron bridge and stay rods. A heavy disk of cast iron is mounted on a driving shaft, running in bearings fitted to each side of the framing. One of these bearings is carefully insulated from the framing by suitably formed pieces of ebonite, and also from the shaft, by a cylinder of the same substance. Through the side of the disk, and parallel with its axis, sixteen holes are bored, at equal angular distances from each other, for the reception of the same number of cores or armatures. The cores project about two inches through each side of the disk, and are held firmly in their places by screws tapped through its periphery. Around each inside face of the circular framing, and concentric with the driving shaft, sixteen cylindrical electro-magnets are fixed, at the same angular distance from each other and from the centre of the shaft as the iron cores round the disk; the two circles of magnets, consequently, have their poles opposite each other, with the disk and its circle of iron cores revolving between them. The ends of the cores are terminated with iron plates of a circular form, which answer the double purpose of retaining the helices surrounding the cores in their places, and overlapping for a short distance the spaces between the poles of the electro-magnets.

The cylindrical bar magnets are each coiled with 659 feet of copper wire, 0·075 of an inch in diameter, insulated with cotton. The helices are grouped together to form a fourfold circuit, 2,636 feet in length, and are joined up in such a manner that adjacent magnets in each circle, as well as those directly opposite in both circles, have north and south polarity in relation to each other. A charge of permanent magnetism was imparted to the system of electro-magnets by the current from a separate electro-magnetic machine. The armatures, although formed of sixteen pieces of iron, are, by projecting through both sides of the disk, thirty-two in number. The length of insulated wire on each armature is 116 feet, and the thickness is the same as that on the electro-magnets. These helices are divided into eight groups of four each, and coupled up for an intensity of 4×116 feet.

One of the groups is used for producing the minor current for exciting the circles of electro-magnets, while the remaining groups are joined together for a quantity of seven and an intensity of four for the production of the major current of the machine. The aggregate weight of wire on the electro-magnets is 356 lbs., and on the armatures 26 lbs. The helices for exciting the electro-magnets are connected with a commutator, while those producing the major current are placed in connection with two rings, or in place thereof with another commutator, according as the alternating or the direct current from the machine is required. The strength and proportions of the several parts of the machine enable it to be driven with advantage from 300 to 1,000 revolutions per minute.

At the medium velocity of 500 revolutions per minute, the major current will melt eight feet of iron wire 0·065 of an inch in diameter (No. 16 B.W.G.), and will produce two electric lights in series, each consuming carbons half an inch square at the rate of three inches per hour.

When driven at a velocity of 1,000 revolutions (equivalent to 16,000 waves) per minute, the current will fuse 12 feet of iron wire 0·075 of an inch in diameter, (No. 15 B.W.G.)

At this velocity the light from two sets of carbons in series is unendurably intense as well as painful to those exposed to its immediate influence. Estimated on the basis afforded by the performance of the excellent magneto-electric light machines of MM. Auguste Berlioz and Van Malderen, who have made a careful study of the photometric intensity of the electric and oil lights; the power of the new machine is equal to that of 1,200 Carcel lamps, each burning 40 grammes (1·408oz. avoir.) of oil per hour, or of 9,600 wax candles. The amount of mechanical energy expended in producing this light is about 10 indicated horse power.

A comparison between the power of the new machine and that of the 10 inch machine will show that while the current from the former fuses 12 feet of iron wire 0·075 of an inch in diameter, the current from the latter fuses only 7 feet of wire 0·065 of an inch in diameter; and is, consequently, only about half as powerful as that from the new machine. Besides this, the quantity of copper used in the construction of the new machine is about 3½cwt., and of iron 15cwt.; while the weight of these metals in the 10 inch machine is 29cwt. and 60cwt. respectively. In other words, we have in the new machine a double amount of power, with less than one-fourth the amount of materials employed in the construction of the 10 inch machine. Another advantage possessed by the new machine is the great reduction of temperature in the armatures by their rapid motion through the air, which acts much more efficiently than the circulation of water through the magnet cylinder. By increasing the diameter of the electro-magnetic circles, conjointly with the number of electro-magnets and armatures, the angular velocity of the machine may be so diminished that it may be driven directly from the crank of a steam engine, concurrently with an increase of electric power proportionate to the number of electro-magnets and armatures in the electro-magnetic circles.

In his paper "On a Property of the Magneto-electric Current to Control and Render Synchronous the Rotations of the Armatures of a number of Electro-magnetic Induction Machines,"* the author stated that this property would be available when the machines were used for the electro-deposition of metals from their solutions. It has, however, been found that the small resistance presented by depositing solutions to the passage of the currents, prevents this property from manifesting itself (in accordance with what the author

* Proceedings of the Literary and Philosophical Society of Manchester, December 15th, 1868.

stated in his paper respecting the effect of joining the poles with a good conductor), and it is only when the machines are employed for the production of electric light, or other purpose, where the external resistance is considerable that this electro-mechanical function of the current comes into useful operation.

The author, before concluding his description of this further development of the principle of electro-magnetic accumulation, considers it a duty he owes to himself as well as to science, that he should not allow to pass unnoticed the views and statements of certain writers respecting the place and value of his investigations in the history of natural knowledge. The peculiar good fortune which enabled him to follow up the discovery of a great principle to such brilliant results has contributed, accidentally in some instances, to establish the idea, that these results are an expansion of Faraday's discovery of magneto-electricity rather than a distinct step in electricial science. A brief glance at the history and progress of electricity and magnetism will suffice to show the erroneousness of this view, and also that his discovery bears only the same kind of relation to that of Faraday as that philosopher's discovery does to those of Galvani, Volta, and Grove in galvanic electricity; and of Oersted, Ampère, Arago, and Sturgeon in electro-magnetism. That the discovery of the indefinite increase of the magnetic and electric forces from quantities indefinitely small is a fundamental advance in electrical knowledge, and not simply an expansion of known principles or an improvement in a machine, as it has been made to appear by some, is evident from the fact that the principle since its enunciation in 1866, together with the author's invention of minor and major magneto-electric circuits, has been embodied in the machines of different forms constructed by Ladd, Holmes, d'Ivernois, Gramme, and others. Moreover, Faraday himself, while on the threshold of his discovery, distinctly negatived its possibility. Reasoning on the magnet as a source of electricity in a paper "On the Physical Character of the Lines of Magnetic Force" (Philosophical Magazine, s. 4, vol. III., p. 415), he says, "Its analogy with the helix is wonderful, nevertheless there is as yet a striking experimental distinction between them; for whereas an unchangeable magnet can never raise up a piece of soft iron to a state more than equal to its own, as measured by the moving wire, a helix carrying a current can develop in an iron core magnetic lines of force of a hundred or more times as much power as that possessed by itself when measured by the same means. In every point of view, therefore, the magnet deserves the utmost exertions of the philosopher for the development of its nature, both as a magnet and also as a source of electricity, that we may become acquainted with the great law under which the apparent anomaly may disappear, and by which all these various phenomena presented to us shall become *one*." Now, it was the precise and absolute manner in which Faraday stated the definiteness of the relation between the magnetism of a permanent magnet and that of a piece of iron magnetised by its influence, that led the author to enunciate in terms equally absolute and precise the antithesis of Faraday's proposition. How far Faraday's hopes and preconceptions of the electro-magnet as a source of electricity have been realized, the results described in this and the author's former papers will show. Already has it superseded the use of the voltaic battery in every electro-depositing establishment of note in this country, and it is making rapid progress abroad.

That the transformation of mechanical energy into other modes of force on so large a scale, and by means so simple, will find new and much more important applications than that above mentioned is one of the author's most firm convictions.

In a note to his paper the author reviews the attempt by

M. Gramme to arrive at a nearer approximation to the continuous current of the voltaic battery than that produced from a magneto-electric machine when rectified by means of a commutator of the ordinary construction. This refinement, the author states, possesses little or no advantage in any of the applications of magneto-electricity, when the rectified waves succeed each other at the rate of 5,000 per minute, and upwards—a rate of succession easily attainable, and far exceeded by the machines of Berlioz and Holmes. At this rate the discontinuity of the waves is not distinguishable in the electric light; nor in the magnetisation of electro-magnets; nor on galvanometer needles; nor in electrolytic processes; and it can only be perceived by the vibrations of a steel spring, placed before the poles of a small electro-magnet, round which the current is transmitted. Such instrument would, the author thinks, also indicate similar points of maxima and minima in the current from Gramme's machine. As the armature helices in this machine are each connected with separate pieces of metal, forming the segments of a circle, from which the current is taken by means of ordinary metallic brushes, the number of helices producing currents available for external use, at any given moment, is only a fraction of those constituting the whole circle, and, consequently, for a given weight of materials such a magneto-electric machine must be greatly inferior in power to machines in which the current is delivered from the whole of the helices simultaneously, as in those hitherto constructed. The substitution by M. Gramme of a commutator with multiple segments insulated from each other, and having adjacent segments of the same polarity, while those diametrically opposite have a polarity different, requires the same precautions to be taken to prevent the spark at the change of contacts, and is subject to the same wear from friction, as commutators of the ordinary form, in which the segments are united with a common metallic base. Moreover, long experience has proved that for the production of electric light the alternating current is greatly superior to the continuous one, as commutators are dispensed with, and it has the important advantage of consuming the carbons equally, and thereby always retains the luminous point in the focus of any optical apparatus used in connection with it.

In short, M. Gramme, in his endeavour to reconcile the incompatible relations of the voltaic current and the magneto-electric wave at the instant of its generation, has, by inverting the order and functions of the organic parts of an ordinary magneto-electric machine and suppressing the action of a number of the armature helices, brought about results retrogressive from those previously attained by Nollet, Berlioz, and Holmes, and it is only by the adoption of the principle of electro-dynamic accumulation (*i.e.*, the exciting of a major electro-magnetic induction machine by a minor one, fixed on the same base), in accordance with the principles laid down by the author in his former papers, that the results obtained by M. Gramme exceed those from ordinary magneto-electric machines.

PHYSICAL AND MATHEMATICAL SECTION.

April 22nd, 1873.

ALFRED BROTHERS, F.R.A.S., President of the Section, in the Chair.

Results of Rain Gauge Observations made at Eccles, near Manchester, during the year 1872, by THOMAS MACKERETH, F.R.A.S., F.M.S.

The characteristic of the rainfall of the past year is its immense excess of the average fall. From the table given below this excess will be seen to be more than 13 inches, or about 36·7 per cent. over the average fall of the year. There were only two months of the year, August and December, that had a fall less than the average of twelve years, but this minimum was exceedingly small. The greatest excess above the average happened in the summer quarter, July to September, and the fall in July was 142 per cent. above the average for that month. June, July, and September were the wettest months of the year.

The number of days on which rain fell during the past year was very large. There were only 101 days throughout the year on which rain did not fall. There was 27 per cent. over the average of twelve years of days on which rain fell during the year. But the number of wet days exceeded the average most in the first six months of the year. The number in excess in the first three months being as much as 34 per cent.

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

Quarterly Periods.		1872.	Fall in Inches.	Average of 12 years.	Differences.	Quarterly Periods.	
Average of 12 years.	1872.					Average of 12 years.	1872.
Days.	Days.					Inches.	Inches.
		January	4·096	2·693	+1·403		
52	70	February	2·849	2·391	+0·458	7·516	9·739
		March	2·794	2·432	+0·362		
		April	3·003	2·193	+0·810		
46	61	May	2·548	2·088	+0·460	7·014	10·946
		June	5·395	2·733	+2·662		
		July	7·327	3·022	+4·305		
51	60	August	2·988	3·001	—0·013	10·254	16·849
		September	6·534	4·231	+2·303		
		October	4·404	4·245	+0·159		
58	73	November	3·427	3·200	+0·227	10·618	10·882
		December	3·051	3·173	—0·122		
207	264		48·416	35·402	—13·014		

In the next table I give the results obtained from rain gauges of two different kinds, placed in close proximity in the same plane, and 3 feet from the ground. The one has a 10 inch round receiver, and the other a 5 inch square receiver. The large receiver had an excess over the small one in every month excepting April, June, July, and December; but in June the rain-fall in both cases was the same. The total difference of the fall in the two gauges was not great, being less than half an inch on 48½ inches of rain-fall. In comparing, however, the fall in the two gauges for an average of five years, a larger difference arises, being more than 6-10ths of an inch on an average fall of 36 inches, and an excess of the large gauge occurred in every month excepting March.

1872.	Rainfall in inches in 10 in. round Receiver 3 ft. from ground.	Rainfall in inches in 5 in. square Receiver 3 ft. from ground.	Differences.	From 1868 to 1872. Average of 5 years rainfall in inches, in 10 in. round receiver 3 ft. from ground.	Average of 5 years rainfall in inches, in 5 in. square receiver 3 ft. from ground.	Differences.
	1872.	1872.				
January..	4·096	3·996	+·100	2·823	2·805	+·018
February.	2·849	2·714	+·135	2·590	2·542	+·048
March ...	2·794	2·735	+·059	2·233	2·284	—·051
April ...	3·003	3·048	—·045	2·490	2·467	+·023
May	2·548	2·484	+·064	1·876	1·846	+·030
June	5·395	5·395	,,	2·535	2·493	+·042
July	7·327	7·409	—·082	2 618	2·596	+·022
August...	2·988	2·971	+·017	2·598	2·522	+·076
Septembr.	6·534	6·363	+·171	4·255	4·204	+·051
October...	4·404	4·347	+·057	5·232	5·191	+·041
Novembr.	3·427	3·422	+·005	2·941	2·580	+·361
December	3·051	3·059	—·008	3·816	3·806	+·010
	48·416	47·943	+·473	36·007	35·336	+·671

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 6 inch square receiver, and the one is placed 3 feet, and the other 34 feet above the ground. The total fall in the one 3 feet from the ground was 47·943 inches, and in the one 34 feet from the ground it was 41·002 inches for the last year. The difference between the fall in the two gauges is 6·941 inches, or about 14½ per cent. less rain fell last year in the higher than in the lower gauge. In the same table I give the average fall for five years in each gauge, and by comparing the results I find that for such an average fall about 16 per cent. less rain falls in the upper than in the lower gauge.

1872.	Fall of rain in inches in 5 inch square receiver 3 feet from the ground. 1872.	Fall of rain in inches in 5 inch square receiver 34 feet from the ground. 1872.	From 1868 to 1872.	
			Average fall of rain in inches for 5 years, in 5 inch square receiver 3 feet from ground.	Average fall of rain in inches for 5 years, in 5 inch square receiver 34 feet from ground.
January	3·996	3·019	2·805	1·997
February.........	2·714	2·212	2·542	1·917
March	2·735	2·166	2·284	1·787
April	3·048	2·590	2·467	2·116
May...............	2·484	2·181	1·846	1·665
June	5·395	4·762	2·493	2·220
July...............	7·409	6·947	2·596	2·325
August	2·971	2·607	2·522	2·178
September	6·363	5·714	4·204	3·608
October	4·347	3·638	5·191	4·312
November	3·422	2·455	2·580	2·260
December	3·059	2·711	3·806	3·207
	47·943	41·002	35·336	29·592

In the next table I give the fall of rain 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. The amount of rain that fell during the day exceeded the fall during the night in six months of the year, but in the remaining months, namely, January, August; September, November, and December, the fall during the night exceeded the day fall. The total difference between the night and day fall is much less than during 1871. In that year the excess of the day over the night fall was 4·136 inches, whilst during the past year it was only 1·891 inches.

	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	1·860	2·136	+0·276
February	1·413	1·301	—0·112
March	2·061	0·674	—1·387
April	1·737	1·311	—0·426
May	1·297	1·187	—0·110
June	3·309	2·086	—1·223
July	4·398	3·011	—1·387
August	1·444	1·527	+0·083
September	2·092	4·271	+2·179
October	2·366	1·981	—0·385
November	1·470	1·952	+0·482
December	1·470	1·589	+0·119
	24·917	23·026	—1·891

In the next table I present the average day and night fall for five years. This table continues to show, as previous ones which I have presented have done, that the night fall is, as a rule, in excess after the heavy falls of rain set in in August to the end of the year, and during the first months of the year. The only exception which the present table presents to this rule is the month of October. It is remarkable, however, how near the total results of the two periods are to each other, the difference being really only two per cent. of the day over the night fall.

Average of Five Years from 1868 to 1872.

	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	1·363	1·444	+0·081
February	1·053	1·489	+0·436
March	1·335	0·948	—0·387
April	1·434	1·032	—0·402
May	1·214	0·632	—0·582
June	1·298	1·195	—0·103
July	1·542	1·053	—0·489
August	1·135	1·386	+0·251
September	1·884	2·319	+0·435
October	2·676	2·514	—0·162
November	1·419	1·550	+0·140
December	1·688	2·118	+0·430
	18·032	17·680	—0·352

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 24th, 1873.

Professor W. C. Williamson, F.R.S., President of the Section, in the Chair.

The President exhibited specimens of Calamostachys Binneyana and Selaginella Wallichii.

April 21st, 1873.

Professor W. C. Williamson, F.R.S., President of the Section, in the Chair.

Mr. Thomas Rogers was elected an Associate, and Mr. James C. Melvill, M.A., F.L.S., a member of the Section.

Mr. Hardy exhibited specimens of Veronica Buxbaumii (Ten) gathered on the 14th of April, by the side of a new road leading from Barlow Moor Lane to the river bank; growing apparently wild. Buxton in his "Botanical Guide," mentions its occurrence in a lane at Sale in 1847; and Mr. Bailey stated that the late Dr. Windsor had met with it as a garden weed at Whalley Range. Mr. H. C. Watson's remark in his Compendium of the Cybele Britannica (referring to the British Islands generally) that this plant is "an alien fast becoming a denizen," would therefore appear to be strictly applicable to the Flora of the Manchester District.

Mr. John Barrow read a paper "On the Use of Naphthaline in Section Cutting."

I wish to bring before the notice of the members and those microscopists who are interested in cutting sections of soft or delicate tissues the use of Naphthaline as a support for such tissues in the section cutter.

The advantages obtained by the use of Napthaline over wax and other bodies recommended for this purpose are, a low fusing point, absence of contraction in the cutter, very little injury to the edge of the knife, and very ready solubility after cutting in Benzol or spirit, so that the substance is removed at once from the section without injury.

Napthaline is a body not very generally known outside the works of the tar distiller or colour maker, so that possibly some of the members may not be able to obtain samples readily, but I shall have pleasure in supplying it to any of our own members.

Professor Williamson recommended an admixture of wax and oil with the Napthaline, and stated that the knife cuts better with this addition; he also exhibited some extremely beautiful longitudinal and cross sections made in this way.

"Note on a Fossil Spider in Ironstone of the Coal Measures," by Mr. John Plant, F.G.S.

More than forty years ago Mr. William Anstice found a fossil insect in a nodule of ironstone from the coal formation of Coalbrook Dale. It was figured in Dr. Buckland's Bridgewater Treatise, plate 46, and described by Mr. Samouelle the entomologist as a beetle allied to a type of tropical Curculios, and provisionally named as *Curculioides Prestvicii*. Since that time many insects have been discovered in the coal measures both in England and America, and wings of Neuropterous insects have been found as low down in palæozoic rocks as the Devonian — below which no true insects have been yet observed. The specimen figured by Dr. Buckland remained unique for a long time—until 1871, when another was discovered by Mr. Elliott Hollier of Dudley, so well known for his cabinet of rare Silurian trilobites, in an ironstone nodule from the Dudley coal field. This discovery has thrown considerable light upon the real character of the one first mentioned, which turns out not to be a beetle but a spider allied to an existing genus of tropical spiders of the family of Tarentulæ. The nodule in which this specimen is embedded has split cleanly down the axis of the insect, and both the under and upper surfaces have been preserved in a singularly beautiful manner, whereas in Dr. Buckland's figure the insect is less perfect and displays rather confusedly a portion of each surface.

Mr. H. Woodward has described and figured Mr. Hollier's specimen in the Geo. Mag. September, 1871, under the name of *Eophrynus Prestvicii*, from its analogy to the spiders of the genus Phrynus.

The appearance of each surface of this fossil is so remarkably unlike that they might be readily mistaken for separate species. This is a character which may be seen in living species of Phrynus. The upper surface in the fossil is smooth and ringed, and the under surface granulated. In Phrynus the body is flat, divided into rings, the thorax broad and crescent-shaped, the skin is horny and hard, as in the scorpions. Spiders are generally soft and without rings. The palpi terminate in prehensile claws, the tibia of the forelegs are of enormous length, with the tarsi of extreme fineness, admirably adapted for delicate organs of feeling. The Tarentulæ comprise Arachnids of high organization — approaching the scorpions — which have been found fossil in coal measures; and this discovery of a spider opens to our contemplation another link of a prolific life existing in the vast forests of tropical coal plants.

Annual Meeting, May 5th, 1873.

Mr. Joseph Sidebotham, F.R.A.S., in the Chair.

The following report of the Council for the year ending 5th May, 1873, was read and passed: —

Papers on the following subjects have been read during the past session :

October 7th, 1872.—"On the Destruction of British Ferns," by Joseph Sidebotham, F.R.A.S.

"On Malpighiaceous Hairs," by Charles Bailey.

November 4th, 1872.—"The Flora of Alexandria," by H. A. Hurst.

"On the Anatomy of Musca domestica," by T. S. Peace.

January 27th, 1873.—"Notes on the Minerals of Venezuela," by John Plant, F.G.S.

February 14th, 1873.—"On the occurrence of Unio Tumidus in the Manchester district," by John Hardy.

"Remarks on an old Microscope," by Joseph Sidebotham, F.R.A.S.

March 24th, 1873.—"On Hœmopis sanguisorba," by T. S. Peace.

"Notes on Calamostachys Binneyana and Selaginello Wallichii," by Professor W. C. Williamson, F.R.S.

April 21st, 1873.—"The use of Naphthaline in Section cutting," by John Barrow.

"Note on a Fossil Spider in ironstone of the coal measures," by John Plant, F.G.S.

The most valuable subject in connection with the communications brought under the notice of the section was an exhibition on December 11th, 1872, of a very large collection of Natural History and other objects, brought by Mr. James M. Spence from Venezuela, which remained open to the public for some days, and was visited by a large number of persons. As Mr. Spence has just returned to this country we may hope for further communications respecting its resources and natural history products.

The Section has to deplore the recent death of Mr. George Edward Hunt, so well known as a muscologist, and whose papers were some of the most valuable contributed by the members.

The ordinary members of the Section now number 37, the associates 12.

From the accompanying statement of accounts it will be seen that the financial position of the Section is satisfactory, the treasurer having a balance in hand of £37 13s.

The Microscopical and Natural History Section of the Literary and Philosophical Society, in Account with H. A. Hurst, Treasurer.

1872.	£	s.	d.	1872.	£	s.	d.
To half cost of Linnean Transactions and Gardeners' Chronicle	14	13	5	By Balance	37	6	10
„ Parent Society, for use of Rooms	2	2	0	„ Subscriptions	25	10	0
„ W. Roscoe, for Teas	5	14	4	„ Interest from Bank	0	18	11
„ Chas. Simms and Co., Printing Circulars	2	17	0				
„ J. E. Cornish, Microscopical Journal	0	16	0				
„ Balance	37	13	0				
	£63	15	9		£63	15	9
				1873.			
				May. By Balance	£37	13	0

Examined and found correct,

(Signed) J. Barrow, Walter Morris, } Auditors.

The election of officers for the Session 1873-4 was then proceeded with, and the following gentlemen were appointed:

President.

W. C. WILLIAMSON, F.R.S.

Vice-Presidents.

J. SIDEBOTHAM, F.R.A.S.
JOSEPH BAXENDELL, F.R.A.S.
SPENCER H. BICKHAM, Jun.

Treasurer.

HENRY ALEXANDER HURST.

Secretaries.

CHARLES BAILEY.
WALTER MORRIS.

Of the Council.

HENRY SIMPSON, M.D.
JOHN BARROW.
THOMAS COWARD.
ROBERT B. SMART.
ALFRED BROTHERS, F.R.A.S.
T. H. NEVILL.
J. C. MELVILL, M.A., F.L.S.

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Barrow, John.
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Binney, Edward Wm., F.R.S., F.G.S.
Brockbank, W., F.G.S.
Brogden, Henry.
Brothers, Alfred, F.R.A.S.
Cottam, Samuel.
Coward, Edward.
Coward, Thomas.
Dale, John, F.C.S.
Dancer, John Benj., F.R.A.S.
Darbishire, R. D., B.A.
Dawkins, W. Boyd, F.R.S.
Deane, William K.
Gladstone, Murray, F.R.A.S.
Heys, William Henry.
Higgin, James, F.C.S.
Hurst, Henry Alexander.
Latham, Arthur George.
Maclure, John Wm., F.R.G.S.
Melvill, J. C., M.A., F.L.S.
Morgan, Edward, M.D.
Morris, Walter.
Nevill, Thomas Henry.
Piers, Sir Eustace.
Rideout, William J.
Roberts, William, M.D.
Sidebotham, Joseph, F.R.A.S.
Simpson, Henry, M.D.
Smart, Robert Bath, M.R.C.S.
Smith, Robert Angus, Ph.D., F.R.S., F.C.S.
Vernon, George Venables, F.R.A.S.
Williamson, Wm. Crawford, F.R.S., Prof. Nat. Hist., Owens College.
Wright, William Cort.

List of Associates.

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Hardy, John.
Hunt, John.
Labrey, B. B.
Linton, James.
Meyer, Adolph.
Peace, Thos. S.
Plant, John, F.G.S.
Rogers, Thomas.
Ruspini, F. O.
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Waterhouse, J. Crewdson.