PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

VOL. XI.

SESSION 1871—72.

MANCHESTER:

PRINTED BY THOS. SOWLER AND SONS, RED LION STREET, ST. ANN'S SQUARE.

LONDON: H. BAILLIÈRE, 219, REGENT STREET.

1872.

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 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.

INDEX.

ERRATUM.

Page 99, line 9 from top, for "Regnalt" read "Renault."

PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 3rd, 1871.

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

Mr. Thomas Harrison and Mr. Thomas Livesey were elected Ordinary Members of the Society.

Dr. Joule, F.R.S., exhibited curves showing the diurnal variation of the magnetic inclination in Manchester during the months May, June, and July. These observations, along with those of horizontal force, showed that the total force was nearly a constant quantity.

Professor O. Reynolds, M.A., exhibited a series of models which he had designed to illustrate problems in the geometry of planes and solids.

The President said that public attention had been justly called to the high rate of mortality in the city of Manchester and its adjoining borough Salford. One of the leading newspapers had lately stated that the gigantic infant mortality of our great towns is notorious. In some parts of Liverpool for example 58 per cent of the children under one year of age die, while in other districts of the same town only 5 per cent die.

The subject of infantile mortality engaged public attention nearly a century ago, for I find from the late Mr. George Walker's Journal, kindly presented to the Society by Mr. B. H. Green, that

"Dr. Percival took from the Register at Manchester and Salford for six years, from 1768 to 1774, and found there had died under two years (compared with the whole) as 1 to 2·9, or nearly 1 to 3. Died under 2 years of baptised children (as above) as 1 to 3·6, say 1 to 3½. From January 1, 1780, to January 1, 1791, 12 years, Buried 17,597, of which number have died under 2 years, 5,529; from 2 to 5, 1,823, all of whom were baptised." In addition, the still-born and those who died before baptism have to be added. Mr. Walker also states that

"The probability of the duration of life from observations on the Bills of Mortality of London, on an average of ten years, by Thomas Simpson, Mathematician, 1790, Infants just born, 1,000; living at the end of one year, 680; at the age of 2 years, 547; at the age of 3 years, 496. Therefore more than one half the children died under 3 years."

From these extracts it appears that the rate of mortality amongst infants is not confined to a manufacturing population, for it was high in Manchester before the Cotton Manufacture had made much progress, and higher still in former times in London, where no such employment of females prevailed, to take the mothers from their children.

Dr. Percival, F.R.S., a former President of this Society, and Mr. Simpson, the eminent mathematician, are both first-rate authorities on the subject, and their results fully accord with those of our Secretary, Mr. Baxendell, as given to the Society and printed in the Proceedings for April 19th, 1870. The mortality of our city no doubt is bad enough, but it does not arise altogether from infantile mortality as has been asserted, but from adult mortality as well.

Ordinary Meeting, October 17th, 1871.

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

"On the Oxychlorides of Antimony," by Mr. William Carleton Williams, Student in the Laboratory of Owens College, communicated by Professor H. E. Roscoe, F.R.S.

Phosphorus Oxychloride $PO\,Cl_3$ having been prepared by heating together one molecule of phosphorus pentoxide with three of pentachloride, it appeared not unlikely that a similar reaction might occur with antimony giving rise to the missing oxychloride corresponding to the phosphorus compound above mentioned.

The following investigation was undertaken at Dr. Roscoe's request with the view of elucidating the above reaction as no oxychlorides derived from the pentachloride have as yet been described.

A mixture of one molecule of antimony pentoxide prepared by heating the pentachloride with water with three molecules of the pentachloride was heated for some hours in sealed tubes to 140° C. On opening the tube after cooling it was found to contain, besides unchanged pentachloride and pentoxide, two distinct solid crystalline compounds. When the pentoxide prepared by the action of nitric acid on the metal is heated with the pentachloride in a similar way no oxychloride is formed.

One of these fuses at 85° C. to a clear yellowish liquid, whilst the other, produced only in small quantities, is found adhering to the top of the tube in minute yellowish crystals, which fuse at a higher temperature. In order to obtain the first of these substances in a pure state it is sufficient to place the tube upright in a vessel of water at 90° with the empty end downwards; the fusible oxychloride then melts and collects as a perfectly clear yellowish liquid. After cooling, the tube is opened and the small quantity of residual pentachloride having been poured off, the solid mass is dried on a porous plate over solid caustic potash in vacuo. The oxychloride thus obtained is a perfectly white crystalline substance, exceedingly hygroscopic, so that when exposed to the air for a few minutes it becomes a pasty mass which rapidly changes to a liquid. It readily dissolves in an aqueous solution of tartaric acid, whilst it is decomposed by water and is perfectly insoluble in carbon disulphide. The melting point of the substance is 85° C. as a mean of well agreeing determination made with four different preparations. When heated in a retort until it boils, chlorine gas is evolved, whilst pentachloride and trichloride of antimony distil over, a residue of antimony pentoxide remaining in the retort.

A modification of Rose's well known method of precipitation first as insoluble antimoniate of soda, and then as antimony sulphide was employed for the determination of the antimony; the precipitated sulphide was (1) oxidised to Sb_2O_4 either by treatment with pure fuming nitric acid or by heating with from 10 to 20 times its weight of pure mercuric oxide, and (2) the sulphide was completely reduced to metallic antimony by heating gently in a current of hydrogen until sulphuretted hydrogen ceases to be evolved. In the estimation of chlorine it was found that when silver nitrate is added to a solution of an antimony oxychloride acidified by nitric acid, a small trace of antimony is invariably carried down with the silver chloride. In order to free the precipitate from antimony, the silver chloride is first heated gently in a current of hydrogen when the silver is reduced, and, on stronger ignition the whole of the antimony is volatilized as the hydrogen compound. Thus 1·277 grams of an alloy containing 2·5 parts of antimony to 97·5 parts of silver was found to lose on heating in hydrogen, 0·0321 grm. corresponding to 97·48 % of silver.

The accuracy of each of the above methods was tested by determining the percentage of antimony and chlorine in pure antimony trichloride, the results agreeing closely with each other and with the theoretical composition. The objection to Schaeffer's method of decomposing the oxychloride by boiling with a solution of sodium carbonate is that the precipitated oxide of antimony being in a very finely divided state a portion of it is very apt to pass through the filter on washing.

The simplest formula which agrees with the analytical results is $Sb_3Cl_{13}O$ or three molecules of pentachloride in which two of chlorine are replaced by one of oxygen.

	Calculated.		Found.
Sb_3	43·39	......	43·46
Cl_{13}	54·71	......	54·75
O	1·90	......	—
	100·00		

That this is a definite compound and not a mere mixture of pentoxide and pentachloride ($Sb_2O_5 + 14\,SbCl_5$) is evident from the fact that the latter substance is not dissolved out by washing with carbon disulphide. The calculated percentage of pentoxide contained in this compound is 7·68; on heating 2·517 grams of the oxychloride in a tube retort a residue of 0·1799 grams of pentoxide remained, corresponding to a percentage of 7·14.

The second oxychloride formed by heating the mixture of one molecule of pentoxide and three of pentachloride is produced only in small quantities as yellowish crystals. To obtain it in the pure state, that portion of the tube in which the substance is found is cut off and after the tube has been re-sealed it is placed in a slanting direction in a vessel containing water heated from 85° to 90°. The $Sb_3Cl_{13}O$ melts and runs down, leaving the other less fusible oxychloride behind; this is then dried on a porous plate in vacuo over solid caustic potash. Two determinations showed that the melting point of this substance is 97°·5 C.

The simplest formula agreeing with the analytical numbers is $Sb_3O_4Cl_7$, or three molecules of antimony pentachloride in which four atoms of oxygen replace eight of chlorine.

	Calculated.		Found.
Sb_3 …………	53·94	……	53·89
Cl_7 …………	36·62	……	36·58
O_4 …………	9·44	……	——
	100·00		

From the above results it is clear that the simple phosphorus oxychloride is not reproduced under similar circumstances in the antimony series, but that this element in agreement with its general deportment gives rise to more complicated compounds,

The oxychlorides derived from antimony trioxide have been frequently examined; the results of the analyses of powder of algaroth made by different investigators varies considerably, and Sabanejeff has recently shown that these differences are probably due to the presence in the substance of antimony trichloride in varying quantities. This impurity he gets rid of by washing the oxychloride obtained by the action of a large excess of water on the trichloride with ether or carbon disulphide in which the trichloride dissolves. In this way he obtains a compound having the constant composition $Sb_4Cl_2O_5$, or two molecules of trioxide in which one of oxygen is replaced by two of chlorine, whilst a simpler monoxychloride SbOCl is prepared by acting with only from 2 to 10 molecules of water on the trichloride. But this on treatment with ether or carbon disulphide loses trichloride and yields $Sb_4Cl_2O_5$; thus $5\,SbOCl = SbCl_3 + Sb_4Cl_2O_5$.

The results of my experiments lead me to the conclusion that the body obtained by the action of boiling water on the trichloride does not possess the composition $Sb_4Cl_2O_5$, but consists of 10 molecules of this substance and one of the trichloride, which latter, however, can be removed by washing with either carbon disulphide or ether. Antimony determinations in two different preparations gave

(1) 75·45 % Sb. (2) 75·88 % Sb; corresponding chlorine determinations gave (1) 12·43 % Cl; (2) 12·49 % Cl.

Hence we have:—

	Calculated for $10Sb_4Cl_2O_5+SbCl_3$		Calculated for $Sb_4Cl_2O_5$		Found
Antimony...	75·57	......	76·37	......	75·66
Chlorine ...	12·34	......	11·11	......	12·46
Oxygen ...	12·09	......	12·52	......	——

By acting upon 15 parts by weight of antimony trichloride with one part of trioxide in a sealed tube Schneider (Pogg. Ann. cviii. 407) obtains a crystalline oxychloride to which he assigns the formula $7SbCl_3SbOCl$. Repeating Schneider's experiments I obtained a pearl grey crystalline mass melting at 72° C, the melting point of the trichloride. When acted upon by absolute alcohol it yields powder of algaroth $Sb_4Cl_2O_5$, and its composition appears to be even more complicated than that assigned to it by Schneider.

Antimony determinations in two specimens gave (1) 54·24 % Sb; (2) 54·16 % Sb; whilst the corresponding chlorine estimations were (1) 45·69; (2) 45·87 instead of 55·08 % Sb and 44·02 % Cl required by Schnider's formula, but agreeing with the formula $Sb_{16}Cl_{46}O$, which requires 54·2 % of antimony and 45·357 of chlorine.

The differences here found between the substances as prepared by Schneider and myself may arise from the admixture of antimony trioxide with the oxychloride in the former preparation. When the tube in which the substance has been prepared is placed in an upright position and allowed to cool, the undissolved oxide sinks to the bottom of the tube, but on still further cooling when the contents of the tube are about to solidify the oxide rises from the bottom and mixes with the oxychloride. To obtain the substance perfectly free from undissolved oxide the contents of the tube are gently heated, and when the finely divided oxide is deposited the clear liquid oxychloride is drawn off with a pipette.

Ordinary Meeting, October 31st, 1871.

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

Mr. David Winstanley and Mr. John Ashworth were elected Ordinary Members of the Society.

Mr. Wm. Boyd Dawkins, F.R.S., gave a short account of the discoveries in the Victoria cave, made since the last account was published in the Transactions of the Society. The clay forming the bottom of the cave, and which hitherto had been barren, was now yielding broken fragments of bone, some of which had been gnawed by the cave-hyæna. A lower jaw of this animal was found, which indicated the presence of the characteristic Pleistocene mammalia in a part of Yorkshire in which they had not been known to have existed up to the present time. There were, therefore, three distinct groups of remains in the cave. The Romano-celtic on the surface, the Neolithic beneath, and lastly that which has been furnished by the clay which is glacial in character. And since two feet of talus had been accumulated above the Romano-celtic layer during the last 1,200 years, it is very probable that the accumulation of debris of precisely the same character between the Romano-celtic and Neolithic layers, six feet in thickness, was formed in about thrice the time, or 3,600 years. If this rough estimate be accepted, and it is probably true approximately the Neolithic occupation of the cave must date back to between 4,000 and 5,000 years ago. There is no clue to the relative antiquity of the group of remains found in the clay; but it may safely be stated to be far greater than that of the Neolithic stratum. Throughout Europe the break between the Pleistocene age represented in the cave by the bones in the clay and the

Prehistoric age—the Neolithic of the cave—is so great and so full of difficulty that it cannot be gauged by any method which has hitherto been invented.

Mr. BOYD DAWKINS also exhibited a remarkably perfect javelin head of bronze which had been dug up in a field near Settle.

"Note on the Chromium Oxychloride described by Hr. Zettnow in Poggendorff's Annalen der Physik und Chemie, No. 6, 1871," by T. E. THORPE, F.R.S.E.

In the above-mentioned number of Poggendorff's Annalen* Hr. Emil Zettnow describes an oxychloride of Chromium to which he assigns the formula $Cr_2Cl_4O + 4CrO_3$. It is obtained by treating potassium chloro-chromate ($K_2Cr_2O_6Cl_2$) with strong sulphuric acid, and, after a somewhat tedious course of preparation, appears as a brownish black, brittle, amorphous substance, exceedingly hygroscopic, and giving up its chlorine with great ease. Hr. Zettnow's analytical results and the numbers required by his formula are :—

	Found.	Calculated.
Cr	47·28	47·23
Cl	22·31	21·42
O	—	31·35
		100.00

In the Proceedings of the Literary and Philosophical Society of Manchester for Nov. 2nd, 1869,† I described a solid chromium oxychloride obtained by simply heating chromyl dichloride in a sealed tube, and which, on completely freeing it from the latter body, "appears as a black non-crystalline powder, which, when exposed to the air, rapidly deliquesces to a dark reddish brown syrupy liquid, which smells of free chlorine" (loc. cit.) These properties, it will be observed, are precisely those which Hr. Zettnow describes as belonging to his chromate of chrom-oxychloride.

* See also "Chem. News," Sept. 15th, 1871.

† Also "Chem. News," Nov. 19th, 1869. Zeitschrift für Chemie, Jan., 1870. 95.

On analysis it yielded, as the mean of four determinations made on different preparations,

Cl	21·06
Cr.	48·91

numbers approximating to those obtained by Hr. Zettnow. To this compound I was induced, for reasons which I need not here reproduce, to give the formula

$$ClCrO_2.\ O.\ \overset{\prime\prime}{Cr}O.\ Cr.O_2Cl.$$

and to regard it as the chromium term of a series of salts a few members of which had already been described by Peligot, viz.—

Potassium chloro-chromate		.... $ClCrO_2.\ O.\ K_2.\ O.\ CrO_2Cl$
Sodium	do.	.... $ClCrO_2.\ O.\ Na_2.\ O.\ CrO_2Cl$
Ammonium	do.	.... $ClCrO_2.\ O.\ (NH_4)_2.\ O.\ CrO_2Cl$
Magnesium	do.	.... $ClCrO_2.\ O.\ Mg.\ O.\ CrO_2Cl$
Calcium	do.	.... $ClCrO_2.\ O.\ Ca.\ O.\ CrO_2Cl$,

The above formula for the chromium chloro-chromate requires

Cl	21·86
Cr	48·54

From the close agreement in the analytical results and correspondence in their physical properties, I am inclined to believe that Hr. Zettnow's compound is identical with mine. Potassium chloro-chromate heated with sulphuric acid yields, among other products, chromyl dichloride, and, doubtless Hr. Zettnow's compound has been derived from this body under circumstances analogous to those in which I have already operated. As my little notice on this matter has evidently not come under Hr. Zettnow's observation, he may be interested to learn that the six or seven weeks' time which he finds necessary to give to the preparation of this rather uninteresting compound may be considerably shortened by simply heating the chromyl dichloride in a closed vessel, when in a few minutes any wished-for quantity may be transformed almost completely into the chromium chloro-chromate and free chlorine.

"On Aurine," by R. S. DALE, B.A., and C. SCHORLEMMER, F.R.S.

In the July number of the Journal of the Chemical Society, we have published a short note on *Aurine*, a colouring matter discovered by Kolbe and Schmitt, in 1861, and which is now found in commerce under the name of aurine, yellow coralline, or rosolic acid. The commercial product which is obtained by heating phenol with oxalic and sulphuric acids, is a mixture of different bodies, from which we have isolated the pure colouring matter by dissolving the crude aurine in alcohol, and treating this solution with ammonia. A crystalline precipitate, a compound of aurine with ammonia separated out, whilst the other bodies present remained in solution. The ammonia compound was washed with alcohol by means of Bunsen's filter pump, and decomposed by dilute acetic acid. The aurine thus obtained was further purified by repeated crystallisation from strong acetic acid. It crystallised in rhombic needles or prisms, the colour of which varies according to the concentration of the acid, and as it appears also, according to the purity of the substance. We have obtained it in needles having the colour of chromic acid, and a brilliant diamond lustre, or in darker red crystals of varying shades, with a steelblue, greenish blue, or splendid beetle-green reflection. We have analyzed these different specimens, partly dried at 100° and partly at higher temperatures, and although samples of the same preparation gave very agreeing results, those of different preparations varied very much in their composition. The reason of this is, that aurine retains most obstinately water and acetic acid, a fact which has also been observed by Fresenius,* who has lately published a note on the same subject.

From concentrated hydrochloric acid aurine crystallises in fine, hairlike red needles, which, dried at 110°, contain a large quantity of hydrochloric acid. We tried to obtain the pure compound by precipitating a dilute alkaline with

* Journ. f. Pract. Chem., No. 10, 1871.

dilute hydrochloric acid, and washing the precipitate by Bunsen's filter pump, but also this product contains hydrochloric acid, which was only given off above 110°.

By spontaneous evaporation of an alcoholic solution, aurine is obtained in dull red crystals, with a green metallic lustre. Dried at 110° this body contains no alcohol, but still retains a large quantity of water, which only escapes between 140°—180°, the crystals not changing their appearance at all, and they may be heated up to 200° without any further alteration, which fact does not agree with Fresenius' observation, that aurine crystallised from alcohol or acetic acid melts at 156°. The analysis of this body, dried at 200°, which we believe to be pure aurine, gave numbers closely agreeing with the formula $C_{20}H_{14}O_3$ and the mode of its formation may, if this formula is correct, be expressed by the equation.

$$3C_6H_6O + C_2O_2 = C_{20}H_{14}O_3 + 2H_2O.$$

The substance dried at 110° lost at 180° 5·4% of water corresponding to the formula $C_{20}H_{14}O_3 + H_2O$.*

Caro and Wanklyn obtained by the action of nitrous acid on rosaniline a body, which they believe to be identical with aurine, and to which they assign, from the mode of formation, the composition $C_{20}H_{16}O_3$,† differing from our formula only by two atoms of hydrogen.

Nascent hydrogen converts aurine into colourless *leuco-aurine* $C_{20}H_{18}O_3$. This reduction may be effected by heating it in an alkaline solution with zinc dust, but at the same time a dark resinous body is formed, from which the leuco-aurine cannot be easily freed. Better results are obtained by acting with zinc dust on a solution of aurine in strong acetic acid. Leuco-aurine crystallises from acetic acid or alcohol in compact colourless prisms.

A body resembling leuco-aurine is contained in crude aurine; we have not as yet obtained it in a pure state. It

*Fresenius analysed aurine which was crystallized from alcohol and dried at 100°. His numbers agree exactly with the formula $C_{20}H_{14}O_3 + 2\frac{1}{2}H_2O$.

†Proceed. Roy. Soc. xv., 210.

differs from leuco-aurine, however, by yielding a purple solution on adding potassium ferricyanide to its alkaline solution, whilst leuco-aurine under the same conditions is oxidised to aurine, which dissolves in alkalis with a magenta red colour.

By passing sulphur dioxide into a hot alcoholic solution of aurine, brick red crystals separate, being a compound of aurine with sulphur dioxide. They do not smell of sulphur dioxide, undergo no change when exposed to the air, and are only decomposed at a temperature above 100°, when they split up into sulphur dioxide and aurine.

On mixing an alcoholic solution of aurine with a solution of a bisulphite of the alkaline metals, the liquid becomes colourless, a compound of aurine with the bisulphite being formed, which by spontaneous evaporation of the solution, is obtained in splendid, colourless, needles. These compounds are decomposed by acids as well as alkalis. We have not as yet analysed these different compounds, but intend to do so, hoping thus to find the correct formula for this remarkable compound.

By heating aurine with alcoholic ammonia in closed vessels to 110°, the so-called *red coralline* is obtained, a body which has great resemblance to the yellow aurine, but dyes a redder shade. This compound we have also obtained in fine crystals.

"Species viewed Mathematically." By T. S. ALDIS, M.A.

We have learnt that all energy is really one, whether seen in heat, constrained position or motion. Many also believe that life is really one, whether seen in man or a toadstool. But for our part we have often felt a difficulty. Why, if all life be one, do we not see it passing through every variety of form instead of being restricted to certain well defined types? The present paper is an attempt to explain this.

Let us consider what Plato might have called the *αὐτοζωον* or complete type of animal. It consists of a certain definite number of organs, composed of a certain definite number of parts. It will also have certain aliments, location, enemies, &c., which we may call its province, necessary for its life. Thus our type animal is capable of a flux passing through all possible forms and provinces in all possible combinations. I include amongst these of course many arrangements necessarily absurd. To each arrangement of organs and provinces thus imagined would correspond a certain vitality or power of living in the type. I mean not merely power of individual existence, but existence as a race.

The vitality is therefore a function of a large number of variables, some independent, others connected by equations of condition. It is to us quite an unknown function, but not therefore indefinite. Therefore, as in any other function of variables, certain relations amongst the variables will give maxima values of the vitality. These maxima of vitality constitute species. Vitality is not mere physical might or agility or fecundity, but compounded of all.

Now for a maximum, we know that any change in the variables lessens the function. We thus see how species are stable. In the constant variation, for no being seems capable of reproducing itself exactly, all individuals have less vitality as they depart from the special type which gives the maximum of vitality, and will be choked out by those which, being nearer to the type, possess more vitality. So Hybrids, intermediate between two maxima, will possess less vitality than either, and will be choked out, though the main cause of failure is that the process is like that devised by Swift's Laputan philosopher, who sawed the Whigs' and Tories' heads in half, and changing them, left each brain to settle its politics in itself. So the poor mule, with a bundle of habits, half horse and half ass, in this intestine conflict, has little power to take care of itself. Of course all maxima may not have plants or animals representing them. If there be several maxima suited for nearly the same province, the maximum of greatest intensity will choke out the others. So, too, there are probably many maxima now unoccupied, as for instance, the thistle represented a maximum of vegetable life in South America, but till man imported the thistle to fill it up, other maxima of less intensity held the ground. In some cases possibly several maxima are closely related, and differ little in their intensity, so that slightly differing species exist together, and may in their variation pass one into the other, as perhaps in brambles and some species of St. John's wort, &c.

If then the province of a species, *i. e.* the physical geography of a country alter, and its enemies and food with them, clearly the maximum will shift and the species change. But this is not the evolution of new species, though to a person who only notes geological evidence it appears so. For just as in a storm the lightning shews the trees still, though really waving to and fro, so the different species in geology are probably but steps in a constant change. Such a change of course must be slow for life to follow it, for a species consists quite as much in a bundle of acquired and transmitted habits as in a certain formation of organs, and the change in habit will probably be far slower than the change in form.

How then do new species arise ? For we see that, if the species be a maximum of vitality, in a multitudinous progeny those nearest the type will choke out the others and the species will be stable. Varieties will be connected with maxima of vitality in two ways. Firstly, slight differences in the province will slightly shift the maximum. Thus mountain sheep would be more agile than low land sheep. Secondly, in such a way as this. Suppose this table a low mound, narrow though long. Then the height at any point will be a function of the distances from the N. and E. walls of the room. There will be one point of maximum height, but whilst a change N. or S. produces a great change in the altitude, one E. or W. will produce but little. So there will be variations in some characteristics which will produce little alteration in the whole vitality. Thus amongst wild oxen probably no varieties without horns would exist, for they affect the vitality. Amongst protected races they do not, and so hornless varieties arise. Still these varieties are but varieties, and are not steps towards a new maximum which a gulf of lesser vitality still separates them from.

Or let us consider the varieties that we try to make by select breeding. These are least of all likely to produce new species. We simply by main force depress vitality in removing individuals as far as we can from the normal type, and when the vitality is sufficiently depressed we can go no further. As for altering the province, the independent variables, so to speak, we know so little how to do it, and certainly could not do it gradually enough, that we have no chance in this way of effecting anything.

How then can new species arise? Apparently in some such way as this, by what we may call the bifurcation of a maximum. If we drew a horizontal line along which the variation of the organs of an animal were expressed and the corresponding vitality were drawn by ordinates, we should get a curve we might call the vitality curve whose maxima values would be species. As time elapses and the conditions of the earth, &c., alter, the constants, so to speak, of the curve alter, and we get our curve to vary and the maxima shift; and as the curve alters, one maximum may separate into two or more others, and thus in the lapse of time one species may separate into two or more others. Roughly to illustrate it, suppose some species developed free from the influence of carnivora, and that, owing to various causes, size little effects its vitality, it may vary all through, from little and swift to big and heavy. Now, introducing carnivora, we can see how a bifurcation of our maximum would take place. The very light and swift would preserve themselves by their agility, the strong and heavy by their strength, whilst the intermediate would be killed out, and thus two distinct species would arise, which might in course of time by further variation separate still further apart.

Doubtless, however, this bifurcation goes back to very remote times. Carnivores and herbivores probably separated not as mammals but as reptiles, or even long before, whilst ruminants and non-ruminants may have separated since they became mammals.

Thus Australia seems to have possessed at one time only some marsupial, which has bifurcated into various marsupials, but not into any of another kind. The older the species grow the deeper is the gulf between them, and, like a river, we have to ascend nearly to the source before we can make a passage from one bank to the other.

To recapitulate.—Maxima of vitality are species. Any alteration from the normal type produces less vitality, hence the normal type is stable. A slow change of physical geography, &c., slowly changes these maxima, and the species change with them, extinct species being generally glimpses of steps in this change. New species will generally arise from the bifurcation of maxima under circumstances over which man can exercise little control, and which, if he could, he would very likely alter so as either hardly to affect the maximum at all, or too rapidly for the species to shift with it. Selected breeding produces types of less vitality, and therefore will hardly produce new species. Thus the present stability of species is no argument against the doctrine of evolution.

We hope we have not trespassed on the time of the Society in thus putting before them not new views, but perhaps a slightly new aspect of old views. Still as we felt a difficulty and thought we saw a solution, we felt we might ask their opinion upon it.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 9th, 1871.

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

"Notices of several recently discovered and undescribed British Mosses," by G. E. HUNT, Esq.

Gymnostomum Calcareum, N. and H., var. *brevifolium*, B. and S. *Gymnostomum viridulum*, Bridel.

Perennial? dioicous; stems cœspitose, sparingly branched, very slender, a third of an inch in height, of a reddish brown colour below, upper part pale green, slightly glaucous; leaves ovate or ovate lanceolate, with erect bases, thence spreading, papillose, margin crenulated in the upper part; cells in the upper portion of the leaf opaque, quadrangular, in the lower portion elongated, sub-diaphanous; nerve thick, papillose, extending almost to the apex. Male flowers gemmiform, on very short axillary branches which usually spring from an innovation; perigonial leaves ovate, suddenly acuminated, nerved to the apex.

I have not seen female flowers or fruit.

Habitat: Rocks at Blackhall, near Banchory, where it was discovered by Mr. John Sim.

Entosthodon minimum, Hunt, sp. nova. Annual, dioicous; stems gregarious, erect, an eighth to a quarter of an inch high; lower leaves obovate, margin reflexed, nerve thin, vanishing below the apex; upper leaves oblong, suberect, subcanaliculate, margin recurved, crenulate in the upper part, nerve rather strong, produced almost to the apex; areolæ large, those of the lower part of the leaf elongate-hexagonal, of the upper part shorter.

Male plants with the flowers terminal, antheridia 6 to 8, sessile, without paraphyses, perigonial leaves usually like the upper stem leaves, but occasionally (together with all the stem leaves) obovate, when they contain clavate, slightly swollen paraphyses, without antheridia.

Female plants with the flowers both terminal and in the axils of the upper stem leaves, archegonia with a few rather long filiform paraphyses; no distinct perichœtial leaves; vaginula short, cylindrical; seta an eighth to a quarter of an inch long, erect; capsule with a distinct neck, smooth, when dry obconical, widest at the mouth, operculum conical acute. Calyptra when young brown, very narrow conical, cleft on one side for a third of its length, cells spirally arranged; peristome half immersed, teeth sixteen, very slender, linear-subulate, transverse articulations distant.

Fruit matures in August. Discovered near Glasnevin, Dublin, on the top of a sandstone wall, by Mr. David Orr.

It has no nearly related European allies.

Webera Breidleri, Juratzka (*fide* Fergusson). Dioicous, growing in extended light green patches, procumbent in the lower part, which is of a reddish brown colour; stems about 1½ inch long, leaves ovate, decurrent, erecto-patent, concave, serrated towards the apex, margin recurved; nerve thin, vanishing below the apex; areolæ rather large, upper ones narrow elongate, acute at both ends, lower ones narrow elongate-quadrangular. Male flower terminal, discoid; outer perigonial leaves spreading, elliptic-lanceolate, longer than the stem leaves, saccate at the base, margin strongly recurved, apex cucullate, serrated; inner perigonial leaves obovate, suddenly acuminated, serrated at the diaphanous apex, areolæ large, elongate-quadrangular; antheridia sub-sessile with short filiform paraphyses. Perichœtial leaves linear lanceolate, recurved at the margin, strongly nerved, nerve vanishing below the apex; seta geniculate near the base, slender; capsule oval pendulous, glaucous green when young, pale reddish brown when mature.

Fruit matures July to August. Habitat: Abundant on wet debris of slaty rocks near springs, on the table lands above the head of Glen Callater, also on Loch-na-gar, and in Canlochan Glen. Its companions above Glen Callater are *Dicranum Starkii*, *D. falcatum*, *Oligotrichum hercynicum*, and *Polytrichum sexangulare*. In the springs themselves abound the following, viz.—*Philonotis*, several species; *Splachnum vasculosum*, *Mnium cinclidioides*, and several allied species; *Hypnum exannulatum*, *H. falcatum*, *Thuidium decipiens*, *Webera albicans*, var. *glacialis*, and numerous other interesting plants.

Webera Ludwigii differs in its narrower, hardly concave, patulous leaves, more strongly decurrent; with larger, longer, and more diaphanous areolæ. The whole foliage also is frequently of a fine red colour. Fruit matures in August. Habitat: Abundant on the fine debris of granitic rocks, by streamlets issuing from the perpetual snow beds near the summits of Ben-mac-Dhui, Ben-na-Boord, and doubtless all the other mountains of like character. On the slaty formations it is rare, and I have only seen it by a streamlet in one small ravine above Glen Callater, where in the middle of July the snow was lying abundantly.

Webera Schimperi, Wils. (not of B. & S. Bry. Eur.), has leaves more rigid, erect, narrow lanceolate, less decurrent; nerve stronger, continued almost to the apex; areolæ a little longer, more obscure. Fruit matures in July. Habitat: Frequent on debris of micaceous rock, on Ben Lawers, and on most of the other Perthshire mountains. It also occurs on debris near the summit of Snowdon, but barren.

Philonotis adpressa, Ferg. Plant widely cœspitose, erect, two or three inches high, either dull glaucous green, or with a fine red tinge; leaves papillose, when moist erect, with one wide plica on each side of the nerve, incurved towards the apex, when dry slightly twisted, widely ovate, from an amplexicaul base, not acuminate, apex either obtuse and cucullate, with a very slight mucro, or in the more slender forms of the plant rather acute; margin denticulate, slightly reflexed; nerve very thick, continuous; cells in the upper part of the leaf small ovoid, towards the base a little shorter and wider. I have seen neither flowers nor fruit.

Habitat: Glen Prossen, Clova, and various other places in the Clova district—Rev. J. Fergusson. Glas Mheal, Perthshire, at an elevation of 2,500 feet—G. E. Hunt. In the letter station it was accompanied by *Thuidium decipiens*, De Not.; *Bryum Duvalii*, *Splachnum vasculosum*, and other rare species.

The allies of Philonotis adpressa may be distinguished from it as follows.

Philonotis calcarea has longer, secund, very acute leaves, with areolæ twice or thrice larger, oblong, basal areolœ larger, elongate-hexagonal.

Philonotis fontana has leaves usually spreading, but sometimes secund, longer, suddenly acuminated half way up, very acute, very distinctly plicate, margin strongly recurved, nerve mu ch thinner, areolæ linear above, small and oblong towards the base of the leaf.

Philonotis seriata, Mitt., has leaves with a distinctly spiral arrangement, from a suberect base, patent towards the apex, ovate, acute, plicate, margin distinctly reflexed; areolæ linear above, small and ovoid towards the base of the leaf; perigonial leaves from an erect dilated base which is composed of rather large linear cells with a red tinge, upper part of leaf widely spreading, cordate triangular, obtuse, areolæ elongate-quadrangular, very small and obscure, nerve thick and indistinct, continuous or vanishing below the apex, margin slightly denticulate. This species was first described in Mitten's Musci Indiæ Orientalis, in the Proceedings of the Linnean Society for 1859. It is frequent in springs at the head of Clova, fruiting freely in favourable seasons.

Thuidium decipiens, De Not.; Hypnum rigidulum, Ferg. MSS. This species was lately described by the Rev. J. Fergusson in Science Gossip, and noticed in Journal of Botany, October, 1871. It had been collected in 1866 on Ben Lawers by Dr. Stirton, but was for some years confounded with Hypnum commutatum, to which species it bears much resemblance. The Rev. J. Fergusson, however, satisfied with its distinctness, distributed it in 1870 as *Hypnum rigidulum*, Ferg., species nova; and a few months since Juratzka identified it with Thuidium decipiens, De Notaris, Briologia Italiana, 1869. It occurs in springs, and is found in Britain on Ben

Lawers and Glas Mheal, Perthshire; at Auchinblae, Kincardineshire, first observed by Mr. John Sim; and abundantly in various places in Clova and Braemar, first observed by the Rev. J. Fergusson. From every form of Hypnum commutatum it is at once separated by its papillose leaves with much dilated auriculate bases; by its larger alar cells; by the ovoid cells of the upper portion of the leaf, those of H. commutatum being linear; by its monoicous inflorescence, and by the time of the fruit, which is at maturity in autumn. Fruit has been found only in Italy and in South Prussia.

Mr. CHARLES BAILEY distributed specimens of *Æcidium Statices,* Desm., which Mr. John Barrow and he had found in some abundance on a species of *Statice* (probably *S. Limonium*) on the 3rd of June last, on the eastern shore of Walney Island. The *Statice* occurs on ground covered each high tide, on Tummer Hill Marsh, near the Water Garth Nook. This leaf fungus had been announced in "Science Gossip," 1st July, 1871, as new to Britain, it having been found by Mr. R. S. Hill on the low muddy shores of Southampton Water.

Mr. Bailey mentioned that the *Urocystis pompholygodes,* Sch., also occurred on Walney Island in great plenty near Bent Haw Scar, on *Thalictrum eu-minus,* a. *maritinum* E.B.; also that *Æcidium crassum,* Pers., was common on *Cornus Mas.* L., at Silverdale, Warton Crag, and other places in North Lancashire.

"Notes on Dorcatoma bovistæ," by Mr. JOSEPH SIDEBOTHAM, F.R.A.S.

In August, 1857, my friend Mr. Kidson Taylor found some larvæ in small fungi, on the coast at Barmouth, and from them bred a number of the rare Dorcatoma bovistæ. Each year since he has had sent to him, by a friend at Barmouth, a box of fungi, gathered in the same place, but has not succeeded in obtaining from them a single specimen. Our associate Mr. Linton and I spent a few days at Barmouth last month, and having been informed by Mr. Taylor of the exact locality where he met with the Dorcatoma, we determined, if possible, to find it again. The place indicated is situated between the railway and the shore, and consists of a flat common joining up to the sandhills. Here the most conspicuous and interesting plant to a botanist is Juncus acutus, which occurs in very large tufts, the pretty little Neottia spiralis, was also abundant, and, on the sandhills, Iberis amara, and other scarce plants.

Scattered over this common we found many fungi, in all stages of growth—*Bovista nigrescens, Bovista plumbea, Geaster limbatus,* and another smaller species, and one or two species of Boletus. We carefully examined these in search of larvæ, but for some time without success. At length we found a few in very small dry specimens of Bovista plumbea. We then collected what we could find in the same condition. In less than a week several perfect specimens of Dorcatoma bovistæ made their appearance, and others have since continued to do so very sparingly. Sometimes the larva eats its way out of the fungus and at once changes into the pupa state, from which it becomes the perfect insect in about ten days, but usually it forms a cocoon of spores, changes to the pupa state inside the fungus, and the perfect insect eats its way out.

We found Bovista plumbea in all stages of growth, from the size of a pea to the old dry specimens in which were the larvæ, but found no traces of larvæ in any of the fresh ones, although it seems most probable that eggs had been laid and hatched in some of them; probably the larvæ were too small to be easily discovered.

The antennæ of D. bovistæ are very curious, especially in the male, and it would be interesting to discover the reason for their singular formation, suited, no doubt, to their peculiar mode of life.

I have sent for exhibition a few specimens of the Dorcatoma, with legs and antennæ displayed, also folded together, in which state they look more like seeds than insects; a specimen of the pupa case, some of the larvæ, and a specimen of Bovista plumbea probably containing others.

Ordinary Meeting, November 14th, 1871.

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

Mr. Watson Smith, Jun., F.C.S., was elected an Ordinary Member of the Society.

The President said that the Society had lately lost by death one of its most distinguished Honorary Members, Sir R. J. Murchison, Bart., a geologist of world-wide reputation. He had enjoyed the privilege of Sir Roderick's friendship for over thirty-five years, and he could fully confirm all that had been stated in the public prints of the deceased's great scientific attainments, his liberal patronage of science, and his kind and good heart; but there was one quality, namely, that of learning to the last and being ever ready to alter his views as new facts were discovered, that, in his opinion, had not been sufficiently noticed. For many years he (the President) and Sir Roderick had held different views as to the geological age of certain rocks in Yorkshire, and latterly, on more careful examination of the district by the officers of the Geological Survey, the latter changed his opinion. Immediately on their doing so he wrote as follows:—

"Belgrave Square, 4th June, 1869.

"Dear Binney,

"My geological surveyors have, I understand, come to the conclusion (though nothing has yet been published on it) that the Plumpton Rocks, near Knaresborough, belong to a well-defined band of the Millstone Grit Series.

"I have mislaid and cannot find your paper in which you expressed the same opinion, in opposition to the views of

Sedgwick, Phillips, and myself. If so, please to refer me to your paper, which, if I mistake not, had an accompanying diagram. In this case you will be happy to have your views confirmed.

"I connected the Plumpton Rocks with the red sandstone which, underlying the magnesian limestone of Knaresborough, is unequivocally *Permian*. But I could not connect the two stratigraphically, and I came to my conclusion merely through the close lithological similarity of the Plumpton Rocks to the well-known beds of the German Rohte Liegende.

"Never too late to admit errata to the end of my Chapter of Life.

"May you work on as steadily and successfully as you have done in this, and many a year to come.

"Yours sincerely,

"ROD. J. MURCHISON."

Such a letter speaks volumes for the love of truth and the kind heart of the deceased geologist whose loss is so deeply deplored.

The PRESIDENT said that, on Friday the 10th instant, he observed, at Douglas, in the Isle of Man, a splendid display of the aurora borealis. At 8 p.m. it appeared as an arch of a greenish colour, extending from west to east, through the tail of the Great Bear. Afterwards, at 10 o'clock, the same kind of arch was observed with another higher up, which ranged west and east through the Pole Star. At this time numerous streamers and flashes of light of a green and yellowish-white colour flashed up from near the horizon to the zenith, from east, south, and west; those towards the west had a reddish hue. The sky was beautifully clear and the light from the aurora was greater than ever previously observed by him.

"On the Origin of our Domestic Breeds of Cattle," by Wm. Boyd Dawkins, F.R.S.

Mr. Boyd Dawkins then made some remarks on the origin of our domestic cattle. There are at the present time three well marked forms inhabiting Great Britain. 1. The hornless cattle, which have lost the horns which their ancestors possessed through the selection of the breeder. The polled Galloway cattle, for instance, are the result of the care taken by the grandfather of the present Earl of Selkirk, in only breeding from bulls with the shortest horns. The hornless is altogether an artificial form, and may be developed in any breed. 2. The *Bos longifrons,* or the small black or dark brown Welsh and Scotch cattle, which are remarkable for their short horns and the delicacy of their build. 3. The red and white variegated cattle, descended from the urus, and which have on the whole far larger horns. These two breed freely together, and consequently it is difficult to refer some strains to their exact parentage.

The large domestic cattle of the urus type are represented in their ancient purity by the Chillingham wild oxen, as they are generally termed, but the exact agreement of their colour with that specified in the laws of Howel Dha proves that they are descended from an ancient domestic cream-coloured ox with red ears. The animal was introduced by the English invaders of Roman Britain, and was unknown in our country during the Roman occupation.

The *Bos longifrons,* on the other hand, was the sole ox which was domestic in Britain during the Roman occupation, and in the remote times out of the reach of history it was kept in herds by the users of bronze, and before that by the users of polished stone. This is proved conclusively by the accumulations of bones in the dwelling places and the tombs of those long-forgotten races of men.

The present distribution of the two breeds agrees almost exactly with the areas occupied by the Celtic population and the German, or Teutonic, invaders. The larger or domestic urus extends throughout the low and fertile country, and indeed through all the regions which were occupied by Angle, Jute, Saxon, or Dane, while the smaller *Bos longifrons* is to be found only in those broken and rugged regions in which the unhappy Roman provincials were able to make a stand against their ruthless enemies. The distribution, therefore of the two animals corroborates the truth of the view taken by Mr. Freeman, that the conquest of Britain by the English was not a mere invasion of one race by another, but as complete a dispossession as could possibly be imagined. The *Bos longifrons* lingers in Wales, after having once occupied the whole country, just as its Celtic owners still linger, while the urus is an invader just in the same sense as their English possessors. Both these animals were kept in a domestic state on the Continent, and they make their appearance with all the domestic animals, except the cat, in the possession of the dwellers on the Swiss lakes in the neolithic age. The *B. longifrons* is of a stock foreign to Europe, and the urus most probably was domesticated in some other region by those neolithic people. Both these animals have probably been derived from an area to the south and east of Europe, and were introduced by the neolithic herdsmen and farmers at a very remote period.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 6th, 1871.

JOSEPH BAXENDELL, President of the Section, in the Chair.

"On Tricophyton tonsurans," by Mr. JOHN BARROW.

Tricophyton tonsurans is the name now given to a vegetable parasite which lives in and upon the skin of man and some of the lower animals.

For some months past this parasite has forced itself under my attention, and I have been anxious to obtain the best information concerning it, and, believing that the observations I have made may be of interest to the Section, I will state what they are.

Three forms of disease are known to which the popular, or unpopular, name of ringworm is applied, viz.—ringworm of the scalp, ringworm of the body, ringworm of the chin, and another nearly allied, the liver spot.

There appears little doubt that, of these three, the two first are identical; but, as I have not had any opportunity of observing any but the second—that of the body—I will confine myself to that particular form.

The first indication of the presence of this parasite was on a child eight years old. A red ring appeared on the face, about an inch in diameter, the edges being slightly raised, and the centre rough and somewhat scaly. This was declared to be ringworm, or herpes circinatus, by one authority, and sulphurous acid was applied with success. Very soon afterwards several patches appeared on the child's body, varying from ¼in. to 2in. diameter. Sulphurous acid was not successful here, and carbolic and nitric acids were used, but successive growths in various parts of the body occurred during a space of some twelve months. Meantime the adults in the same family were one after another subject to the same attacks. In one case of a very obstinate nature only one spot, about one inch in diameter, appeared on the upper lip; this was treated at once with carbolic acid, or benzol, and the cuticle in two or three days was renewed, and the spot had apparently disappeared. In a few days a ring, external to the one destroyed, began to show itself. This was again destroyed with carbolic acid, and then an irregular growth commenced, the ring, although interrupted, was yet easily seen in the position that the various patches occurred upon the face, nose, temples, and forehead, the hairs of the upper lip being the worst.

Three names were given, by another authority, to the disease at this stage, viz.—favus, tinea circinatus, and tinea sycosis. It was at this stage that I made the microscopical examination of the hairs of the upper lip, and at the same time became aware of the unsatisfactory state of our knowledge on this and kindred subjects.

For a long time all my efforts were fruitless. I could neither get spores nor mycelium, nor anything giving indications of what I sought. Having obtained some of the hairs shaved from the upper lip, and having washed these with absolute alcohol, then with benzol, and afterwards mounted them in balsam, mycelium chains became distinctly visible, clothing the diseased hairs very thickly.

This was sufficient proof of the fungoid character of the parasite, but I wanted to see the spores also.

Chancing to examine the alcohol with which the hairs had been washed, small transparent bodies were seen, which looked like spores. These, mounted in glycerin, changed their shape, appearing to swell out and lose their character, and in balsam becoming so transparent as to escape detection.

Having examined these bodies in alcohol alone with more care, I had no doubt that they were the true spores removed from their attachment by the action of washing. I have yet to see these spores *in situ.*

The slides I present to the cabinet of the Section will show that the diseased hairs are covered by nucleated cells, square, attached end to end, and branching in all directions. This is the mycelium, or what I hold to be the true parasitic plant. It possesses the same relation to the spore that a tree does to its seed, and, if we keep this in view, the life-history in the main of most, if not all, these plants becomes easily understood. The full and complete life-history, which must include of necessity the mode in which reproduction takes place in plants so minute as these, requiring $\frac{1}{4}$in. object glass even to see them, will probably long remain unwritten, but analogy leads us to expect that at some period of the life of these plants, and in some way or other, a true sexual process of reproduction does take place.

There is no doubt that the spores, which you will see on the slides presented, give existence to the mycelium, and then this again produces filaments bearing the spores. These filaments must not be confounded with the mycelium. The cells of these filaments having very different characters. Infection or contagion (one or both) will then take place whenever the spores find a resting place upon the skin of animals in that condition of health suited for their development. In the cases that came immediately under my notice the worst occurred where bodily health was impaired, whereas contagion did not take place in one instance, even though the boy slept regularly with his brother for months during the continuance of the disease.

I was quite unable to obtain mycelium from the *skin* of the face in the case of the adult. The disease travelled all over the face, leaving the beard and whiskers unattacked; but although the hair folicle of the upper lip was filled with mycelium, I could not get it from the skin.

I believe this to be the reason of the obstinacy of the disease; the mycelium had burrowed deep down into the skin, beyond the reach of ordinary parasiticides, and thence sent to the surface the spore-bearing filament. The cuticle was repeatedly destroyed by both carbolic and nitric acids without the destruction of the parasitic plant.

Taking this view of the subject, I venture to suggest that the true mode of attacking these plants will be found to be by sealing them up, whenever they appear, from the action of light and air, the two necessities of plant growth; but, as it is known that fungoid growths require a larger supply of oxygen than the flowering plant, partaking more of the nature of animal life, the exclusion of air ought to be of especial benefit. I am now trying an old remedy which ought to have this effect of excluding light and air, viz., varnishing the affected part with a thick coating of tar varnish, but I cannot as yet speak of the result.

I had intended to have given the result of my *search* after knowledge among the hand-books on the subject of skin diseases, but perhaps it will be sufficient to say that I found more confusion than knowledge, and that the only safe conclusion I have as yet arrived at is that it is the imperative duty of every botanist and microscopist to do what in him lies to throw light upon this subject of vegetable parasites, which are not only disfiguring, depressing, and painful, but in many cases continue their growth for years on the same individual.

Ordinary Meeting, November 28th, 1871.

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

Mr. Richard Samuel Dale, B.A., was elected an Ordinary Member of the Society.

"Encke's Comet, and the Supposed Resisting Medium," by Professor W. STANLEY JEVONS, M.A.

The observed regular diminution of period of Encke's comet is still, I believe, an unexplained phenomenon for which it is necessary to invent a special hypothesis, a *Deus ex machina*, in the shape of an imaginary resisting medium. I cannot be sure that the suggestion I am about to make has not already been made, but I have never happened to meet with it; and therefore I venture to point out how it seems likely that the retardation of the comet may be reconciled with known physical laws.

It is asserted by Mr. R. A. Proctor, Professor Osborne Reynolds, and possibly others, that comets owe many of their peculiar phenomena to electric action. I need not enter upon any conjectures as to the exact nature of the electric disturbance, and I do not adopt any one theory of cometary constitution more than another. I merely point out that if the approach of a comet to the sun causes the development of electricity arising from the comet's motion, a certain resistance is at once accounted for. Wherever there is an electric current some heat will be produced and sooner or later radiated into space, so that the comet in each revolution will lose a small portion of its total energy. In the experiments of Arago, Joule, and Foucault the conversion of mechanical energy into heat by the motion of a metallic body in the neighbourhood of a magnet was made perfectly manifest. If then there is any magnetic relation whatever between the sun and comet, the latter will certainly experience resistance.

The question is thus resolved into one concerning the probability that a comet would experience electric disturbance in approaching the sun. On this point we have the evidence now existing that there is a close magnetic relation between the sun and planets. If, as is generally believed, the sun-spot periods depend on the motions of the planets, a small fraction of the planetary energy must be expended. I find, indeed, that a very brief remark to this effect was given in the memoir of the original discoverers of the relation, namely, Messrs. Warren de la Rue, Balfour Stewart, and B. Loewy. At p. 45 of their Researches on Solar Physics they add a small note to the following effect: "It is, however, a possible enquiry whether these phenomena do not imply a certain loss of motion in the influencing planets." As I conceive, no doubt can exist that periodic disturbances depending upon the motions of bodies must cause a certain dissipation of their energy, for if stationary the constant radiation of the sun could not produce any periodic changes, unless the sun were itself variable. Is there not then a reasonable probability that the light of the Aurora represents an almost infinitesimal fraction of the earth's energy, and that in like manner the light of Encke's comet represents a far larger fraction of its energy? It is also worthy of notice that the tail of a comet is usually developed most largely at those parts of its orbit where the rate of approach or recess is most rapid, and where the electric disturbance would be correspondingly intense.

I do not, of course, deny that the resisting medium may nevertheless exist, or may by other observations or experiments be made manifest. But I hold that so long as other physical causes can be pointed out which might produce the same effect, it is quite unphilosophical to resort to a special hypothesis. Encke's comet ought not to be quoted as evidence of the existence of such a medium until electric disturbance is shown by calculation to be insufficient to account for the observed diminution of period.

"On Cometary Phenomena," by Professor OSBORNE REYNOLDS, M.A.

In all comets which have been observed through powerful telescopes there is an action going on which appears to be the result of evaporation. Jets of something like vapour are seen to issue from what is supposed to be a solid nucleus on that side which is towards the sun.

No such signs of evaporation are observed on the planets, nor is there any phenomenon, that we are aware of, which can be compared with this taking place on our earth. At first sight it seems strange that the sun should act to more effect on such small bodies as comets than it does on the larger bodies, even when the latter are nearer to it than the former. When, however, we come to look closer, I think good reason may be given for this; and I think that the difference of evaporation on the earth and on a comet may probably be the cause of electrical phenomena existing on the latter which certainly do not exist on the earth, and that the relation between the motion of the comet and the evaporation which might be expected to take place is precisely that which is observed between the motion and those appearances which I would explain on an electrical hypothesis.

The first thing to be done is to take notice of the following facts:—

1. Comets move in very eccentric orbits, whereas the planets move in orbits nearly circular.

2. Comets are supposed to be much smaller than the planets.

3. All the heat received by a body from the sun must be expended in one or other of the following ways:—

I. By radiation from the body.

II. By evaporating the materials.

III. Producing chemical change in these materials, or in electrical separation, &c.

That spent in the third method may be considered small. Thus

the heat which a body receives=heat radiated+heat spent in evaporation (1)

and

$$\frac{\text{heat radiated}}{\text{heat received}} = (\text{some constant}) \times \frac{\text{temperature of body}}{(\text{distance of sun})^2} \quad (2)$$

Now the temperature at which any given material, say water, would evaporate would be much lower on a comet than on a planet, on account of the comet being so much smaller. For we may assume that there is a limit to the pressure which an atmosphere of vapour of unlimited extent can exert on the materials of the body it envelopes, then the limit of the temperature of the body will be that which will evaporate the material of the body under this pressure. It is clear that if there be such a limit it must increase very rapidly with the mass of the solid body, and hence that it would be much higher in the case of a planet than in that of a comet. This temperature may be called that of permanent evaporation, for as long as it was maintained the body would continue to evaporate; therefore the temperature of permanent evaporation of the planet would be much greater than that of the comet. That is, from equation (2,) the ratio of the heat radiated away to that received would be much less in the case of the comet than in the case of the planet, leaving, by equation (1), a greater ratio for evaporation in the former than in the latter.

Now it is clear that our earth is well out of reach of this permanent evaporation; for the temperature at the equator is much less than 212° F., which is the boiling point of water, its most volatile substance; and we may assume that the same is the case with all the other planets. If, however the earth's atmosphere were removed, then evaporation would go on until there was another atmosphere formed which would hold the liquid in check. If, however, the earth had no attraction for vapour, or only a very slight one, then it would go on evaporating, in the first place, until all the water was ice, and then it would spend all the heat it got from the sun in vapour. This, according to Sir J. Herschel's rate, is sufficient to melt ice just enough to reduce the diameter of the earth by an inch in about four hours and a half, and if it had to evaporate the water as well as melt the ice it would evaporate about one inch in 130 hours. Now, although this is a purely imaginary condition with regard to the earth, yet it must exist in the case of a small body like a comet; that is to say, there would be no liquid on the comet even when evaporation was going on, and, when the comet was near enough the sun, permanent evaporation would go on, which would only be ended by the comet removing itself, or by the exhaustion of the volatile material. This latter would take place supposing a comet should change its orbit when near the sun into a circular orbit, like a planet or meteorite. Even in the case of a periodic comet there must be some exhaustion of the volatile materials. During the two hours in which the comet of 1843 was within close approximation to the sun, if the comet had been made of ice covered with lamp black it would have received the heat of 47,000 suns according to Sir J. Herschel's computation. This would have evaporated the ice at the rate of 55 feet per hour on that side next the sun, or 13 feet over the whole comet. But in fact, owing to the protection of its atmosphere and imperfect absorbing power, it would have been much less than this, that is to say, the diameter of the comet would not have been reduced 10 feet. However it may be that all the material evaporated is not lost. For, from the way in which comets approach and recede from the sun, it is probable that part of their orbit lies without and part within the range of permanent evaporation. Hence during part of their motion, when they are distant from the sun, condensation will be going on if there is anything to condense. This agrees well with the observed fact that a periodic comet makes less and less display each revolution. There the heat acts on the surface of the comet so that the less volatile substances would form a skin over the softer ones, through which the heat would have to pass, and through which the steam would have to force its way in jets.

Now such jets as these would act the same part as the jets in Armstrong's hydro-electrical machine, and the vapour which emerged would be charged with either positive or negative electricity as the case might be, the solid being charged with electricity of the opposite kind.

The vapour as it formed an atmosphere round the nucleus would then discharge some of the electricity back. This would cause those portions which were nearest the nucleus to be bright (self-luminous), brighter than the more distant. Although the variations in temperature would be slight, yet as the atmosphere moved outwards from the nucleus there would be expansion, and consequently condensation; hence the outside of the coma might be illuminated by the direct rays of the sun, or we might have several bands of condensed vapour so illuminated, as suggested by Sir J. Herschel. On the other hand, I think this illumination may be due at least in part to the electric action between the matter of the comet and matter previously in space. This point will probably be settled by Mr. Huggins when the next large comet makes its appearance.

The period of greatest display is not reached till after the comet has passed its perihelion, and the tail is visible for much longer after this than it was before it. Now, if we suppose the comet to be made up of hard and volatile substances, owing to the heat absorbed by the hard substances the evaporation would lag somewhat behind the position of the comet, and consequently be greatest after it had passed its perihelion distance, just as a thick retort will continue to boil after the lamp has been removed. Hence we see that if the evaporation causes electrical separation in the comet, this will be at its maximum just when the display is observed to be at a maximum.

This communication is not intended as an alteration of the views which I expressed in a former communication, but as an extension of those views, for I formerly advanced no hypothesis as to the possible cause of the electricity. Also with regard to the formation of tails, I wish to add somewhat to my former remarks. Professor Norton has shown that the primary tail of Donati's comet might have been formed by matter emitted by the comet and repelled by the sun with a force equal to from ·75 to ·55 the attraction of the sun for ordinary matter. The matter repelled with ·55, forming the following edge, that with ·75, the leading edge of this tail. Professor Norton suggests that these forces may be electrical or magnetic.

Accepting Professor Norton's calculations as correct, it is certain that if for some cause or other the sun repelled negative electricity, and there were two streams of electrified matter leaving the comet, charged in the ratio of ·75 to ·55, these would be repelled in the ratio he wants; at the same time I do not think he has sufficiently taken into account the repulsion one stream would have on the other.

Professor Norton does not suggest an explanation of the straight tails seen with most comets as primary or secondary tails. These I maintain can only be explained on the supposition that there is matter in space in the form of gas, and that the comet causes it to be electrically illuminated by a brush, as I stated in my former communication.

Again, if the tail of the comet be electricity of one kind (say negative), leaving the comet never to return, then the comet must leave the neighbourhood of the sun with a charge of positive electricity, which, as it gets further from the sun and evaporation becomes feeble, will in time overpower the negative electricity in the atmosphere, which will then be attracted by the sun instead of repelled, and if the comet has any tail it will now turn away from the sun; in which condition it will probably remain until its approach to our sun or some other star again cause it to become negative and turn round. In this case a periodic comet would turn its tail round at definite points in its orbit, and owing to the lagging of the symmetri of the comet's appearance in its orbit the point of turning will be nearer to the sun on its return than on its departure. Now, it seems from a remark of Professor Airy that comets, when first seen, often have their tails before them, and that such is the case with Encke's comet now visible.

"On the Rupture of Iron Wire by a Blow," by JOHN HOPKINSON, B.A., D.Sc.

The usual method of considering the effect of impulsive forces, though in most cases very convenient, sometimes hides what a more ultimate analysis reveals. The following is an attempt to investigate the effect the blow of a moving mass has on a solid body in one or two simple cases; I venture to lay it before the Society on account of its connexion with the question of the strength of iron at different temperatures.

I assume the ordinary laws concerning the strains and stresses in an elastic solid to be approximately true, and that if the stress at any point exceed a certain limit rupture will result. Take the case of an elastic wire or rod, natural length l, modulus E, fixed at one end, the other end is supposed to become suddenly attached to a mass M moving with velocity V, which the tension of the wire brings to rest. The wire is thus submitted to an impulsive tension due to the momentum MV, and according to the usual way of looking at the subject of impact, the liability to rupture should be independent of l and proportional to MV. But in reality the mass MV is pulled up gradually, not instantaneously, and the wire is not at once uniformly stretched throughout, but a wave of extension or of tension is transmitted along the wire with velocity a when $a^2 = \frac{E}{\mu}$ (μ being the mass of a unit of length of the wire); in an infinite wire this wave would be most intense in front, as in the figure in which the ordinates are proportional to the tension. In the wire of length l this wave is reflected at the fixed point, and returns to the point of attachment of the mass M, and the effects of the direct and reflected waves must be added, and again we must add the wave as reflected from M back towards the fixed point. The question then of the breaking of the wire is very complicated, and may depend not merely on the strength of the material to resist rupture, but also on a, E, and l, and on M and V independently, not only on the product MV.

First take the case of an infinite wire; let x be the unstretched distance of any point from the initial position of the extremity which is fast to M, $x + \xi$ the distance of the same point from this origin at time t. The equation of motion is

$$(1) \quad \frac{d^2\xi}{dt^2} = a^2 \frac{d^2\xi}{dx^2}$$

and we have the condition

$$(2) \quad M\frac{d^2\xi}{dt^2} = E\frac{d\xi}{dx} \quad \text{when } x = 0.$$

The general solution of (1) is $\xi = f(at - x)$.

Substitute in (2) and put $x=0$.

$Ma^2f''(at)=-Ef'(at)$; but $a^2=\frac{E}{\mu}$,

Therefore $Mf'(at)=-\mu f(at)-\frac{MV}{a}$;

for initially $f(at)=0$ and $f'(at)=-\frac{V}{a}$;

Therefore $\frac{Maf''(at)}{\mu f(at)+\frac{MV}{a}}=-a$.

$$\mu f(at)+\frac{MV}{a}=\frac{MV}{a}\varepsilon^{-\frac{\mu at}{M}}$$

Therefore $\xi=-\frac{MV}{\mu a}\left(1-\varepsilon^{-\frac{\mu(at-x)}{M}}\right)$ true at any point after $t>\frac{x}{a}$

Tension $=E\frac{d\xi}{da}=\frac{VE}{a}\varepsilon^{-\frac{\mu}{M}(at-x)}$. This is greatest when $at-x=0$, and then $=V\sqrt{E\mu}$.

So that for the case of an infinite wire it will break unless the statical breaking force $>V\sqrt{E\mu}$; a limit wholly independent of M. This result is approximately true in the case of a very long wire: if F be the force which acting statically would break the wire, velocity necessary $=\frac{F}{\sqrt{E\mu}}$.

Any change then, which increases E will render such a wire more liable to break under impact: cold has this effect; we arrive then at the apparently anomalous result that though cold increases the tensile strength of iron, yet owing to increasing its elasticity in a higher ratio it renders it more liable to break under impact.

Now let us return to the case of the wire length l. We have the additional condition that when $x=l$ $\xi=0$ for all values of t, and this will introduce a number of discontinuities into the solution. Up to the time $\frac{2l}{a}$ we may deduce the solution from the previous case; from $t=0$ to $t=\frac{l}{a}$ we have as before

(3) $\xi = \frac{MV}{\mu a}\left(\varepsilon^{-\frac{\mu(at-x)}{M}} - 1\right)$

but then reflexion occurs, and we have

(4) $\xi = \frac{MV}{\mu a}\left\{\varepsilon^{-\frac{\mu(at-x)}{M}} - \varepsilon^{-\frac{\mu(at-2l+x)}{M}}\right\}$

It is to be observed that for any point x equation (3) applies from $t=\frac{x}{a}$ till $t=\frac{2l-x}{a}$, whilst (4) applies from $t=\frac{2l-x}{a}$ to $t=\frac{2l+x}{a}$.

I will not go into the question of the reflection at the mass M, but notice that when the wave is reflected at the fixed point

$$\frac{d\xi}{dx} = 2\frac{V}{a}$$

Therefore tension $=2V\sqrt{E\mu}$ or double our previous result.

We infer then, that half the velocity of impact needed to break the wire near the mass is sufficient to break it at the fixed point, but that in both cases the breaking does not depend on the mass.

These results were submitted to a rough experiment. An iron wire, No. 13 gauge, about 27 feet long, and capable of carrying $3\frac{1}{2}$cwt. dead weight, was seized in a clamp at top and bottom, the top clamp rested on beams on an upper floor, whilst the lower served to receive the impact of a falling mass. The wire was kept tort by a 56lb. weight hung below the lower clamp. The falling weight was a ball having a hole drilled in it sliding on the wire. It is clear that, although the clamp held without slipping, the blow must pass through it, and will be deadened thereby, so giving an advantage to the heavy weight. If the wire breaks some way up the wire, or at the upper clamp, it may be considered that the wire near the lower clamp stood the first onset of the blow, and hence that if the wire had been long enough it would have stood altogether.

I first tried $7\frac{1}{4}$lbs.; the wire stood the blow due to falls of 6′ and 6′ 6″ completely, but broke at the lower clamp with 7′ 0″ and 7′ 2″. We may take 6′ 9″ as the breaking height. With a 16lb. weight dropped 5′ 6″ the wire broke at the upper clamp. A 28lb. was then tried, falls of 2′ and 3′ respectively, broke it near the upper clamp; 4′ 6″ broke it three feet up the wire in a wounded place; 5′ broke it at the top clamp, and 6′ was required to break it at the lower clamp. This may be taken as a rough confirmation of the result that double the velocity is required to break it at the lower clamp to that required to cause rupture at the upper. Lastly, 41lbs. was tried, a fall of 4′ 6″ broke it at the upper clamp, of 5′ 6″ at the lower; take 5′ as height required to break at the lower.

In problems of this kind it has been usually assumed by some that two blows were equivalent when their vis vivas were equal, by others when the momenta were equal; my result is that they are equal when the velocities or heights of fall are equal.

Taking the 41lbs. dropped 5′ as a standard, since it will be least affected by the clamp, I have taken out the heights required for the other weights. Column 1, is the weight in lbs.; 2, the fall observed; 3, the fall required on vis viva theory; 4, that required by momentum theory:

(1)	(2)	(3)	(4)
41	5	5	5
28	5 " 6	7 " 4	6
16	6 " 0	12 " 11	8
$7\frac{1}{4}$	6 " 9	28 " 3	11 " 11

It will be seen that the law here arrived at is the nearest of the three, besides which its deviation is accounted for by the deadening effect of the clamp.

But it remains to be explained why the $7\frac{1}{4}$lbs. weight could not break the wire at the top at all, whereas the 28lbs. broke it with a fall of only 2 feet. We should find some means of comparing the searching effect of two blows. For this we must look to friction.

Assuming that the friction between two sections of the wire is proportional to their relative velocity, a hypothesis which accounts well for certain phenomena in sound, I worked out its effect in this case, but the result failed to account for the facts. This should not be surprising, for though this assumption may be true or nearly so for small relative velocities, it may well fail here when they are large. The discrepancy may perhaps be attributed to the fact that a strain which a wire will stand a short time, will ultimately break it, and possibly in part to want of rigidity in the supports of the upper clamp, both of which would favour the heavy weight.

I think we may conclude from the above considerations and rough experiments,

1st. That if any physical cause increase the tenacity of of wire, but increase the product of its elasticity and linear density in a more than duplicate ratio, it will render it more liable to break under a blow.

2nd. That the breaking of wire under a blow depends intimately on the length of the wire, its support, and the method of applying the blow.

3rd. That in cases such as surges on chains, etc., the effect depends more on the velocity than on the momentum or vis viva of the surge.

4th. That it is very rash to generalize from observations on the breaking of structures by a blow in one case to others even nearly allied, without carefully considering all the details.

"Observations upon the National Characteristics of Skulls," by S. M. Bradley, F.R.C.S., Lecturer on Comparative Anatomy, Royal School of Anatomy and Surgery, Manchester. Communicated by Professor H. E. Roscoe, F.R.S.

The object of this paper was to show that the classification at present in vogue, which arranges the crania of different nations into four groups, viz., 1, dolicocephalic-orthognathic; 2, dolicocephalic-prognathic; 3, brachycephalic-orthognathic; and 4, brachycephalic-prognathic, can no longer be accepted as scientifically accurate.

The measurements of Professor Retzius, who introduced this classification, were taken on a level with the glabella in front and the occipital tuberosity behind, *i.e.*, just along the line which the hat takes when placed upon the head, and it is owing to this circumstance that I have been able to take the measurements of hundreds of skulls by employing an instrument used by hatters, which gives the outline of the skull and repeats it in miniature upon a piece of cardboard. We can in a moment obtain the actual size of the skull by running a two-inch gauge completely round the miniature.

Turning to the examples before us, amongst the English skulls we find extreme specimens of dolicocephalism, or longheadedness, extreme specimens of brachycephalism, or broadheadedness, and specimens of every intermediate type, *e.g.*, one gives a cephalic index of 75, measuring 8 inches in length by 6 in breadth, while another gives a cephalic index of 88·1, measuring $7\frac{3}{4}$ inches by $6\frac{3}{4}$ inches.

In the German skulls, of which I have tracings, there is not a single example of dolicocephalism, although Retzius classes them as dolicocephalic.

Of the Danish skulls, both the examples shown are dolicocephalic.

Of the two Russian skulls, one is brachycephalic and one dolicocephalic.

The extremest type of brachycephalism is met with in a Greek skull, which measured $6\frac{3}{4}$ by $6\frac{1}{4}$ inches, giving a cephalic index of 93 or nearly so.

The evidence afforded by the Jewish skull is interesting. We have hitherto been dealing with the skulls of nations who freely intermarry with other nations, and whose skulls might in consequence be expected to vary, but this is not the case with the Jew; yet we meet with long heads and broad heads equally in this race with the others.

Another point illlustrated by these tracings is the absence of a bilateral symmetry in human skulls. Though the unsymmetry varies, it is probable that no such thing as a perfectly symmetrical human skull exists.

As to orthognathism and prognathism, it may be observed that Retzius includes amongst the orthognathi the Celtic Scotch, Irish, and Welsh. Any one who has travelled amongst these peoples would be able to confute the universal, or even general, truth of this statement. Amongst the lower Irish, indeed, prognathism is the prevailing type, and there is this further interest about the subject, that prognathism appears to be a type rapidly acquired by changed external circumstances. The conclusions arrived at are as follows:—

It is probable that when the struggle for existence was less keen than it is at present, and the human brain was in consequence less prone to rapid growth, human skulls preserved a pretty uniform type, thus, *e.g.*, all the neolothic skulls yet found are dolicocephalic, and what is also worth noting, they are of an unusually symmetrical character. It is in accordance with the doctrine of evolution to suppose that different environments (such as differences in climates, soil, mode of livelihood, *e.g.*, living by the chase or by agriculture) would produce certain and definite cranial changes: hence would arise national types of skulls, slow in arriving at such a difference as exists between the Eskimo and the Negro, and slow in changing that type when acquired. After a time the influence of civilization would come into operation, which would tend to produce varieties in the crania of a nation in accordance with the varieties of the environments of the individuals comprising the nation. A similarity of external circumstances and an absence of intermarriage would tend to produce but *one* type of skull, a difference in external circumstances and intermarriage would tend to produce a *varying* type. These factors are both at work in civilized countries. Nations whose skulls have long ago been of a well-marked distinctive character are exposed to the same environments and intermarry—the result is a confusion and mingling of the different forms.

When Retzius made his observations there is no reason to doubt that he was right in the main, but there is sufficient evidence in these tracings to show that the exceptions are so numerous as to render a classification founded on such principles valueless.

One other point is of interest. Progressive development always means greater integration and greater differentiation. The brain of the primates becomes constantly more unsymmetrical as it becomes larger. In the bosjesman, as in the chimpanzee, the convolutions are comparatively simple and symmetrical. It is, to say the least of it, not improbable, that the increasing cerebral asymmetry will produce some effect upon the bony cranium, and hence it is not fanciful to look upon this bilateral asymmetry as evidence of a higher type than would be afforded by a perfectly symmetrical skull.

Ordinary Meeting, December 12th, 1871.

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

Mr. Louis Lucas was elected an Ordinary Member of the Society.

Among the Donations announced were a series of copper plates with the late Dr. Byrom's shorthand engraved thereon, presented by Edward Byrom, Esq., of Kersall Cell.

On the motion of Dr. Roscoe, seconded by Mr. Spence, it was resolved unanimously — That the thanks of the Society be given to Mr. Byrom for his valuable Donation.

"The Illness of the Prince of Wales and its Lessons," by Edmund John Syson, L.R.C.P.E., &c.

I need make no excuse for asking a few moments for the discussion of certain matters connected with the Prince's sad illness, and, confining myself to its bearings on the general health of the nation, try, if possible, to make a great national calamity become not unbarren of much national good.

The specific illness of the Prince is what is technically termed Typhoid Fever. Until 1840, Typhus was the name under which Typhoid Fever was generally known. Dr. Alexander P. Stewart was the first to point out the distinction between Typhoid and Typhus, but not until some years afterwards did the profession at large accept this great fact. Dr. Budd of Bristol prefers the name Intestinal Fever, and certainly it is a far preferable one, for its symptoms and manifestations are essentially intestinal. For minute information as to Typhus and Typhoid and their subdivisions I must refer you to that prince of works on Medicine — Watson. Suffice it here to say that Typhus and Typhoid have each their distinctive periods of duration, rash, symptoms, and probably causation. Typhoid Fever is essentially a drain fever, and may be caused or excited by drinking impure water or inhalation of impure air. Most people hold that the specific Typhoid poison cannot be generated *de novo*. I hold most positively that it can, and not only it, but every individual kind of fever poison. Such is not the rule, but the exceptions are so numerous and well marked as to leave no doubt that certain conditions of putrefactive decay or decomposition give, as their resultant, certain definite specific fever poisons. As it may be said this is a matter for the curious rather than for the practical, I will leave it as it stands. All however agree that tainted water and tainted air may and do predispose to or excite attacks of Typhoid and other fevers, and that they are both pregnant sources of blood-poisoning. It is also agreed that "even a fractional contamination of the air of a sleeping room with sewer gas is almost certain to produce disease sooner or later."

Yet notwithstanding the universal testimony of medical men of common sense and observation that sewer gas is so fatal in its results, we have, as a sequence on our advance in domestic civilisation, so constructed our houses, our sewers, and our drains that our living rooms and the rooms in which our food is cooked, dressed, or stored, are *par excellence* the receptacles of tainted air. It is to this frightful state of things that I would call your special attention.

We have in our towns main and minor sewers. These are too often not sewers but cesspools, and if cesspools, of course generators of sewer gases. As a rule these sewers have been laid piecemeal without any reference to a definite general system. The existence of a river has had the effect of determining the direction of sewers quite independently of any sanitary considerations. All relating to the direction, &c., of sewers, ought to be decided without any reference to the existence or non-existence of a river passing through the town. Good sewers should be constructed so as to require no artificial supply of water to flush them. They should be self-cleansing. It is almost needless to say that our sewers here in Manchester and Salford do not comply with these conditions. I lay a report of the Salford Surveyor (J. Bowden, C.E.) before you. From it will be seen the condition of old Salford sewers. We are trying to remedy these. The sewers in many streets in Manchester are in like condition. I state this from personal observation. With these defective sewers our houses are directly connected by means of drains which are if possible in a worse condition. House drainage is the work of unskilled private individuals; it is done by contract. The work is generally scamped, and there is no guarantee that either the fall is sufficient or the jointing effective. In some districts unsocketed pipes are used — the authorities unwisely compelling their use. An unsocketed pipe drain must become defective. Even in clay soil they are unadvisable. In putting in drains, instead of what is technically termed "boneing," the workmen usually use a straight-edge and level, and allow each pipe $\frac{1}{4}$ or $\frac{1}{2}$ inch fall. This leads to an irregular and inconstant fall. These defective drains become attenuated cesspools, and belch forth their disease-dealing fumes into our cellars, our bathrooms, our lavatories, our closets, and our sculleries. The street grids are generally trapped artificially by dirt, and the only free openings into the sewers are in private houses. As a consequence, our heated rooms are constantly sucking in gas from the sewers. Where a rain spout does communicate with a drain it does not act as a ventilator, but rather as a down shaft.

For valuable experiments as to the futility of many accepted modes of ventilation I must refer you to Dr. Sanderson and Parke's report.

Very few scullery pipes are trapped; the same may be said of bath and lavatory pipes; and owing to defective construction water-closets all more or less leak at one oı more of their many junctions. Nurseries being generally next to bath rooms, the consequence is that our children are freely exposed to sewer gas. The scullery, the bath room, and the room next the closet, are sure to be tainted spots.

The remedy for all these evils is very simple. Of course the reconstruction of our sewers will be an expensive proceeding, but not so expensive as imperative. In reconstructing these, their size, their shape, their fall, their depth will all have to be reconsidered. A maximum depth must be established below which no house drain must be laid. As a rule sewers, main and minor, are not sufficiently get-at-able. House drains must be made capable of easy examination at definite points, and examination should be periodic. The fall should be such that their contents should never stagnate, but flow on uninterruptedly from the house to the sewer junction. All direct communication with houses should be cut off. That is, all inlets to drains should be outside houses. Household slop-water and slops should fall on to a trapped drain inlet outside the house. Even the water closet should do this. No brick-work drains should be allowed, and socketed glazed pipes should be imperative for house drains. The semi-socket I count a socket, but cannot allow the plea of ease of pulling to pieces to weigh for one moment in favour of the mischievous unsocketed pipe. In addition to these precautions all basements should be waterproof, and a really efficient system of sewer ventilation established.

I have always preferred that system urged by Mr. Peter Spence, viz., a cupola fire shaft at chosen sewer junctions. What we want is a system which shall cause the external air to turn inwards rather than outwards; rather enter the sewers than escape.

Trapping is an important point. Hitherto traps have been insisted upon more with a view to prevent solids entering the sewers than to prevent the escape of effluvia. A great number of the traps in ordinary use are of no use whatever for either purpose. If the plan of outside communication with drains were adopted there would be no necessity for any trap in any house. An efficient trap often itself becomes a great nuisance through the putrefaction which takes place in its fluid contents: without fluid no trap exists.

It is impossible to more than touch on the evils of our existing system of Towns' drainage. I know of my own knowledge that there are very few houses into which sewer gas does not permeate. From actual observation I know that our general sewage system is most defective. That is, if you agree with me that no sewer is rightly constructed which allows its contents to stagnate or solid matters to accumulate. Our house drains are many of them in a state which beggars description, and through them, and through our abominable middens, the soil on which we live is supersaturated with fœcal matter.

If health authorities are wise they will at once take steps to set their houses in order, and the only way to banish Typhoid fever from the land is by radically reforming the defects which I have pointed out.

The Prince's illness has compelled attention to these defects, and I am only sorry to see men of eminence in the scientific world urging such paltry palliative remedies as charcoal pans, instead of insisting on what will prove cheapest in the end—real radical reform of commonly admitted evils.

Mr. Henry H. Howorth remarked that he spoke without any special knowledge of the subject, and as a mere Philistine, but he thought that some elementary facts of common experience were overlooked by the gentlemen who were engaged in improving our drainage system. He was born in Lisbon, whose streets were open sewers and its atmosphere noted for its impure taint. Other Portuguese towns had the same character, as had also the towns of Italy and the Rhine. Yet in all these cases the deaths from typhoid fever did not compare unfavourably with those in English towns supposed to be decently drained and under some sanitary supervision. The moral from this seems to be that domestic sewage is not harmful unless diluted, and that the evils of typhoid fever first became critical when water closets were substituted for privies. If human excretions were allowed to decay naturally without the addition of water, as they did in the old privies and still do in continental towns in the open streets, however noisome the smell may be there is apparently little fear of fever.

He also thought that the notion of ventilating the miles of drains of a large city like Manchester by means of a few tall chimneys with fires at their bases was chimerical. There is no continuous draught in the drains, this being broken by the many grids in the streets. Now, by the ordinary laws of pneumatics it follows that if the street be cold and the house warm, there is a continuous current of tainted air passing on to the pantry and the closet from the drain, the fresh air being supplied at the open grid. The remedy that suggests itself is first to discover which classes of sewage are innocuous, and which are liable to fermentation leading to the formation of fever germs, and to separate the latter, and allow them either to dry by themselves or to apply earth or ashes so that fermentation may be prevented.

Mr. R. D. Darbishire, F.G.S., gave an account of a remarkable discovery of prehistoric relics in Ehenside or Gibb Tarn, near Braystanes Station, near St. Bees, Cumberland.

He introduced the subject by recapitulating the classification by the Danish antiquarians of the moss deposits, into (1) *Boggy levels* (Engmose), chiefly composed of, or at least with a substratum of peat, covered with water plants and grass, lying low at the bottom of valleys, and traversed by water courses; these are generally less deep than the other deposits, say 5 to 12 feet thick. (2) *Peat bogs* (Lyngmose, Svampmose), large tracts composed of long-continued growths of Sphagnum and Hypnum, kept wet from below by concealed water supply, and usually covered more or less with heather or other vegetation. The lower portions of the moss consolidate into peat. They ordinarily measure from 8 to 15 feet in depth; and (3) *Forest moss pits* (Skovmose). These are peculiar, and have proved the most interesting of such deposits. They occur in depressions in the surface of the glacial clays of the country, usually of small extent, but sometimes of considerable depth, down to 30ft. or more. They are distinguished by a marginal mass of tree stems, with branches and leaves. These trees are always found to have fallen in (towards the centre of the pit) and are often so closely packed that it would seem difficult to place more of them in the space. When the pit is large enough to admit of it the central portion is filled up with moss, and forms a small peat bog, without or with the superficial growths.

In places where time has allowed ground to consolidate and still later vegetation to find footing, the Danish pits arecommonly covered by successive growths of pine, then beech, then alder, and lastly hazel.

M. Steenstrup has calculated that to complete the development of such a deposit, of say 10 to 20 feet in thickness of peat, some 4,000 years may be required; but the period is at present conjectural only.

In the course of elaborate researches it has been ascertained that the Danish forest pits exhibit an earliest age of forests of pines (P. abies), a tree which is, except so far as recent plantations of imported trees have taken place, absolutely prehistoric in that country. That age was succeeded by degrees by an age of oaks (Q. robur, sessiliflora, Smith).

Above the oak layer appears a bed of beech trees—now the forest tree *par excellence* of Denmark. Throughout the term of these three strata, the records so to speak of successive ages of pine, oak, and beech, the poplar (*populus tremula* L.) appears, while the white birch (*betula alba* L.) lies in the lower beds, and is succeeded above by the *betula verrucosa* L. which is the form now prevalent in Denmark. In Denmark these forest pits are considered the most ancient of the three peat or moss formations. The whole of these, according to M. Steenstrup, are full of relics of bygone races of men. He states that he believes that there is not a pillar a yard square of any moss in Denmark that would not yield some specimen of ancient handiwork.

The forest pits do not at the bottom exhibit traces of human presence, but amongst the pines objects of the stone age appear, proving the great antiquity of the primitive population of Denmark. M. Steenstrup himself took stone implements from under the stems of ancient pines. Pieces of wood cut (with the help of fire) also occur.

It would seem that the age of bronze implements coincided with the oak era, and the age of iron, which falls within historic ken, with the still current period of the beech.

In the British Islands the forest pits have not hitherto been distinguished. In Ireland the peat bogs prevail over a large extent of country, and the boggy levels also occur. Each has furnished a large store of stone instruments, and occasionally objects of wood of greater or less antiquity.

In England stone implements are not unfrequently found in the low level tracts of river valleys.

The peat bogs, passing under the name of Mosses, are of comparatively small extent, and have not, perhaps from less complete observation, yielded antiquarian results of much consequence.

In the east of England a characteristic form of the peat deposits occurs in the Fens of that region. These have yielded many relics of the stone period.

In the western extremity of Cumberland, the River Ehen runs down from Ennerdale Lake, past Cleator to Egremont, and thence southerly almost parallel to the sea-coast, through which it breaks near Sellafield, along with the River Calder.

For the last three miles of its course the Ehen has cut a considerable valley, with precipitous sides, through a moss of marine deposits of clay, gravel, and sands, and in process of time has levelled the bottom for a width of a quarter to half a mile, through which it now meanders. This level tract in its lower part nearest to the sea is characteristically called the Bogholes. It is in fact a typical instance of the low level river formation above alluded to.

A precisely similar valley bottom lies in the remarkable depression which cuts off the headland of St. Bees from the higher land towards the east, running from Whitehaven southwards, past St. Bees to the sea-shore, where its water-course, called Pow Beck, debouches.

Each of these tracts when excavated shews many prostrate stems of fair sized oak trees. Bog oak is to be found in great abundance below the sands at the mouth of Pow Beck and throughout the Bogholes. Mr. D. described and shewed a cast of a polished celt of greenstone found in a drain in this latter tract, and now belonging to Dr. Clark, of Beckermet.

Between the Ehen River and the sea the marine deposits form an elevated promontory, generally pretty level, at a height of from 50 to 70 feet above the sea, known as Low-side Quarter. Above this table land are numerous isolated hillocks, rising somewhat above 100 feet in height above the sea, and many small depressions now appearing as small tarns or as peat bogs or mosses. One of the largest of these Tarns was known as Ehenside Tarn (on the ordnance map called Gibb Tarn)—an oval basin some four or five acres in extent, sheltered N., W. and S. by hills.

In 1869 Mr. John Quayle, an enterprising farmer, at Ehenside, determined to drain the tarn and make land. He dug a drain 15 feet deep from the easterly end and thence to the river, and, as the water went away, cut deep drains round and across the bottom of the lake.

The lake bottom consisted apparently of peat moss, with many trunks of trees embedded.

In 1870 the Rev. S. Pinhorn found in the heaps thrown up by the drainers stone celts and certain wooden objects shewing handiwork. Mr. Pinhorn laid by some of these, and they have since been presented by his widow to, and now form part of, the Christy collection attached to the British Museum.

The Rev. J. W. Kenworthy, having visited the spot, was struck with the locality and the objects discovered, and made an interesting communication on the subject to the *Whitehaven Herald*, in which he suggested that the discovery had been made of a real lake dwelling. Mr. Kenworthy mentioned the subject to Mr. Franks of the British Museum who proposed to prosecute the discovery in detail. Owing to the death of Mr. Pinhorn, his only means of connection with the district, his purpose was laid by until last summer when an exploration was conducted on the spot. By this time the lake bottom was exposed and superficially dry. Mr. Quayle's drains had done good work, and the material from having been so soft that a dog could not have run across it, was now solid enough to walk over.

The new research added considerably to the list of objects, most of which will soon find places in the Museum. Mr. Quayle had preserved several very interesting specimens, all of which he has been so good as to hand over for a similar deposit.

The find is a remarkable one, and appears to be, so far, unique in England, affording apparently a characteristic instance of the *forest moss-pits.* A watchful observation had failed, so far, to detect any traces of piles or platforms such as indicate what are known as Lake dwellings.

Mr. Darbishire then exhibited and described a series of celts, more or less highly finished, certain very interesting specimens of wooden hafts for celts, clubs, and paddles, a quern, and several remarkable grinding stones of different forms; and fragments of rude earthenware, found by Mr. Pinhorn, Mr. Quayle, and himself.

[The details of the locality and its exploration, and the results, were intended to appear presently in the shape of a more formal report.]

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Ordinary Meeting, December 4th, 1871.

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

Mr. R. D. DARBISHIRE, B.A., F.G.S., sent two photographs of a plant of *Cereus grandiflorus*, Mill, taken with magnesium light, on the 12th of June last. Mr. Darbishire stated that the plant was grown by the late Mr. James Darbishire, about fifty years ago, against a south wall, in a hothouse at Greenheys Hall. There it used to flower about once in three years. The largest number of flowers out at a time, that can now be recollected, was three.

In 1852 the plant was removed and replanted against a standard wire lattice, in a pine pit, at Pendyffryn, near Conway.

The removal seemed at first to have checked the growth of the plant, but it soon recovered and throve well. During several succeeding years the beautiful flowers continued to come out more and more freely, and latterly so abundantly that special record was kept of their appearance.

In 1869 the first flower opened on the night of the 29th of May, and the last on the 30th of June. The greatest number out at once was 67, on the 26th of June, forming a truly magnificent spectacle. That year there were altogether 131 flowers.

In 1870 the first bloom again appeared on the 29th of May; the last on the 4th of July. The greatest number at once was 28 on the 17th of June, the total that season 95.

In 1871 the flowering again began on the 29th of May. It continued, with little intermission, daily till the 28th of

June. The greatest numbers of flowers open at once were, on the 12th June 31, and on the 14th 21. This year 118 flowers opened perfectly.

The plant is at present a great mass of intertwining stalks with very numerous air roots, a shaggy, ugly, piece of vegetation. It measures 9 feet across, 5 feet high, and about 1½ feet thick. It shows no sign of weakness.

Cuttings taken off it grow very freely, and soon flower.

The Rev. J. E. Vize, M.A., of Forden, near Welshpool, presented the Section with a slide of *Xenodochus carbonarius*, Schl., and reported that this rare fungus occurs near Welshpool in a railway cutting, with a south westerly aspect well sheltered by a hill and a wood. The first appearance on the leaves of *Sanguisorba officinalis*, L., was noticed in the middle of May when the Lecythea-form was in perfection, but the stems and other portions of the Burnet were greatly distorted by it. A month afterwards the magnificent vermillion coloured spores were well sprinkled over the leaves, the form of which was unaltered. In the middle of July the intensely black brand spores made their appearance, many of which had twenty or more articulations, and were plentifully scattered over the leaves in tufts.

Mr. Vize stated that he had not watched the transition state from the Uredo to brand-spores, but he hoped to do so if opportunity offered.

Mr. John Barrow sent the following communication upon the results of two experiments with tar for eradicating *Tricophyton tonsurans*, in completion of the paper read at the previous meeting of the section :—

Three rings of several months standing, which had resisted applications of carbolic acid, nitric acid, and ammonia chloride of mercury—each ring being about two inches in diameter, and having at the time the raised rough edge usual in this disease, were painted over with a thick coating of tar.

In two days the tar had been partly removed by washing and wear, and was then completely removed by means of benzole. The rough edge of the rings had disappeared and could not be discovered when the finger was drawn across it. Since then the skin has gradually recovered its natural condition, and no appearance of a return has shown itself.

At the same time a fresh ring which had made its appearance on the body of another child was treated in a similar manner, and the disease disappeared with the tar in the course of a couple of days.

I am happy to say that I have no further means of continuing these experiments.

Mr. CHARLES BAILEY, in distributing some specimens of *Erica vagans*, L., from the Lizard, Cornwall, suggested that British botanists, in recording the localities on the labels of plants, should also add the province and vice-county as given in Mr. Watson's "Compendium of the Cybele Britannica."

Ordinary Meeting, December 26th, 1871.

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

Among the Donations announced was another volume of the MS. Journal of the late Mr. George Walker, presented by B. H. Green, Esq.

On the motion of Mr. W. Mellor, seconded by Dr. Joule, it was resolved unanimously — That the thanks of the Society be given to Mr. Green for his valuable Donation.

The President said that in looking over one of the MS. books of the late Mr. Walker, kindly presented to the Society by Mr. B. H. Green, he found the following remarks on Cotton and Sugar, made nearly a century ago:

On Cotton.—Kidney cotton is so called from the seeds being conglomerated or adhering firmly to each other in the pod. In all the other sorts they are separated. It is likewise called chain cotton, and I believe is the true cotton of Brazil. A single negro may with ease clean 65 lbs. in a day; it leaves the seeds unbroken and comes perfectly clean from the rollers. At the end of five months from the planting of the seeds the plant begins to blossom and put forth its beautiful yellow flowers, and in two months more the pod is formed. From the seventh to the tenth month the pods ripen in succession, when they burst in three partitions, displaying their white glossy down to the sight.

Account of cotton wool imported into Great Britain from all parts in years —

	Lbs.	Supposed Value when manufactured.
1784	11,280,338	3,950,000
1785	17,992,888	6,000,000
1786	19,151,869	6,500,000
1787	22,600,000	7,500,000

On Sugar.—The sugar in about three weeks grows tolerably dry and fair; it is then said to be cured, and the process is finished. Sugar thus obtained is called *Muscovado,* and is the raw material from which the British sugar bakers make their loaf or refined lump. There is another sort which was formerly much approved in Great Britain for domestic purposes, and was generally known by the name of Lisbon sugar; it is fair, but of a soft nature, and in the West Indies is called clayed sugar. The process is as follows. A quantity of sugar from the cooler is put into conical pots or pans, called by the French *formes,* with the points downwards, having a hole about half an inch in diameter at the bottom for the molasses to drain through, but which at first is closed with a plug. When the sugar in these pots is cool and becomes a fixed body, which is discoverable by the middle of the top falling in (usually about twelve hours from the first potting of the sugar), the plug is taken out and the pot placed over a large jar intended to receive the syrup or molasses that drains from it. In this state it is left as long as the molasses continues to drop, which it will do from twelve to fourteen hours; when a stratum of clay is spread on the sugar and moistened with water, which oozing imperceptibly through the pores of the clay, unites intimately with and dilutes the molasses, consequently more of it comes away than from sugar cured in the hogshead, and the sugar of course becomes so much the whiter and purer. A pound of sugar from a gallon of raw juice or liquor is reckoned in Jamaica a very good yielding.

The loss of weight in claying is about one third. Thus a pot of 60 lbs. is reduced to 40 lbs. But if the molasses which is drawn off in this practice be reboiled it will give near 40 per cent of sugar, so that the real loss is little more than one fourth. East India sugars being ranked among the Company's imports as manufactured goods, pays a duty of £37. 16s. 3d. per cent *ad valorem,* on sale.

The circumstance which presses with the greatest weight on the British planters in the West Indies is that branch of the monopoly which, reserving for the manufacturers of Great Britain all such improvements as the colonial produce is capable of receiving beyond its raw state, or first stage of manufacture, prohibits the colonists from refining their great staple commodity, sugar, for exportation. This is effected by a heavy duty of £4. 18s. 8d. the cwt. on all refined or loaf sugar imported, while raw or Muscovado sugar pays only 15s. the cwt. This difference operates (as it was intended) as a complete prohibition.

The quantity of raw or Muscovado sugar imported into Great Britain on an average of four years (1787 to 1790) was somewhat more than 140,000 hogsheads of 14 cwt. each at King's Beam. The drainage at sea amounted to 280,000 cwts., being in value £500,000 sterling. Such is the loss to the public. And let it be remembered that this loss is not merely contingent or possible, but plain, positive, and certain; it being undeniably true that 280,000 cwt., or 14,000 tons of sugar were sunk in the sea in the transportation of 140,000 hogsheads of the raw commodity as that this number was imported into Great Britain; and it is equally certain that every ounce of it would have been saved if the planters had been permitted to refine the commodity in the colonies. The consequent loss to the revenue is easily calculated: 64 gallons of molasses will produce 40 gallons of rum Jamaica proof.

"On the Inverse or Inductive Logical Problem," by Professor W. S. Jevons, M.A.

Logical deduction consists in ascertaining from a law or laws the combinations of qualities which may exist under those conditions. The natural law that all metals are conductors of electricity really means that in nature we may find three classes of objects, namely,

(1) Metals conductors.
(2) Not-metals conductors.
(3) Not-metals not-conductors.

It comes to the same thing if we say that it excludes the existence of the class *metals not-conductors.* But every scientific process has its inverse process. As addition is undone by subtraction, multiplication by division, differentiation by integration, so logical induction is the inverse process of deduction. Given certain classes of objects, we endeavour by induction to pass back to the laws embodied in those classes. There does not exist indeed any distinct method of induction except such as consists in inverting the processes of deduction, by noting and remembering the laws from which certain effects necessarily follow. The difficulties of induction are thus exactly analagous to those of integration.

As I have fully explained in my previous essays and papers, two terms or classes can be combined consistently with the laws of thought in four different ways. Now out of four such combinations sixteen selections (two to the power four) can be made. As each distinct law gives a different series of combinations, it follows that there could not possibly exist more than sixteen distinct forms of law governing the combinations of two classes. But in one case, where all the combinations remain, no special law applies; in other cases it can be shown that the combinations remaining are so few as to imply self-contradiction. Only six sets of combinations require further consideration. By deductive examination it is found that four of these cases correspond to varieties of the general form of law, $A = AB$, which expresses the inclusion of the class A in the class B. By the introduction of negative terms this general form may receive four essentially different logical variations. Thus we have

A part of B
A part of not-B
Not-A part of B
Not-A part of not-B.

Other apparent varieties, such as B part of not-A, will be found equivalent to one or other of the above, equivalent laws being those which lead to the same possible combinations.

The remaining two selections of combinations are found to correspond to the general form of law A=B expressing the coincidence of the classes A and B, as, for instance, the coincidence between equilateral and equiangular triangles. This form is capable of only one other logically distinct variety, that expressing the coincidence of A with the class not-B. Thus the solution of the inverse logical problem of two terms leads us to the conclusion that only two forms of relation can exist between two classes, namely, the relations of *partial* and *complete coincidence*, but these relations may exist in six different ways altogether, capable of expression in a still greater number of different propositions.

The inverse problem of three terms is a far more complex matter, since the possible combinations are eight in number, and the selections of such combinations, the eighth power of two, or 256. Many of such selections involve self-contradiction, but there appears to be no mode except exhaustive examination of ascertaining how many. By methods of inquiry fully described in the paper, it is shown that there cannot exist more than fifteen general types or forms of logical conditions governing the combinations of three classes of objects. Some of these forms of law, for instance A=ABC, expressing the inclusion of A in the class BC, are capable of as many as 24 variations; other forms of law admit 12, 8, or 6 variations. A remarkable and unique form is discovered in the proposition

A = BC or not-B not-C,

which is capable of but one other variety, namely,

A = B not-C or not-BC.

Each of these propositions can be expressed in six apparently different modes, which on examination are found to have exactly the same logical meaning.

A complete solution of the problem of three terms having been obtained, it is pointed out that the corresponding problem for four terms is almost impracticable, since it would involve the detailed examination of 65,536 different selections of combinations. The problem of five terms may be called impossible as regards complete solution, since it involves no less than 4,294,967,296 cases. Similarly, six terms admit of more than eighteen trillions of cases. Thus it is quite impossible that the complete solution of the inverse logical problem should ever be carried more than one step further than it has been done in this paper.

Ordinary Meeting, January 9th, 1872.

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

The PRESIDENT exhibited some specimens of a fossil plant resembling the *Psaronius Zeidleri* found in the Upper Foot Coal Seam, near Oldham. This species has been described by Corda, in his Beitrage Zur Flora Der Vorvelt, and figured in Plate XL., but has not hitherto, he believed, been met with in the British coal fields. The Oldham specimen appeared to him to be a petiole, of about one-eighth of an inch in diameter, and is of a nearly circular form in its transverse section, two-thirds of it consisting of a zone of strong parenchymatous tissue and an internal axis of vascular tissue arranged in four radiating arms of an irregular oval form, resembling a St. Peter's cross. As he could not connect the specimen with a stem of *Psaronius*, he proposed to call it *Stauropteris Oldhamia*.

In the above-named coal, as well as that of the Lower Brooksbottom Seam, there is a great variety of beautiful petioles which have not yet been described. Some of them evidently belong to the genus *Zygopteris*, and may probably be discovered in connection with their stems, but most of them have been found detached and sometimes mistaken for the rootlets of *Stigmaria*. From some specimens in his cabinet he is led to believe that Cotta's *Medullosa elegans* is merely the rachis of a fern or a plant allied to one. For the best specimen of *Stauropteris* he is indebted to the liberality of that intelligent collector of fossil plants, Mr. James Whitaker, of Watersheddings, near Oldham.

"On the Influence of Gas and Water Pipes in determining the Direction of a Discharge of Lightning," by HENRY WILDE, Esq.

Although the invention of the lightning conductor is one of the noblest applications of science to the wants of man, and its utility has been established in all parts of the world by the experience of more than a century, yet, a sufficient number of instances are recorded of damage done by lightning to buildings armed with conductors to produce, in the minds of some, an impression that the protective influence of lightning conductors is of but questionable value.

The destruction, by fire, of the beautiful church at Crumpsall during a thunderstorm on the morning of the 4th inst., has induced me to bring before the Society, with a view to their being known as widely as possible, some facts connected with the electric discharge which have guided me for some years in the recommendation of means by which disasters of this kind may be averted.

For the proper consideration of this subject it is necessary to make a distinction between the mechanical damage, which is the direct effect of the lightning stroke, and the damage caused indirectly by the firing of inflammable materials which happen to be in the line of discharge,

Instances of mechanical injury to buildings, not provided with conductors, are still sufficiently numerous to illustrate the terrific force of the lightning stroke, and at the same time the ignorance and indifference which prevail in some quarters with respect to the means of averting such disasters; for wherever lofty buildings are furnished with conductors from the summit to the base, and thence into the earth, damage of the mechanical kind is now happily unknown.

Even in those cases, where lightning conductors have not extended continuously through the whole height of a building, or where the lower extremity of the conductor has, from any cause, terminated abruptly at the base of the building, the severity of the stroke has been greatly mitigated, the damage being limited, in many case, to the loosening of a few stones or bricks.

The ever extending introduction of gas and water pipes into the interior of buildings armed with lightning conductors has, however, greatly altered the character of the protection which they formerly afforded, and the conviction has been long forced upon me that, while buildings so armed are effectually protected from injury of the mechanical kind, they are more subject to damage by fire.

The proximity of lightning conductors to gas and water mains, as an element of danger, has not yet, so far as I know, engaged the attention of electricians, and it was first brought under my notice at Oldham in 1861, by witnessing the effects of a lightning discharge from the end of a length of iron wire rope, which had been fixed near to the top of a tall factory chimney, for the purpose of supporting a long length of telegraph wire. The chimney was provided with a copper lightning conductor terminating in the ground in the usual manner. In close proximity to the conductor, and parallel with it, the wire rope descended, from near the top of the chimney, for a distance of 100 feet, and was finally secured to an iron bolt inserted in the chimney about 10 feet from the ground. During a thunderstorm which occurred soon after the telegraph wire was fixed, the lightning descended the wire rope, and instead of discharging itself upon the neighbouring lightning conductor, darted through the air for a distance of 16 feet to a gas meter in the cellar of an adjoining cotton warehouse, where it fused the lead pipe connections and ignited the gas. That the discharge had really passed between the end of the wire rope and the lead pipe connections, was abundantly evident from the marks made on the chimney by the fusion and volatilization of the end of the wire rope, and by the fusion of the lead pipe. As the accident occurred in the daytime, the fire was soon detected, and promptly extinguished.

Another and equally instructive instance of the inductive influence of gas pipes in determining the direction of the lightning discharge occurred in the summer of 1863 at St. Paul's Church, Kersal Moor, during divine service. To the outside of the spire and tower of this church a copper lightning conductor was fixed, the lower extremity of which was extended under the soil for a distance of about 20 feet. The lightning descended this conductor, but instead of passing into the earth by the path provided for it, struck through the side of the tower to a small gas pipe fixed to the inner wall. The point at which the lightning left the conductor was about 5 feet above the level of the ground, and the thickness of the wall pierced was about 4 feet; but beyond the fracture of one of the outer stones of the wall, and the shattering of the plaster near the gas pipe, the building sustained no injury.

That the direction of the electric discharge had, in this case, been determined by the gas pipes which passed under the floor of the church, was evident from the fact that the watches of several members of the congregation who were seated in the vicinity of the gas mains, were so strongly magnetized as to be rendered unserviceable.

The church at Crumpsall is about a mile distant from that at Kersal Moor, and the ignition of the gas by lightning, which undoubtedly caused its destruction, is not so distinctly traceable as it is in other cases which have come under my observation, because the evidences of the passage of the electric discharge have been obliterated by the fire. From information, however, communicated to me by the clerk in charge of the building, as to the arrangement of the gas pipes, the most probable course of the electric discharge was ultimately found.

The church is provided with a copper lightning conductor, which descends outside the spire and tower as far as the level of the roof. The conductor then enters a large iron down-spout, and from thence is carried into the same drain as that in which the spout discharges itself. Immediately under the roof of the nave, and against the wall, a line of iron gas pipe extended parallel with the horizontal lead gutter which conveyed the water from the roof to the iron spout in which the conductor was enclosed. This line of gas-piping, though not in use for some time previous to the fire, was in contact with the pipes connected with the meter in the vestry, where the fire originated, and was not more than three feet distant from the lead gutter on the roof. As no indications of the electric discharge having taken place through the masonry were found, as in the case of the church at Kersal Moor, it seems highly probable that the lightning left the conductor at the point where the latter entered the iron spout, and by traversing the space between the leaden gutter and the line of gas-piping in the roof, found a more easy path to the earth by the gas mains than was provided for it in the drain.

In my experiments on the electrical condition of the terrestrial globe* I have already directed attention to the powerful influence which lines of metal, extended in contact with moist ground, exercise in promoting the discharge of electric currents of comparatively low tension into the earth's substance, and also that the amount of the discharge from an electro-motor into the earth increases conjointly with the tension of the current and the length of the conductor extended in contact with the earth. It is not, therefore, surprising that atmospheric electricity, of a tension sufficient to strike through a stratum of air several hundred yards thick, should find an easier path to the earth by leaping from a lightning conductor through a few feet of air or stone to a great system of gas and water mains, extending in large towns for miles, than by the short line of metal extended in the ground which forms the usual termination of a lightning conductor.

It deserves to be noticed that in the cases of lightning discharge which I have cited, the lightning conductors acted efficiently in protecting the buildings from damage of a mechanical nature—the trifling injury to the church tower at Kersal Moor being directly attributable to the presence of the gas pipe in proximity to the conductor. Nor would there have been any danger from fire by the ignition of the gas if all the pipes used in the interior of the buildings had been made of iron or brass instead of lead; for all the cases of the ignition of gas by lightning, which have come under my observation, have been brought about by the fusion of lead pipes in the line of discharge. The substitution of brass and iron, wherever lead is used in the

* Philosophical Magazine, August, 1868.

construction of gas apparatus, would, however, be attended with great inconvenience and expense, and moreover, would not avert other dangers incident to the disruptive discharge from the conductor to the gas and water pipes within a building. I have therefore recommended that in all cases where lightning conductors are attached to buildings, fitted up with gas and water pipes, the lower extremity of the lightning conductor should be bound in good metallic contact with one or other of such pipes outside the building. By attending to this precaution the disruptive discharge between the lightning conductor and the gas and water pipes is prevented, and the fusible metal pipes in the interior of the building are placed out of the influence of the lightning discharge.

Objections have been raised by some corporations to the establishment of metallic connexion between lightning conductors and gas mains, on the ground that damage might arise from ignition and explosion. These objections are most irrational, as gas will not ignite and explode unless mixed with atmospheric air, and the passage of lightning along continuous metallic conductors, will not ignite gas even when mixed with air. Moreover, in every case of the ignition of gas by lightning, the discharge is actually transmitted along the mains, such objections notwithstanding. A grave responsibility therefore rests upon those, who, after introducing a source of danger into a building, raise obstacles to the adoption of measures for averting this danger.

Dr. JOULE remarked that, at 20 minute past 4, when the hail storm was at its height, the atmosphere was illuminated by a bright red light. This phenomenon disappeared when the fall of hail ceased.

A Paper was read entitled "Once again—the Beginning of Philosophy," by the Rev. T. P. KIRKMAN, M.A., F.R.S., Hon. Member of the Society.

Ordinary Meeting, January 23rd, 1872.

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

The President exhibited to the meeting a large crystal of Selenite, of an irregular form and eight inches in length, given to him by Mr. Taylor, of Stretford. That gentleman informed him that it was from the mud which had been dredged out of the Suez Canal. When the mud came out of the dredge there was no appearance of crystals, but on its drying and being afterwards broken up, they were found in the mass. The President said that he had noticed the formation of similar but smaller crystals of selenite in the clay taken out of the London and North Western Railway Tunnel during its formation through Primrose Hill. When the clay was first excavated there was no appearance of crystals in it, but after it had been exposed to the weather for a few months, on fracturing the clay these were found dispersed throughout its mass. He had also found crystals of selenite in the till or boulder clay at Egremont on the Mersey and at Blackpool; and the crystals, from their sharp edges, showed that they had been formed *in situ*, and had not come from a distance as many of the stones in the deposit had undoubtedly done. He had also seen in coal mines the formation of small crystals of selenite nearly an inch long in a few weeks. In this case their formation was evidently due to water charged with carbonate of lime coming into the shaft from the overlying drift beds and finding its way down into the workings, and there mixing with water containing sulphate of iron derived from decomposed iron pyrites; the sulphuric acid of the iron going to the lime and forming sulphate of lime, whilst the carbonic acid once united to it went to the iron and formed carbonate of iron. He was not acquainted with the composition of the mud dredged out of the Suez Canal, and therefore could not speak with certainty, but probably the selenite was formed by a somewhat similar double decomposition to that last described.

Mr. Brockbank, F.G.S., exhibited a specimen of mineral wool, produced at the Conshohocken Iron Works, in America, by passing a steam jet through a stream of molten slag in its flow from the blast furnace. It had a lustrous white fibre, singularly like cotton wool from the pod. It can be made at a very trifling cost, and is likely to come into use for several purposes. It is said to be a very effectual non-conductor of heat, and this has led to its being used in the United States for the coating of steam boilers and for the linings of refrigerators. Similar mineral wool is sometimes produced during the blowing in the Bessemer steel converters, but only in small quantities.

Mr. Brockbank also described a very simple mode of utilising slag, adopted at the George-Maria-Hütte Blast Furnaces, at Osnabrück, in Hanover. The molten slag is allowed to fall in a stream, from a height of about eight feet, into water, and is thus formed into large bean-shaped gravel. From the water tank it is lifted into railway trucks by "Jacob's ladders," and is conveyed away as fast as it is produced, and largely used for metalling railways.

In some of the English iron works the slag is now being broken up by Blakes' stonebreakers, and sold for metalling roads;—and in this way it proves a source of profit, instead of being a considerable loss in its usual form of huge heaps of slag, disfiguring the country.

The Bessemer slags of the Hematite furnaces are found to make excellent concrete, on account of the large quantity of lime they contain;—they are also peculiarly suitable for manuring potatoes and barley, as they fall to powder under the action of the atmosphere and yield up their silica and lime to enrich the land.

"A Study of certain Tungsten Compounds," by Professor HENRY E. ROSCOE, Ph.D., F.R.S., &c.

The constitution of the Tungsten compounds, the equivalent of the metal and even its elementary nature, are subjects upon which, for many years, serious doubts have been expressed. Thus Persoz, who at one time proposed to regard the so-called tungsten as containing two elements, at a subsequent date explained this by the assumption that the equivalent of tungsten and the formula of it highest oxide are not 184 and WO_3 respectively, but that the metal is one belonging to the arsenic group, having an atomic weight of 153, and forming a pentoxide and a pentachloride known as the tungstic compounds, together with a lower series which correspond to the lower arsenic compounds. This latter supposition, whilst unsupported by sufficient experimental evidence of its own to attract much attention from chemists, and contradicted by the important fact of the normal atomic heat of the metal corresponding to its old atomic weight, has never been satisfactorily proved to be incorrect, and has received a certain amount of corroboration from the subsequent vapour density determinations of the Chloride of Tungsten published by Debray. In this research Debray shows that the vapour density of tungstic chloride taken in mercury- and sulphur-vapours, is 168·5 (H=1), the normal density for WCl_6 (W=184) being 198·5; whereas that for Persoz's tungstic chloride, $TuCl_5$ (Tu=153), is 165, closely corresponding to the experimental density.

In order to clear up these questions a thorough investigation of the chlorides and oxychlorides of tungsten, together with the corresponding bromine and iodine compounds, appeared before all things necessary.

The author then describes the mode employed for preparing pure metallic tungsten, which was found to possess a spec. grav. of 19·261 at 12° C.

THE CHLORIDES OF TUNGSTEN.

1. *Tungsten Hexachloride,* WCl_6.

For the preparation of this chloride in the pure state it is absolutely necessary to exclude every trace of air or moisture. For this purpose the metal must be burnt in a current of perfectly dry and air-free chlorine, otherwise red oxychloride is formed, and this cannot be separated from the chloride, owing to the slight differences in their boiling points.

Metallic tungsten takes fire in chlorine at a moderate heat. On heating the tube containing the metal a granular sublimate of dark violet opaque crystals of the hexachloride makes its appearance, which, when prepared in quantity, collects as a dark blackish red liquid. In order to purify it this liquid is distilled several times in excess of chlorine, and then slowly rectified in a stream of hydrogen, by which means any traces of adhering oxychloride can be got rid of.

The dark violet coloured crystals decrepitate on cooling, and the mass falls to a crystalline powder. When pure the solid hexachloride does not undergo any change, even in moist air, but in presence of the smallest trace of oxychloride it at once absorbs moisture, evolving fumes of hydrochloric acid, and changes from a violet to a brown colour. Cold water also acts very slowly on the pure substance, but, if impure, the mass is at once decomposed by cold water into a greenish oxide. The hexachloride is readily soluble in carbon disulphide, from which it is deposited in hexagonal plates. On several occasions the tubes containing the crystalline chloride exploded on opening them with a file, the crystals suddenly assuming the form of the decrepitated substance.

On decomposition with hot water a small quantity of chlorine is invariably retained by the tungstic acid formed, even after repeated distillation with water. Hence it was necessary in the analysis to reduce the oxide to metal and to collect the hydrochloric acid formed. This was effected by covering the weighed chloride in a porcelain boat with water and bringing it into a bent combustion tube, one end of which was connected with a hydrogen evolution apparatus, and the other with a flask of water in which the acid was collected. On gently heating the fore part of the tube (the greatest care being taken to prevent spirting) the chloride is converted into the yellow oxide, after which it was more strongly heated and the reduced metallic tungsten weighed whilst the chlorine was estimated with silver.

Six analyses of different material, prepared on different occasions and according to different methods, yielded the following results:—

		Calculated.	Found.
Tungsten W	184	46·35	46·49
Chlorine Cl_6	213	53·65	53·32
	397	100·00	99·81

The exact determination of the melting point of the hexachloride is attended with some difficulty, as the liquifaction takes place gradually and the smallest traces of impurity depress the melting point down to about 180° C, that given by the older observers. A mean of several experiments gave the number 275° C (corrected) as the melting point and 270° as the point of solidification. The constant boiling point of the hexachloride was found to be 346·7° (corr.) under 759·5 mm. of mercury. The vapour density of the hexachloride was determined (1) in sulphur vapour at 440°, and (2) in mercury vapour at 350°. As the hexachloride always leaves on distillation a small quantity of solid residue, the substance was distilled (either in a current of carbonic acid or of chlorine) into the heated bulb from a smaller one attached to it, according to the method adopted by the author in the determination of the vapour density of vanadium tetrachloride. The narrow neck of the bulb was kept open during the experiment by inserting a platinum wire, and after the sulphur or the mercury had been boiling for some minutes the neck was sealed.

The results of three experiments in sulphur vapour at 440° gave the density (H=1) as (1) 167·8, (2) 169·7, (3) 168·8. Two determinations in mercury vapour at 350° gave (1) 190·7, (2) 191·2.* The fact of the alteration of the vapour density from 190 at 350° (closely approaching the normal density 198·5) to 167 at 440° shows pretty clearly that the anomalous vapour density is to be ascribed rather to dissociation than explained by Persoz's suggestion of an error in the atomic weight; and this conclusion is fully borne out by further experiments detailed in the sequel.

The residual chloride from the bulb possesses the same properties and composition as the original substance, there is no trace of free chlorine found in the cold bulb, nor does the colour of the vapour of the hexachloride change when it is strongly heated.

On heating the residue with water, a difference between its behaviour and that of the original hexachloride can however be detected, as the residue yielded an oxide which was perfectly yellow, but had a greenish colour, showing the existence of traces of oxides lower than WO_3, although present in too small quantity to affect the analysis.

In order to ascertain whether the gaseous hexachloride is decomposed at high temperatures, a portion of the pure chloride was distilled upwards in a current of dry carbonic acid for several hours. A continuous liberation of chlorine was clearly shown to occur, for, on passing the exit carbonic acid through a solution of potassium iodide considerable

* Rieth has lately determined the vapour density of "Wolfram Chlorid," showing that its molecule contains 187 instead of 184 of metal, but there is nothing to show whether the substance thus examined was the hexa- or the penta-chloride.

quantities of iodine were liberated. The residual chloride was tested for lower chlorides by titrating a weighed quantity with a standard permanganate solution, which readily oxidizes the blue oxide, formed by the action of water on the pentachloride, into tungstic acid. In one experiment thus conducted the residual chloride contained 3·3 per cent of pentachloride, whilst in another no less than 24·6 per cent of the pentachloride was formed. The pentachloride treated in a similar way yields no free chlorine, and therefore does not undergo a similar decomposition at high temperatures.

2. *Tungsten Pentachloride,* WCl_5.

On distilling the hexachloride in a current of hydrogen a reduction always takes place. If the temperature be kept but little above the boiling point of the hexachloride, the dark red colour of the vapour is seen to vanish, and a light yellow coloured vapour makes its appearance, which soon condenses into black drops or long shining black needles. After two or three distillations in hydrogen a pure product is obtained. Tungsten pentachloride crystallizes in long black shining needles; if condensed in fine powder its colour is dark green, and the powdered crystals also possess a dark green colour like that of potassium manganate. The pentachloride is exceedingly hygroscopic, the crystals becoming instantly covered with a dark golden-green film on exposure to air, and small particles being instantly converted into drops. The crystals do not decrepitate like those of the hexachloride. On treatment with larger quantities of water the pentachloride gives rise to an olive-green solution, although the greater part of the chloride forms the blue oxide and hydrochloric acid. Analyses made with three separate preparations according to the method already described, gave the following mean result:—

		Calculated.	Found.
Tungsten	$W = 184$	50·89	50·90
Chlorine	$Cl_5 = 177·5$	49·11	48·58
	361·5	100·00	99·48

Tungsten pentachloride melts completely at 248° C. and solidifies at 242°; the boiling point is 275°·6 (corr). The vapour density of this chloride taken in sulphur vapour at 440° was found to be (1) 186·4, (2) 186·5, (3) 185·7; the normal calculated density (H=1) being 180·7.

Hence the molecule of pentachloride contains one atom (W=184) of metal.

3. *Tungsten Tetrachloride* WCl_4.

The tetrachloride forms the nonvolatile residue produced in the distillation of the hexachloride in hydrogen. In order to obtain it in a pure state the mixture of the two higher chlorides is distilled at a low temperature, (best in a bath of melted sulphur,) and in a current of dry hydrogen or carbonic acid. The tetrachloride is a loose soft crystalline powder of a greyish brown colour. It is highly hygroscopic, but not so much so as the pentachloride, and it is partially decomposed by cold water into brown oxide and hydrochloric acid, forming also a greenish brown solution, which is rather more stable than the green solutions of the pentachloride in water. The tetrachloride is nonvolatile and infusible under ordinary pressure, but it is decomposed on heating into pentachloride, which distills off, and a lower dichloride which remains behind. On heating in hydrogen at a temperature above the melting point of zinc, the tetrachloride is reduced to metallic tungsten, which is sometimes deposited as a black tinder-like mass, undergoing spontaneous ignition on exposure to the air.

Analyses of four portions gave the following mean numbers :

		Calculated.	Found.
Tungsten	$W=184$	56·45	57·22
Chlorine	$Cl_4=142$	43·55	42·24
	326	100·00	99·46

4. *Tungsten Dichloride*, WCl_2.

This body is formed in light grey crusts on reducing the hexachloride at high temperatures. It can be best prepared from the tetrachloride by heating in a moderately hot zinc bath.

The Dichloride is a non-volatile loose grey powder, without lustre or crystalline structure. It undergoes change on short exposure to air, and is converted by water into brown oxide, with evolution of hydrogen. Analyses of two preparations gave as follows:

		Calculated.	Found.
Tungsten	$W = 184$	72·15	73·00
Chlorine	$Cl_2 = 71$	27·85	26·35
	255	100·00	99·35

Experiments made in the endeavour to prepare the chlorides WCl_3 and WCl were unsuccessful.

5. *Tungsten Oxychlorides.*

The Monoxychloride $WO\,Cl_4$, and the Dioxychloride $W\,O_2Cl_2$, have already been tolerably fully studied, nevertheless we find that Persoz actually doubts the existence of these well characterised compounds, and Debray, obtaining abnormal numbers for the vapour density of the first of these bodies, is unable to explain his results.

The splendid ruby red needles of the monoxychloride are best obtained by passing the vapour of a chloride over heated oxide or dioxychloride in a current of chlorine. The crystals melt at 210·4° and solidify at 206·7°; when heated more strongly the liquid boils at 227·5° C. (corrected), forming a red vapour rather lighter coloured than that of the hexachloride. On repeated distillation in chlorine over charcoal the hexachloride is formed. On exposure to air the red crystals become at once coated with a yellow crust of the dioxychloride.

Analysis gave:—

	Calculated.		Found.
Tungsten	$W = 53·80$	......	53·89
Chlorine	$Cl_4 = 41·52$	......	41·11
Oxygen	$O = 4·68$		
	100·00		

Debray found the vapour density of this body in sulphur vapour to be 148 (H=1), whereas the calculated density is 171. On repeating this determination the numbers (1) 171·3 and (2) 171·7 were obtained; whilst experiments made in mercury vapour gave (1) 175·8, (2) 170·8, proving that the vapour density of the monoxychloride is normal, and that the molecule of this substance contains 184 parts of metal.

The Dioxychloride WO_2Cl_2 is best prepared by passing chlorine over the brown dioxide. Analysis gave

	Calculated.		Found.
Tungsten	......$W = 64·32$	......	64·11
Chlorine	......$Cl_2 = 24·31$	......	24·74
Oxygen	......$O_2 = 11·37$	......	——
	100·00		

The vapour density of the dioxychloride cannot be determined at 440°, as at that temperature the contents of the bulb remains liquid.

BROMIDES OF TUNGSTEN.

Bromine vapour acts rapidly on hot metallic tungsten, forming dark bromine-like vapours which condense to a crystalline sublimate. Especial precautions require to be employed as regards exclusion of oxygen and moisture, as the oxybromide formed when these substances are present possesses very nearly the same colour as the bromide, and cannot be easily separated from the latter.

Tungsten Pentabromide WBr_5.

By the action of excess of bromine on tungsten a penta- and not a hexa-bromide is obtained. Prepared in this way the pentabromide forms dark shining crystals, having a metallic lustre not unlike that of iodine. These crystals melt at 276° and solidify at 273°, the liquid boiling at 333° (corr.) The pentabromide is at once decomposed by excess of water into the blue oxide of tungsten and hydrobromic acid, and immediately undergoes the same decomposition on exposure to moist air. On distillation, a small quantity of of a lower non-volatile bromide remains behind, and this explains the slightly too high percentage of metal found in the analysis.

			Calculated.		Found.
Tungsten ......$W = 184$		......	31·51	......	32·49
Bromine ......$Br_5 = 400$		......	68·49	......	67·74
	584	......	100·00	......	100·23

When the pentabromide is heated to 350° in a current of hydrogen a substance is obtained, which appears to correspond to WBr_3, but this is very readily decomposed, and the dibromide WBr_2 is formed as a black velvety powder. Analysis gave :

			Calculated.		Found.
Tungsten ....$W = 184$		......	53·49	......	52·03
Bromine............... $Br_2 = 160$		......	46·51	......	46·26
	344		100·00		99·29

Oxybromides of Tungsten. The monoxybromide $WO\,Br_4$ is formed together with the Dioxybromide $WO\,Br_2$ by acting on a mixture of 1 part of metal and 2 parts of tungsten dioxide with bromine. It forms shining brownish black needles, which are easily fusible, and can be separated from the dioxybromide by gentle sublimation, when the latter compound remains behind. The monoxybromide melts at 277° and boils at 327·5°, and is readily acted on by water.

The mean of four analyses gives :

			Calculated.		Found.
Tungsten ....$W = 184$		......	35·38	......	36·69
Bromine ....$Br_4 = 320$		......	61·54	......	61·04
Oxygen ....$O = 16$		......	3·08	......	
	520		100·00		

The dioxybromide WO_2Br_2 is formed as light reddish brown vapours, which condense to reddish brown coloured crystals by passing the vapour of the pentabromide over tungsten trioxide. The crystals do not melt, but volatilize at a temperature near to a red heat, and they are not acted on by water.

Analysis of four samples gave :

		Calculated.	Found.
Tungsten	W = 184	48·94	49·18
Bromine	Br_2 = 160	42·55	42·05
Oxygen	O_2 = 32	8·51	
	376	100·00	

IODIDE OF TUNGSTEN, $W I_2$.

On passing iodine vapour together with carbonic acid over metallic tungsten heated to redness a very small quantity of soft scaly crystals having a greenish metallic lustre is found to sublime. The same substance is formed (but also in small quantities) when iodine vapour is passed over the heated brown oxide or a mixture of metal and oxide. The product was analyzed by passing air over the heated iodide when it is ready converted into tungstic acid, iodine being liberated. The iodide is infusible and cannot be redistilled without decomposition and it is not immediately acted on by water.

Analysis gave:

		Calculated.	Found.
Tungsten	W = 184	42·01	42·95
Iodine	I_2 = 254	57·99	56·64
	438	100·00	99·59

ATOMIC WEIGHT OF TUNGSTEN.

1. By reduction of Tungsten Trioxide.

The difficulty of obtaining perfectly pure tungstic acid and the effect which impurity produces on the atomic weight determinations has been pointed out by Dumas. In order to avoid the danger to which all the former determinations are subject, consequent upon the partial reduction of the acid to green oxide which cannot again be oxidised, and the production of which seems to be caused by presence of traces of alkali, the tungstic acid used was prepared by decomposing oxychloride with water and drying and igniting in platinum (contact with glass reduces some WO_3). The loss of weight on reduction in hydrogen and gain of weight on oxidation was several times repeated. The oxide was placed in a porcelain boat being heated in a porcelain tube, and reduced in hydrogen and oxidised in a current of air. After each reduction the boat was found to be partially coated inside with a thin black film having a metallic appearance which oxidised completely when heated in air. A second boat was placed in the tube beyond that containing the substance for the purpose of ascertaining whether any metal was volatilized, but this boat was not found to become the least discoloured. The results of the determinations were as follows :—

1.	Original weight of Oxide.........	7·8840	grams.
2.	Oxide after 1st Oxidation	7·8806	,,
3.	———— 2nd ————	7·8792	,,
4.	Weight of Metal, 1st reduction.	6·2438	,,
5.	———————— 2nd ————	6·2481	,,
6.	———————— 3rd ————	6·2488	,,

It is evident from these numbers that the 2nd and 3rd weights of oxide and the 2nd and 3rd weights of metal are the only ones which can be relied on as being perfectly pure. Taking the mean of these two series, we have 7·8799 grams of oxide, giving 6·24845 grams of metal, or 79·296 per cent. This corresponds to the atomic weight 183·84. In order to have obtained the number 184·00 the weight 7·8799 grams of oxide must have yielded 6·24960 grams of metal, differing by 0·00115 grams from the experimental number.

2. By Analysis of the Hexachloride.

Perfectly pure hexachloride was prepared from the pure metal (itself obtained from oxychloride). No traces of oxychloride could be detected in the hexachloride employed, and it yielded a perfectly canary yellow trioxide on treatment with water, showing absence of any pentachloride. In the determination of the chlorine, the substance was weighed in the piece of drawn-out combustion tubing, in which it was afterwards reduced in hydrogen, the hydrochloric acid being collected and estimated as silver salt. The determination of metal was made in a porcelain boat in which the weighed hexachloride was first carefully converted into trioxide by exposure for two days to a moist atmosphere, and afterwards reduced in hydrogen. Analysis gave :—

		Grams.
(1)	Weight of Tungsten hexachloride taken	19·5700
	,, Chlorine found	10·4901
	Percentage of Chlorine	53·605
(2)	Weight of Chloride taken	10·4326
	,, Metal obtained	4·8374
	Percentage of Metal	46·368

Hence the atomic weight of the metal is 184·25; or, taking the mean of the two methods, we have 184·04 as the atomic weight of tungsten.

The author wishes to express his thanks to Mr. H. Rocholl who has ably aided him in the above research.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 15th, 1872.

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

A paper was read on *Nemosoma elongata* by JOSEPH SIDEBOTHAM, F.R.A.S.

The Author having discovered a considerable number of specimens of this very rare species under bark of elm, at Beeston, Notts., in November last, and having the opportunity, carefully observed its habits, of which he gave a detailed account, illustrated by specimens and by portions of bark and diagrams; showing also specimens and drawings of *Hylesinus vittatus*, on which it is parasitic.

Mr. THOMAS COWARD exhibited some tropical species of Compositæ having some curious superficial resemblances to species of widely separated orders.

Ordinary Meeting, February 6th, 1872.

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

Mr. Sidney Jewsbury was elected an ordinary Member of the Society.

Dr. JOULE, F.R.S., called attention to the very extraordinary magnetic disturbances on the afternoon of the 4th instant, and from which he anticipated the aurora which afterwards took place. The horizontally suspended needle was pretty steady in the forenoon of that day, but about 4 p.m. the north end was deflected strongly to the east of the magnetic meridian, and afterwards still more strongly to the west. The following were the observations he had made:—

Time.	Deflection from the Magnetic Meridian. °	′		Time.	Deflection from the Magnetic Meridian. °	′	
4-0 p.m.	0	50	E.	6-10 p.m.	1	24	W.
4-30 ,,	0	47	W.	6-12 ,,	1	8	,,
4-55 ,,	2	22	,,	7-41 ,,	0	10	,,
4-58 ,,	3	0	,,	7-43 ,,	0	0	,,
5-9 ,,	3	45	,,	8-9 ,,	0	42	,,
5-12 ,,	0	52	,,	8-31 ,,	0	10	,,
5-23 ,,	5	36	,,	8-54 ,,	1	18	,,
5-24 ,,	2	28	,,	8-58 ,,	0	52	,,
5-35 ,,	0	52	,,	11-3 ,,	0	5	,,
5-55 ,,	0	52	,,				

Mr. Sidebotham states that he also expected the magnificent aurora on account of the violent disturbance of the needle at Bowdon, amounting to at least 3°. Observation with the spectroscope by Dr. Joule showed a bright and almost colourless line near the yellow part of the spectrum. This line appeared to whatever part of the heavens the instrument was directed, and could be plainly seen when the sky was covered with clouds and rain was falling. When looking at the most brilliant red light of the aurora a faint red light was seen at the red end of the spectrum, and beyond the bright white line towards the violet end two broad bands of faint white light.

Mr. THOMAS HARRISON stated that he saw the aurora on last Sunday evening from $6^h\ 15^m$ to $9^h\ 30^m$ and took spectroscopic observations thereon from various parts of the sky. In each case, however, he discovered only one bright yellow line, situated between D and E, being on Kirchoff's scale about 1255 to 1260. He is not acquainted with any known substance that gives a corresponding line. The line throughout was very clear and decided both in the narrow and wide slit; but he failed to discover any continuous spectrum. The line was also very perceptible by reflection from those parts of the sky in which no trace of aurora was visible; and although the streaks were both red and white, the spectroscope appeared to give the aurora as a monochromatic light.

"Note on the Destruction of St. Mary's Church, Crumpsall, on the 4th January, 1872, by Fire from a Lightning Discharge," by JOSEPH BAXENDELL, F.R.A.S.

The interest taken in the question as to the cause of the recent accident by lightning to St. Mary's Church, Crumpsall, induces me to submit to the Society the following results of a careful examination of the lightning conductor, spouts, gas piping, &c., at the church and rectory, which I made on the 27th ultimo.

The lower part of the conductor passes through an iron down-spout, and terminates in a common drain-pipe at a distance of only 3 feet 9 inches from the lower end of the spout, and at a depth of only about 18 inches below the surface of the ground. It has therefore no direct connection with the earth, and is in consequence absolutely useless for the purpose for which it was intended. The iron down-spout through which the conductor passes received the end of a lead gutter, which extended the whole length of the church to the top of a similar iron down-spout built in the wall inside the rectory, and connected with another iron spout outside the wall by a leaden bend pipe. This leaden bend was above the floor of the vestry, and at a distance of 18 inches from it, and below the floor, there was a lead gas pipe connected with the large gas meter, which received its supply from a main laid in the street leading to the rectory. There was a small meter under the tower, but no part of the piping connected with it approached the conductor, the spouts, or the lead gutter, within a less distance than 3 feet.

Assuming, then, that the lightning struck the top of the conductor, its course would be through the lead gutter to the iron down-spout in the vestry, and then by a disruptive discharge from the lead bend to the lead gas pipe under the floor of the vestry and through the meter to the street main. The lead gas pipe would be melted and the gas ignited, and it is very probable that the disruptive discharge from the lead bend would also ignite any inflammable materials that might be in that corner of the vestry.

When the discharge arrived at the gas main in the street, part of it would pass down the main in a westerly direction and part up the main to the supply pipe and meter at the rectory. Here a small lead pipe passed from the meter for a short distance along the ceiling of the cellar, and in its course came in contact with an iron water supply pipe; the discharge melted part of the small lead pipe, ignited the gas, and finally passed off through the water supply pipe into the main in the street.

I have assumed that the lightning struck the top of the conductor, but I must state that I was unable to discover the slightest trace of any action tending to support this view; and it is at least equally probable that the stroke fell directly on the top of the iron down-spout at the east end of the church. It is stated that the bell in the tower was heard to ring at the time of the discharge; but the mere passage of the electric fluid down the conductor would not affect the bell, and the concussion of the air from a discharge on the top of the conductor would act upon the tower in a vertical direction, and would not, therefore, be likely to give the bell a swinging movement. If, however, the discharge was directly on the spout at the east end of the church, then the concussion of the air would act laterally upon the tower in an east and west direction, and, as the bell swings on an axis lying north and south, it is quite conceivable that an oscillating movement might be given to it sufficient to cause it to ring. In either case, however, whether the discharge took place upon the top of the conductor or on the top of the down-spout in the vestry, the ultimate results would be precisely the same. Had the conductor been directly connected with the gas main, as suggested by Mr. Wilde, the accident to the church would have been prevented, but not that at the rectory. The practical conclusion, therefore, to be drawn from a consideration of all the circumstances of this disastrous occurrence is that, in towns and districts where systems of gas and water mains and pipes exist, all lightning conductors should be directly connected with the mains of both systems. Had this been done at St. Mary's Church no accident would have occurred either to the church or the rectory.

Mr. Boyd Dawkins, F.R.S., called the attention of the Society to a remarkable group of crystals of calcite and sulphide of iron surrounding stalactitic bitumen, found at Castleton in Derbyshire, by Rooke Pennington, Esq. The mode of formation was this. When the mountain limestone of that district became charged with bitumen, the latter penetrated into a cavity which it traversed in long stalactite drops. Subsequently the cavity was more or less filled with crystals of calcite and sulphide of iron, which were deposited by the water charged with those substances around the drops of bitumen. The heat by which the bitumen found its way into the rocks must have disappeared before the crystals were formed; for had the latter been the result of hydrothermal action, they may have been coated, but certainly could not have been traversed by the solid bituminous stalactites.

"On the Boiling Points of the normal Paraffins and some of their Derivatives," by C. SCHORLEMMER, F.R.S.

It is generally asserted that the boiling points of the members of homologous series increase regularly for each increase of CH_2. Thus it is stated that in the series of the alcohols and fatty acids the boiling point is raised 19° for each addition of CH_2, whilst in other series this difference is sometimes smaller, sometimes larger, but always the same in the same series. But in many cases the boiling points calculated by this rule do not agree at all with those which have been observed. One reason for this discrepancy is that the compounds of which the boiling points have been compared are not true homologous bodies, *i.e.* that they have not an analogous constitution although they differ in the composition by CH_2 or a multiple thereof. During the last year, however, we have become acquainted with some true homologous series, namely, the series of the normal paraffins and the normal alcohols and their derivatives.

In a paper read before the Royal Society I have already pointed out that the difference between the boiling points of the lower members of these paraffins is not the same, but that it decreases regularly by 4° until it becomes the well known difference of 19°, as the following table will show —

	Boiling-points. Found (mean).	Calculated.	Difference.
$C\ H_4$	——	——	
$C_2\ H_6$	——	——	
$C_3\ H_8$	——	——	
$C_4\ H_{10}$	1°	1°	
$C_5\ H_{12}$	38	38	37°
$C_6\ H_{14}$	70	71	33
$C_7\ H_{16}$	99	100	29
$C_8\ H_{18}$	124	125	25
$C_{12}H_{26}$	202	201	4 × 19
$C_{16}H_{34}$	278	278	4 × 19

It appeared to me of interest to compare the boiling points of other normal compounds, selecting of course those only of which the boiling points have been carefully determined and corrected for pressure and expansion of the mercurial column of the thermometer above the vapour. The result of this investigation is that in most of the other series the difference between the boiling points also steadily decreases by about 2°; but I am not in a position to state whether this decrease ceases when the difference becomes 19°, as we do not yet know a sufficient number of compounds.

(1) Normal Iodides.

		Boiling-points. Observed.	Calculated.	Difference.
Methyl	$C\ H_3\ I$	40°	40°	
Ethyl	$C_2H_5\ I$	72	72	32°
Propyl	$C_3H_7\ I$	102	102	30
Butyl	$C_4H_9\ I$	129·6	130	28
Pentyl	$C_5H_{11}I$	155·4	156	26
Hexyl	$C_6H_{13}I$	179·5	180	24
Heptyl	$C_7H_{15}I$	—	202	22
Octyl	$C_8H_{17}I$	221	222	20

NORMAL BROMIDES.

		Observed.	Calculated.	Difference.
Ethyl	C_2H_5Br	39°	39°	
Propyl	C_3H_7Br	71	71	32°
Butyl	C_4H_9Br	100·4	101	30
Pentyl	$C_5H_{11}Br$	128·7	129	28
Hexyl	$C_6H_{13}Br$	—	155	26
Heptyl	$C_7H_{15}Br$	—	179	24
Octyl	$C_8H_{17}Br$	199	201	22

NORMAL CHLORIDES.

		Observed.	Calculated.	Difference.
Ethyl	C_2H_5Cl	12·5°	13°	
Propyl	C_3H_7Cl	46·4	46	33°
Butyl	C_4H_9Cl	77·6	77	31
Pentyl	$C_5H_{11}Cl$	105·6	106	29
Hexyl	$C_6H_{13}Cl$	—	133	27
Heptyl	$C_7H_{15}Cl$	—	158	25
Octyl	$C_8H_{17}Cl$	180	181	23

NORMAL ACETATES.

		Observed	Calculated	Difference.
Ethyl	$C_4H_8O_2$	74°	74°	
Propyl	$C_5H_{10}O_2$	102	101	27
Butyl	$C_6H_{12}O_2$	125·1	126	25
Pentyl	$C_7H_{14}O_2$	148·4	149	23
Hexyl	$C_8H_{16}O_2$	168·7	170	21
Heptyl	$C_9H_{18}O_2$	—	189	19
Octyl	$C_{10}H_{20}O_2$	207	208	19

Whilst in these series the difference between the boiling points steadily diminishes, in the series of the normal alcohols the difference appears to remain the same, being about 19°.

NORMAL ALCOHOLS.

		Observed.	Calculated.
Ethyl	C_2H_6O	78·4°	78·4°
Propyl	C_3H_8O	97	97
Butyl	$C_4H_{10}O$	116	116
Pentyl	$C_5H_{12}O$	137	135
Hexyl	$C_6H_{14}O$	156·6	154
Heptyl	$C_7H_{16}O$	—	173
Octyl	$C_8H_{18}O$	192	192

In the series of the normal fatty acids the difference between the boiling points of the lower members is also constant, being 22°, but afterwards it becomes less.

NORMAL FATTY ACIDS.

		Observed.	Calculated.	Difference.
Acetic	$C_2H_4O_2$	118°	118°	
Propionic	$C_3H_6O_2$	140·6	140	22°
Butyric	$C_4H_8O_2$	163·2	162	22
Pentylic	$C_5H_{10}O_2$	184·5	184	22
Hexylic	$C_6H_{12}O_2$	204·5	206	22
Heptylic	$C_7H_{14}O_2$	220		
Octylic	$C_8H_{16}O_2$	233		
Nonylic	$C_9H_{18}O_2$	254		

Ordinary Meeting, February 20th, 1872.

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

The PRESIDENT said that at the meeting of the Society on the 9th of January last he alluded to the probability of the genus *Zygopteris* being found in the limestone nodules of the Foot Mine near Oldham. He had lately had an opportunity of inspecting the collection of Mr. James Whitaker of Watershedding, and he there recognised a specimen of the *Zygopteris Lacattii* of M. Regnalt. There was a difference between the Autun and Oldham specimens; for whilst the vascular bundles in the petiole of the former were shaped like a double anchor, in the latter they came nearly together and formed a circle; but he thought this difference scarcely sufficient to form another species.

Dr. J. P. JOULE, F.R.S., described some experiments he had been making on the polarization by frictional electricity of platina plates, either immersed in water or rolled together with wet silk intervening. The charge was only diminished one half after an interval of an hour and a quarter. It was ascertained both in quality and quantity by transmitting it through a delicate galvanometer. He suggested that a condenser on this principle might be useful for the observation of atmospheric electricity.

"On an Electrical Corona resembling the Solar Corona," by Professor OSBORNE REYNOLDS, M.A.

The object of this paper is to point out a very remarkable resemblance between a certain electrical phenomenon (which may have been produced before, although I am not aware that it has) and the solar corona. This resemblance seems to me to be of great importance, for the striking features of these two coronæ are not possessed by any other halos, coronæ, or glories with which bright objects are seen to be surrounded.

Until the eclipse of 1871 there was considerable doubt how far the accounts given by observers of the corona could be relied upon; but Mr. Brothers' photograph has left no doubt on the subject. In this photograph we have a lasting picture of what hitherto has only been seen by a few favoured philosophers, and by them only during a few moments of excitement.

This picture shows the beautiful radial structure of the corona, the dark rifts which intersect it, and also shows the disc of the moon, clear and free from light. I have not yet seen any of the photographs of the last eclipse, but I hear there are several, and that they show the radial structure and rifts even more distinctly than this one does, but whether they do or not one photograph is positive evidence; the absence of more simply means nothing.

The features to which I refer as those which distinguish the solar corona are—

1. Its rifts and general radiating appearance.
2. The crossing and bending of rays.
3. Its self-luminosity shown by the spectroscopic observations of Professor Young.

4. The way in which its appearance changes and flickers.

When taken in connection with the blackness of the moon's disc, which shows that the corona does not exist or owe its existence to matter between the moon and the plate on which the photograph is taken, these features show that we see on the card the picture of something which actually existed in the neighbourhood of the sun; that the bright rays which we see photographed were actually bright rays of light-giving matter, standing out from the sun an enormous distance. The rifts and general irregularity of the picture show that these rays do not come out uniformly all over the sun's surface, but that they are partial and local, in some places thinly distributed and in others absent altogether. The rays are not all of them straight or perpendicular to the sun's surface.

Such bright rays as these cannot be the result of the sun's light or heat acting on an atmosphere or matter circulating in the form of meteorites. If they are due to the action of the sun's light or heat at all it must be acting on matter distributed in the rays we see, for the sun's light and heat coming out uniformly all round would illuminate any surrounding matter, if at all, so as to show its figure.

The picture irresistibly calls up the idea of a radial emission. If it is the picture of distributed matter, that matter must exist in the form of streams leaving the sun; if it is the picture of some light-producing action, this also must exist in the form of an emission from the sun.

Such then are the extraordinary features of the solar corona, and as I stated, they resemble those of an electrical corona. Any one who is familar with the various forms of electrical disruptive discharge will recognise the general resemblance they bear to an electric brush. But to the electric phenomenon I am about to describe it is no mere general resemblance, it is an actual likeness with every feature identical.

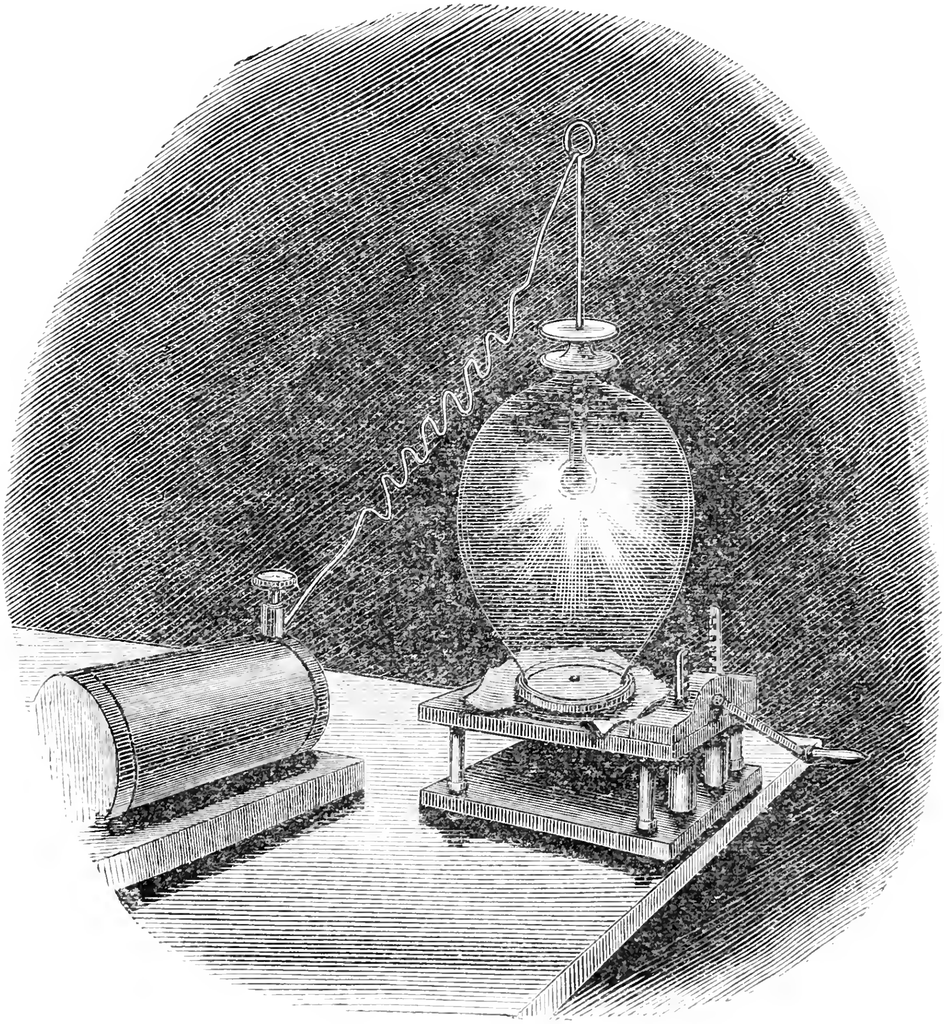
Before describing the phenomenon I may be allowed to state how I came to notice it. It will be remembered that in a former communication to this Society I ascribed the phenomena of comets and the corona to a certain electrical condition of the sun. Well, the peculiar appearance of Mr. Brothers' photograph induced me to try if a brass ball, brought into the condition I had ascribed to the sun, would give off a corona presenting this appearance.

The phenomenon is produced by discharging electricity from a brass ball in a partially exhausted receiver. To do this there is no second pole used, the objects which surround the outside of the glass probably answering to this purpose. In order to produce the desired appearance a certain relation is necessary between the pressure of the air and the intensity of the discharge. It is produced best when the receiver is a glass globe insulated on a glass stand, the ball being supported in its middle by a rod coated with indiarubber, to prevent its discharging and spoiling the effect. It is only negative electricity that is discharged into the globe. What becomes of this electricity is not clear; when a machine is used it probably distributes itself on the inside of the glass, and induces a corresponding charge on the outside. When the coil is used it must escape back for I have had it going for hours without any variation.

There is great difficulty even when the apparatus is right in producing the corona; using a large coil I just exhausted the receiver till the pressure was equal to half an inch of mercury, then there was no appearance of a corona, but one more resembling what is seen in a Geissler tube, I then let the air in gradually, and as the pressure rose the appearance changed at first to a most extraordinary mass of bright serpents twining and untwining in a knot round the ball, then to the branches of an oak tree, and as the pressure kept increasing I gradually observed amongst the branches a faint corona which I saw at once was what I was looking for, it was formed of pencils of light, forming a light radiating envelope round the ball, diminishing in brightness as it receded from the ball, the tree gradually died out until there was nothing left but the bright radiating envelope, out of which a bright ray would occasionally flash. The diameter of this envelope was about three or four times that of the ball. It was not steady but flickered so that it would appear to turn round; it consisted of pencils, or, as they are termed, bundles of rays, between which there were dark gaps. These gaps moved round about the ball; subsequently, however, by sticking sealing-wax on the ball, I rendered them definite and permanent. As the pressure of air increased, the brush became fitful, and finally ceased altogether. It was best when there was about 4 inches of mercury in the gauge. By varying the action of the coil I could do with different pressures of air, and hence I assume that there is a definite relation between the intensity of the charge in the ball and the pressure of the air surrounding it under which the phenomena can occur.

The appearance is very faint; so faint that it is difficult to see it even when close to the ball, and I find that it takes some time before the eye can fully appreciate its beauty. It was unfortunately so faint that Mr. Brothers was unable to photograph it. The plate was exposed ten minutes, but there was not the slightest trace of anything on it.

The adjoining cut represents the apparatus employed, except that the receiver was replaced by the globe described above. The light round the ball gives a fair idea of the momentary appearance, and it is impossible to represent more, as this flickers and changes so rapidly.



This corona when compared with the solar corona has the special features—

1. The rifts and general radial appearance, including the bending and crossing of rays.

2. The self-luminosity.

3. The changefulness and flickering.

There is one point in which it differs from the solar corona, but this is no more than must be expected. The shading off of the light in the solar corona is much more rapid than that in its electrical analogue. If however the pressure of the air could be caused to vary so that it was denser round the ball, even this difference could be done away with.

In this experiment, then, we have actually produced all the very features which are so extraordinary in the larger phenomenon, and were there no other evidence than this that the solar corona may be electrical, it seems to me that this resemblance constitutes very strong proof. When two things existing at different times, or in different places, resemble each other perfectly, and resemble nothing else in the range of our knowledge, surely that is high probability that they are similar.

We may, however, expect, if the sun is electrical, to find some direct indications of its electricity; nor are such wanting. They are increasing every day. There is the sun's effect on the electricity of the earth's atmosphere, its magnetism, and the aurora; the connection between the sun spots and the earth, and the connection between the planets and the sun spots, as shown by M. De la Rue and Dr. Balfour Stewart. It must be admitted that there are evident signs of some influence which the planetary bodies have on the sun and it on them; which is not gravity nor the result of gravity, yet the result of heat. Almost all these signs are of an electrical character, and some are electricity itself. Moreover, electricity or electric induction is the only other action at a distance besides gravity and heat that takes place, Is it not, then, probable that this influence is electrical? Are we to reject an hypothesis which explains some of these phenomena, and may explain all, simply because we do not see any cause for the electrical condition of the sun—why the sun should be charged with negative electricity?

Should we have discovered that the sun was hot if we had waited to find out why it was hot. Surely it is sufficient to say that there is no proof that it is not electrical. We may go further than this, for if we may compare large bodies with small, then we may show a possible reason for its being electrified. When two particles of different metals approach or recede from each other they become electrified with opposite electricities. May not the sun be approaching or leaving some other body of a different material. I do not suggest this as a probable explanation, but simply in answer to those who say that it is absurd to suppose the sun can be electrified.

"On the Electro-Dynamic effect, the induction of Statical Electricity causes in a moving body. The induction of the Sun a probable cause of Terrestrial Magnetism," by Professor OSBORNE REYNOLDS, M.A.

If an electrified body was placed near a moving conductor so as to induce an opposite charge in the moving body, this charge would move on the surface of the conductor so as to remain opposite the electrified body, whatever the motion might be. Suppose the moving conductor to be an endless metal band running past a body negatively charged, the positive charge would be on the surface of the band opposite to the negative body, and here it would remain whatever might be the velocity of the band. Now the effect of the motion of this negative electricity on the conductor would be the same as that of an electric current in the opposite direction to the motion of the band.

If instead of a band the moving body consisted of a steel or iron top spinning near the charged body the effect of the electricity on the top would be the same as that of a current round it in the opposite direction to that in which it was spinning.

It might be that the electricity in the inducing body would produce an opposite magnetic effect on the top; but even if this were so (and I do not think it has been experimentally shown that it would be so), its effect, owing to its distance, would be much less than that of the electricity on the very surface of the top. If we take no account of the effect of the inducing body the current round the top would be of such strength that it would carry all the electricity induced in the top once round every revolution. And if the top were spinning from west to east by south it would be rendered magnetic with the positive pole uppermost, that is, the pole corresponding to the north pole in the earth or the south pole of the needle.

In order to show that such a current might be produced, a glass cylinder, twelve inches long and four across, was covered with strips of tinfoil, parallel to the axis, with very small intervals between them. These strips were about six inches long and one half inch wide, and the intervals between them the two-hundreth of an inch. In one place there was a wider interval, and from the strips adjacent to this wires were connected by means of a commutator with the wires of a very delicate galvometer. This cylinder was mounted so that it could be turned twelve hundred revolutions in a minute, and brought near the conductor of an electrical machine. This apparatus, after it had been thoroughly tested, was found to give very decided results. As much as 20° deflection was obtained in the needle, and the direction of this deflection depended on the direction in which the cylinder was turned, and on the nature of the charge in the conductor. When this was negative the current was in the opposite direction to that of rotation. It may be objected that the measurement was not actually made on the cylinder. It must, however, be remembered that it was made in the circuit of metal round the cylinder, and that my object was to find the relative motion of the cylinder and the electricity. Altogether I think it may be taken as experimental proof of the fact previously stated that if a steel top were spinning under the inductive influence of a body charged with negative electricity the effect would be that of a current round the top such as would render it magnetic.

The cause of terrestrial magnetism has not been the subject of so much speculation as many much less important phenomena. It seems to have been regarded as part of the original nature of things like gravity, and the heat of the sun, as a cause from which other phenomena might result, but not as itself the result of other causes.

Yet, when we come to think of it, it has none of the characteristics of a fundamental fact; it appears intimately connected with other things, and when two phenomena have a relation to each other, there is good reason for believing them to be connected, either as parent and child, or else as brother and sister, the one to be derived from the other, or else them both to spring from the same cause.

Now the direction of the earth's magnetism bears a marked relation to the earth's figure, and yet it can have had no hand in giving the earth its shape, which is fully explained as the result of other causes; therefore, we must assume that the figure of the earth has something to do with its magnetism, or what is more likely, that the rotation which causes the earth to keep its shape, also causes it to be magnetic.

If this is the case then there must be some influence at work with which we are as yet unacquainted—some cause which coupled with the rotation of the earth, results in magnetism. From the influence which the sun exerts on this magnetism we are at once led to associate it with the cause. Yet the cause itself cannot be the result of either the sun's heat, light, or attraction. What other influence then can the sun exert on the earth ?

The analogy between the magnetism produced in a spinning top by the inductive action of a distant body charged with electricity, and the magnetism in the rotating earth, probably caused by the influence of the sun, which influence is not its mass or heat, seems to me to suggest what the influence which the sun exerts is. If the sun were charged with negative electricity, it seems to me to follow, from what the experiments I have described establish, that its inductive effect on the earth would be to render it magnetic, the poles being as they are.

The only other way in which the sun could act to produce or influence terrestrial magnetism would be by its own magnetism. If the sun is a magnet, it would magnetise the earth. If this is the cause the sun's poles must be opposite to those of the earth. Now, it follows that such a condition of magnetism would or might, if its materials are magnetic, be caused by the rotation of the sun under the inductive action of the earth and planets in exactly the same way as that caused in the earth by the inductive action of the sun. As the direction of rotation is the same in both bodies, and the electricities of the opposite kind, the magnetism would be of the opposite kind also. So that on this hypothesis it is probable the sun would act by both causes.

When I first worked out this idea, I was not aware that anything like it had been suggested before; but Mr. Baxendell, after having seen my experiments, noticed a review of a book on terrestrial magnetism, to which he kindly called my attention. The author, without making any assumption with regard to the electrical condition of the sun, assumes it to act on the earth's magnetism precisely as it would under the conditions I have described; and he then proceeds to consider, not only the general features of the earth's magnetism, but all its details—and this in a most elaborate manner—and to show the explanation which this hypothesis offers for them, particularly for the secular variation of the direction of the needle. I am, therefore, able to speak of the hypothesis as affording an explanation of the numberless variations of the earth's magnetism, as well as of its general features.

Ordinary Meeting, March 5th, 1872.

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

"On Changes in the Distribution of Barometric Pressure, Temperature, and Rainfall under different Winds during a Solar Spot Period, by Joseph Baxendell, F.R.A.S.

In my paper "On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface;" read March 8, 1864, I endeavoured to account for some of the phenomena therein described by assuming that variations in the magnetic condition of the earth produce corresponding variations in the direction and velocity of the great currents of the atmosphere; and some time afterwards in considering this hypothesis more carefully it appeared to me that if, as is generally supposed, magnetic changes are intimately connected with the disturbances which take place in the solar photosphere, their influence upon the atmosphere ought to be indicated by variations in the distribution of barometric pressure, temperature, and rainfall under different winds corresponding to the variations of solar spot frequency. Fortunately the means of at once testing the soundness of this view were at hand in the valuable tables numbered XVI. and XVIII. in the volumes of the "Radcliffe Observations," which show for each year the relations between barometric pressure, temperature, and rainfall under different winds at Oxford. I therefore extracted from these tables, and arranged in order, the mean annual barometric pressures, mean temperatures, and amounts of rainfall under different winds for the ten years 1858-67, and on carefully examining the table thus formed I found that changes had taken place in the three elements which corresponded very closely in the times of their maxima and minima with those of solar spot frequency.

The mean length of a solar spot period is about 11 years and 5 weeks, and as the volume of "Radcliffe Observations" for 1868 has been published since I formed the ten years table, I have included the mean results for that year in the following table, which thus represents the changes which took place through a complete solar spot period.

BAROMETRIC PRESSURE, TEMPERATURE, AND RAINFALL, UNDER DIFFERENT WINDS, AT OXFORD, DURING THE YEARS 1858—68.

	N.			N.E.			E.			S.E.			S.			S.W.			W.			N.W.		
	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.	Mean Barom. Pressure.	Mean Tempera-ture.	Total Rainfall.
	In. 29+	°	In.	In. 29+	°	In.	In. 29+	°	In.	In. 29+	°	In.	In. 29+	°	In.	In. 29+	°	In.	In. 29+	°	In.	In. 29+	°	In.
1858	·955	47·9	1·80	·923	46·5	3·24	·866	47·3	4·65	·756	50·2	2·02	·692	52·3	4·30	·629	51·6	7·31	·670	49·1	4·22	·850	46·7	1·25
1859	·861	47·1	5·29	·835	48·1	5·89	·832	49·7	3·14	·773	51·1	3·79	·612	52·6	9·63	·622	52·6	12·30	·723	48·9	3·28	·860	46·9	1·88
1860	·846	44·6	0·54	·895	44·4	1·56	·732	47·0	0·84	·558	46·3	2·02	·636	45·9	8·04	·499	48·3	13·06	·505	46·3	6·98	·730	42·8	0·57
1861	·778	47·1	2·26	·910	46·3	2·56	·802	46·6	1·62	·614	49·1	1·85	·629	51·0	8·59	·631	50·9	11·41	·798	50·0	3·57	·860	48·7	3·01
1862	·858	47·0	5·93	·886	48·4	3·39	·722	49·9	0·88	·661	51·0	1·81	·606	52·3	7·96	·641	51·9	13·49	·726	49·9	5·59	·750	47·8	2·05
1863	·870	47·1	0·75	·861	47·7	2·05	·837	48·4	0·92	·683	51·2	2·63	·651	51·2	9·30	·723	50·9	6·86	·841	49·2	1·61	·857	47·6	0·78
1864	·842	45·0	3·17	·796	46·0	2·16	·578	49·8	2·34	·645	51·0	3·39	·669	51·5	6·83	·728	50·5	5·07	·707	47·8	2·03	·850	45·7	1·24
1865	·924	48·1	5·83	·870	43·9	0·62	·655	50·8	2·44	·620	51·3	7·70	·654	52·5	9·48	·724	50·1	3·28	·721	48·4	2·12	·807	47·2	2·63
1866	·771	48·2	0·38	·707	48·0	1·75	·666	52·9	3·77	·618	52·5	7·72	·647	51·9	17·00	·680	50·1	6·05	·754	48·1	3·07	·718	48·6	2·02
1867	·880	44·7	3·86	·733	45·6	4·70	·694	50·9	1·83	·628	54·4	4·30	·643	52·4	11·84	·773	49·7	4·89	·897	46·8	3·09	·839	44·4	3·11
1868	·810	47·9	1·36	·837	48·0	4·10	·938	49·9	1·04	·497	52·7	2·70	·651	54·4	12·50	·685	53·1	6·67	·812	52·5	2·32	·759	49·0	3·59

According to the observations of Schwabe the numbers of groups of solar spots which occurred in the years 1858-68 were as follows:—

Year.	No. of Groups.	Year.	No. of Groups.
1858	188	1864	130
1859	205	1865	93
1860	211	1866	45
1861	204	1867	25
1862	160	1868	101
1863	124		

The mean number is 135, and therefore it appears that during the five years 1858-62 the frequency of solar spots was above the average, and during the six years 1863-68 it was below. In order then to ascertain the effects of changes of solar activity upon the distribution of barometric pressure, temperature, and rainfall under different winds, the above table was divided into two tables, the first comprising the results for the five years 1858-62, when the number of solar spots was above the average, and the second those for the six years 1863-68, when the number of spots was below the average. The means of the numbers under each wind in both tables were then determined, and a comparison of the two sets of results thus obtained showed, for each element, the nature of the changes which had taken place.

Barometric Pressure.

The mean pressures under different winds for the two periods, and their differences, are as follows:—

	Mean Pressure 1858–62. In.	Mean Pressure 1863–68. In.	Difference. In.
N.	29·859	29·849	+0·010
N.E.	29·890	29·801	+0·089
E.	29·791	29·728	+0·063
S.E.	29·672	29·615	+0·057
S.	29·635	29·652	−0·017
S.W.	29·604	29·719	−0·115
W.	29·684	29·789	−0·105
N.W.	29·810	29·805	+0·005

It appears therefore that in the years of maximum solar spot frequency the maximum barometric pressure took place under north-east winds, and the minimum under south-west; but in years of minimum frequency the maximum and minimum pressures occurred respectively under north and south-east winds. The difference of pressure under north-west winds is almost inappreciable; and the differences under north and south winds are small; but those under north-east, east, south-east, south-west, and west winds are too considerable to be fairly attributable to accidental causes. In order then to determine whether they are due to the operation of a change in the intensity of solar activity I have made the following comparison of the mean pressures under north-east, east, and south-east winds with those under south-west and west winds :—

	Mean Pressure under winds from N.E., E., & S.E. In.	Mean Pressure under winds from S.W. & W. In.	Difference. Inch.
1858	29·848	29·649	+ ·199
1859	29·813	29·672	+ ·141
1860	29·728	29·502	+ ·226
1861	29·775	29·714	+ ·061
1862	29·756	29·683	+ ·073
1863	28·794	29·782	+ ·012
1864	29·673	29·717	− ·044
1865	29·715	29·722	− ·007
1866	29·664	29·717	− ·053
1867	29·685	29·835	− ·150
1868	29·757	29·748	+ ·009

The maximum difference occurred in 1860, when solar spot frequency was at a maximum, and the minimum difference in 1867, when solar spot frequency was also at a minimum, and the general course of the differences has a remarkable similarity to that of the numbers representing the variations of solar spot frequency.

As the rate of variation in the pressures during the maximum years 1858-62 was greatest in the quadrant between north-west and south-west, and as winds from the westward coming over the Atlantic are probably less affected by disturbing causes than those coming from the eastward over the continent of Europe, it appeared to me that the nature of the law of change of the pressures would be best indicated by a comparison of the differences between the pressures under north-west and south-west winds. These differences are as follows: —

1858	·221	1864	·122
1859	·238	1865	·083
1860	·231	1866	·038
1861	·229	1867	·066
1862	·109	1868	·074
1863	·134		

These numbers indicate a maximum at the end of 1859, a minimum in the latter half of 1866, and a secondary maximum at the end of 1863, thus presenting a very close agreement with the results obtained by De la Rue, Stewart, and Loewy from actual measurements of the areas of the sun spots observed during the period under discussion.

The mean pressure under all winds is 29·744 inches in both periods, but the sum of the differences of the individual pressures from this mean is 0·755in. in the first period, and only 0·530in. in the second. It appears therefore that the forces which produce the movements of the atmosphere are more energetic in years of maximum solar activity than in years of minimum.

Temperature.

	Mean Temp. 5 years, 1858-62.	Mean Temp. 6 years, 1862-68.	Difference.
N.	46·7°	46·8°	− 0·1°
N.E.	46·7	46·5	+ 0·2
E.	48·1	50·4	− 2·3
S.E.	49·5	52·2	− 2·7
S.	50·8	52·3	− 1·5
S.W.	51·1	50·7	+ 0·4
W.	48·8	48·8	0·0
N.W.	46·6	47·1	− 0·5

In the first period the maximum temperature occurs under winds from south-west, and in the second period under winds from about south-south-east. The greatest differences between the two periods occur with east, south-east, and south winds. Comparing the mean temperature under south-west winds with that under south and south-east winds we have following differences:—

1858	+ 0·35°	1864	– 0·75°
1859	+ 0·75	1865	– 1·80
1860	+ 2·20	1866	– 2·10
1861	+ 0·85	1867	– 3·70
1862	+ 0·25	1868	– 0·45
1863	– 0·30		

Here we have again a maximum in 1860 and a minimum in 1867.

As the temperature diminished under two winds only, the north-east and south-west, we may compare the means of the temperatures under these winds with those of the wind under which the greatest increase of temperature took place, the south-east, thus:—

1858	– 1·15°	1864	– 2·75°
1859	– 0·75	1865	– 4·30
1860	+ 0·05	1866	– 3·45
1861	– 0·50	1867	– 6·75
1862	– 0·85	1868	– 2·15
1863	– 1·90		

Again we have a maximum in 1860 and a minimum in 1867, and it is therefore evident that the distribution of temperature under different winds, like that of barometric pressure, is very sensibly influenced by the changes which take place in solar activity.

Rainfall.

	Mean Annual Amount.		
	5 years, 1858-62. Inches.	6 years, 1863-68. Inches.	Difference. Inches.
N	3·16	2·56	+ 0·60
N.E.	3·33	2·56	+ 0·77
E.	2·23	2·06	+ 0·17
S.E.	2·30	4·74	– 2·44
S.	7·70	11·16	– 3·46
S.W.	11·51	5·47	+ 6·04
W.	4·73	2·37	+ 2·36
N.W.	1·75	2·23	– 0·48

In the first period the maximum fall occurs with south-west, and in the second period with south winds; and the greatest differences between the two periods are with winds from south-east, south, south-west, and west, the differences with south-east and south winds being negative, and those with south-west and west winds positive. Comparing then the sums of the amounts which fell under the first two winds with those which fell under the last two, we have the following results:—

	Sum of S.E. & S. Inches.	Sum of S.W. & W. Inches.	Difference. Inches.
1858	6·32	11·53	− 5·21
1859	13·42	15·58	− 2·16
1860	10·06	20·04	− 9·98
1861	10·44	14·98	− 4·54
1862	9·77	19·08	− 9·31
1863	11·93	8·47	+ 3·46
1864	10·22	7·10	+ 3·12
1865	17·18	5·40	+ 11·78
1866	24·72	9·12	+ 15·60
1867	16·14	7·98	+ 8·16
1868	15·20	8·99	+ 6·21

It will be observed that in every year of the first period (1858-62) the differences were negative, while in every year of the second period (1863-68) they were positive; or, that the amounts of rainfall under south-west and west winds were invariably greater than those under south-east and south winds during the years when the number of solar spots was above the average, and invariably less in the years when the number of sun spots was below the average; and further, that the greatest difference in the first series of years occurred in 1860, at the time of a solar spot maximum, and that in the second series in 1866, at or very near the time of a solar spot minimum.

Considering the irregular character of rainfall, both in the times of its occurrence and the amounts in which it falls, I confess I was scarcely prepared to expect that the results of rainfall observations would agree so closely with those of barometric pressure and temperature.

Instead of comparing the differences between the amounts of rainfall it would perhaps be more correct to compare their ratios, but the results would be substantially the same. Thus dividing the entire series of 11 years into 3 groups, the first including the four years 1858-61, one of which was a year of maximum frequency of solar spots; the second the four years 1862-65; and the third the three years 1866-68, one of which was a year of minimum frequency, we have the following amounts and their ratios:—

	Sum of Rainfall under S.E. & S. winds. Inches.	Sum of Rainfall under S.W. & W. winds. Inches.	Ratio.
4 years 1858–61	40·24	62·13	0·64
4 years 1862–65	49·10	40·05	1·22
3 years 1866–68	56·06	26·09	2·14

Here we have a small ratio in years of maximum solar activity, and a large ratio in years of minimum, and a ratio of intermediate value for the intervening years.

It will I think be admitted that the results of this investigation support very strongly the hypothesis which led me to undertake it. They show also strikingly that the future progress of meteorology must depend to a much greater extent than has been generally supposed upon the knowledge we may obtain of the nature and extent of the changes which are constantly taking place on the surface of the sun; and therefore, in the interests of meteorological science, it is evidently very desirable that observations of solar phenomena should be greatly multiplied by the establishment, in various parts of the world, of observatories specially devoted to this object, so that a continuous daily or even hourly record may be obtained of the state of the solar disc and its appendages, and the results discussed in connection with those of observations of meteorological phenomena.

"Further Experiments on the Rupture of Iron Wire," by JOHN HOPKINSON, B.A., D.Sc.

In a paper read before this Society some weeks ago I gave a theory of the rupture of an iron wire under a blow when the wire is very long, differing from that usually accepted practically, and an account of a few experiments in confirmation.

In the simple case considered mathematically, certain conditions which have a material effect on the result are wholly neglected, such as the weight hung below the clamp to keep the wire tort, and the mass and elasticity of the clamp; these I have taken into consideration.

Of course it is impossible to make experiments on an infinitely long wire; we are therefore compelled to infer the breaking blow for such a wire from the blow required to break a short wire *close* to the clamp. The wire used in the following experiments was from 9 to 12 feet long the clamp weighed 26 oz., and the weight at the end of the wire was 61 lbs. Several attempts were made to support the upper extremity of the wire on an indiarubber spring, in order that the wire might behave like a long wire and break at the bottom, and not be affected by waves reflected from the upper clamp, but without success; so that I was obliged to fall back on the plan of discriminating the cases in which the wire broke at the lower clamp from those in which the wave produced by the blow passed over this point without rupture and broke the wire elsewhere.

The height observed is corrected by multiplication by the factor $\left(\frac{M}{M+M'}\right)^2$ where M is the mass of the falling weight and M′ of the clamp. This correction rests on the assumption that the clamp and cast iron weight are practically incompressible, and hence that at the moment of impact they take a common velocity which is that causing rupture of the wire. This assumption will of course be slightly in error, and experiments were made in which leather washers were interposed between the clamp and the iron weight to cushion the blow. The error produced by these washers would be of the same nature as that produced by elasticity in the clamp, but obviously many times as large. If the error produced by one thick leather washer be but 10 inches of reduced height, surely the effect of the elasticity of the clamp will fall well within the limits of error in these experiments.

The effect of cold on the breaking of the wire was tried thus — the clamp and the lower extremity of the wire were cooled by means of ether spray, and the weight dropped as before. The effect of cooling the wire near the clamp was in all cases to make the wire break more easily, in some cases very markedly so. A similar result would follow under similar circumstances from the formula for the resilience $\frac{1}{2}\frac{lF^2}{E}$; and it is the almost universal experience of those who have to handle crane chains and lifting tackle that these are most liable to breakage in cold weather. To this effect of temperature and to the variable quality of wire even in the same coil I attribute the discrepancy between the various observations.

The first column gives the height of fall observed, the second the reduced height, and the third the point at which the wire broke. The observations marked * are those in which cold was applied. The two series were tried on different days about a fortnight apart and on wire from different parts of the same coil. In all cases the upper clamp rested on the bare boards of the floor above.

FIRST SERIES.

16lbs. weight.

Inches.	Inches.	Point of Rupture.
72	60	18″ from top.
78	65	12″ from bottom.
78	65	24″ from top.
81	67½	at top and bottom.
82	68½	21″ from top.
84	70	at bottom.
84	70	at bottom.
*48	40	did not break.
*54	45	at bottom.
*60	50	at bottom.
*72	60	at bottom.

28lbs. weight.

72	65	20″ from top.
78	70	close to top.
79½	71½	at bottom.
81	73	at bottom.

7lbs. weight.

81	54	at top.
84	56	at bottom.
*72	48	at bottom.
*75	50	at bottom.

SECOND SERIES.

28lbs. weight.

54	48	broke at top.
60	53½	bottom and half way up.
60	53½	at top.
63	56	at bottom.
66	59	at bottom.
69	61½	at bottom.
72	64½	at bottom.
*36	32	at top.
*48	43	at bottom.

16lbs. weight.

60	50	half way up.
66	55	at bottom.

With one dry leather washer.

72	60	4″ from bottom.
66	55	near top.

Two dry washers.

72	60	6″ from bottom.

Three soaked washers.

78	65	broke in middle.
83	69	at top.

It should be noticed that the formula velocity $= \frac{F}{\sqrt{E\mu}}$ cannot be depended on except as indicating the general character of the phenomena; for let us attempt to deduce the height of fall from this formula, $h = \frac{1}{2g}\frac{F^2}{E\mu}$.

An inch wire 1 foot long weighs 3·34 lbs., the breaking force in proper units = 80,000 × 32, and the elasticity = 25,000,000 × 32, whence h = 38 feet about.

This discrepancy I have not yet accounted for.

PHYSICAL AND MATHEMATICAL SECTION.

November 7th, 1871.

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

"On Changes in the Distribution of Barometric Pressure, Temperature, and Rainfall under different Winds, during a Solar Spot Period," by JOSEPH BAXENDELL, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society held March 5, 1872. See p. 111].

December 5th, 1871.

THOMAS CARRICK, Esq., in the Chair.

"On the Distribution of Rainfall under different Winds, at St. Petersburg, during a Solar Spot Period," by JOSEPH BAXENDELL, F.R.A.S.

In the paper which I read at the last meeting of the Section it was shown that, at Oxford, changes take place in the relative amounts of rainfall under different winds in a period corresponding with that of solar spot frequency. Thus in the years when the number of groups of solar spots, as observed by Schwabe, was above the average, the amount of rainfall under west and south-west winds was greater than that under south and south-east winds, while in the years when the number of groups of solar spots was below the average the reverse of this took place, the amount of rainfall under west and south-west winds being *less* than that under south and south-east winds. The hypothesis which led to the investigation requires, however, that great diversity should exist in the relative amounts of rainfall under different winds at different stations. While at some the distribution will be similar to that at Oxford, at others it will be of an opposite, and in others again of an intermediate character; but, whatever may be the nature of the distribution at any station, the changes to which it will be subject will take place in a period identical with the solar spot period. In some localities the changes will be so slight, or so irregular, as not to be immediately referable to any well-defined law. These points on the surface of the earth may be regarded as nodal points in the general system of circulation of the great currents of the atmosphere.

Among the places at which it seemed to me likely that the law of change in the relative amounts of rainfall under different winds would be found to differ considerably from that which prevails at Oxford is St. Petersburg. I therefore extracted from the volumes of the *Annales de l'Observatoire Physique Central de Russie* the amounts of rain which fell under different winds at St. Petersburg during the eleven years 1854–64. The results are shown in the following table:—

RAINFALL UNDER DIFFERENT WINDS, AT ST. PETERSBURG, DURING A SOLAR SPOT PERIOD.

	N.	N.W.	W.	S.W.	S.	S.E.	E.	N.E.	CALM.
	In.	In.	In.	In.	In.	In.	In.	In.	In.
1854	0·800	0·675	3·543	2·101	1·083	0·776	1·041	1·087	1·644
1855	2·056	2·688	1·192	3·688	1·720	0·558	1·509	0·961	1·325
1856	0·313	1·014	5·174	2·331	1·386	1·551	0·535	1·852	0·800
1857	1·871	0·000	2·700	1·640	1·223	0·757	0·181	2·518	1·856
1858	0·213	0·445	2·218	2·441	0·475	2·759	1·025	1·075	1·002
1859	0·375	0·548	4·961	4·371	2·329	2·251	1·038	0·618	0·639
1860	1·400	1·182	2·194	3·088	1·910	2·460	2·469	0·301	0·683
1861	1·861	0·123	6·327	2·681	3·225	2·259	1·376	0·978	0·332
1862	1·045	1·448	3·290	2·717	2·032	1·921	0·497	0·431	0·368
1863	0·332	2·446	2·521	3·390	3·110	1·984	0·512	0·831	0·000
1864	2·171	6·560	3·038	4·580	2·017	7·532	1·201	2·430	0·656
Means.	1·131	1·557	3·378	3·002	1·865	2·258	1·035	1·189	0·846

From the mean values in the last line of this table it appears that there was a principal maximum of rainfall under west winds, and a secondary maximum under south-east winds; a principal minimum under east winds, and a secondary minimum under south winds.

In the eleven years 1854–64 the number of groups of solar spots, as observed by Schwabe and others, was above the average in the five years 1858–62, and below the average in the remaining six years 1854–57 and 1863–64. I therefore divided the series of rainfall results into two corresponding series, and, taking the means of the amounts under each wind, I obtained the following numbers:

	Mean Annual Amount of Rainfall, 1858–62. Inches.		Mean Annual Amount of Rainfall, 1854–7 and 1863–4. Inches.		Difference. Inches.
N.	0·979	......	1·257	......	– 0·278
N.W.	0·749	......	2·230	......	– 1·481
W.	3·798	......	3·028	......	+ 0·770
S.W.	3·059	......	2·955	......	+ 0·104
S.	1·994	......	1·757	......	+ 0·237
S.E.	2·330	......	2·198	......	+ 0·132
E.	1·281	......	0·830	......	+ 0·451
N.E.	0·681	......	1·613	......	– 0·932
C.	0·605	......	1·047	......	– 0·442

The differences in the last column show that the mean amounts of rainfall under west, south-west, south, south-east, and east winds are greater in years of maximum solar spot frequency than in years of minimum, while the amounts under north-east, north, and north-west winds, and calms, are less. Comparing, then, the total amounts which fell under west, south-west, south, south-east, and east winds in each year with those which fell under north-east, north, and north-west winds, and in calms, we have the following results:—

	Total Amounts of Rainfall under W., S.W., S., S.E., and E. winds. Inches.	Total Amounts of Rainfall under N.E., N., & N.W., winds and calms. Inches.	Ratios.	Corrected Ratios.	Groups of Solar Spots.
1854	8·552	4·026	2·03		
1855	8·697	7·030	1·23	2·00	79
1856	10·977	3·979	2·75	1·67	34
1857	6·501	6·245	1·04	2·34	98
1858	8·918	2·735	3·22	3·71	188
1859	14·950	2·180	6·86	4·49	205
1860	12·121	3·566	3·39	5·00	211
1861	15·868	3·294	4·76	3·77	204
1862	10·457	3·292	3·17	3·71	160
1863	11·517	3·609	3·19	2·64	124
1864	18·368	11·816	1·55		

The mean ratio is 3·01, and the ratios for the years of maximum solar spot frequency are all above this mean, while those for minimum years are all below it, with only one unimportant exception.

In order now to eliminate as far as possible the effects of accidental disturbing causes we may take the means of the ratios of every three successive years, and in this way we obtain the corrected ratios in the fifth column of the above table. For convenience of comparison I have added in the sixth column the number of groups of solar spots observed in each year by Schwabe, and a glance at the two sets of numbers will show the remarkably close agreement which exists between them in the times of their maxima and minima, which seems to me fully to justify the conclusion that both classes of phenomena are intimately connected, either as cause and effect, or as effects of the same cause.

Excluding the amounts of rain which fell during calms the corrected ratios become: —

1855	......	2·77	1860	......	6·42
1856	......	2·15	1861	......	4·37
1857	......	3·32	1862	......	4·04
1858	......	5·40	1863	......	2·80
1859	......	6·31			

It will be observed that the course of these numbers is almost identical with that of the numbers obtained when the amounts of rain which fell during calms are combined with those which fell under north-east, east, and north-west winds.

The close agreement which has thus been shown to exist at St. Petersburg between the times of maximum and minimum frequency of solar spots, and those of the variations in the distribution of rainfall under different winds, gives increased value to the results derived from the Oxford observations, and affords additional support to the hypothesis I ventured to advance in a former paper — that changes in solar activity, and consequently in the magnetic condition of the earth, produced corresponding changes in the directions and velocities of the great currents of the atmosphere, and in the distribution of barometric pressure, temperature, and rainfall. It is therefore evidently very desirable to discuss observations made at stations in various parts of the globe with reference to the variations which take place in solar activity, and thus to determine for each station the nature of the changes which take place in the relations between the several meteorological elements during a solar spot period.

February 27th, 1872.

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

"Results of Observations, registered at Eccles, on the Direction and Range of the Wind for 1869, as made by an Automatic Anemometer for Pressure and Direction," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The following anemometric results have been obtained from an instrument made by Mr. William Oxley, of Manchester, and which has been exhibited and explained at a meeting of this Section of the Society. This instrument records by means of a pencil the range which the wind has made through the degrees of the compass in 24 hours, and the exact point or degree at which the greatest pressure took place, as well as the amount in pounds of such pressure. From these automatic registrations the mean or general direction of the wind for any day is easily obtained, as well as the number of degrees of the compass through which the wind may have veered. The results presented below are for one year only, but it is my intention, as early as possible, to present to the Section the results of the subsequent years, as it is clearly of the utmost importance to all meteorological research that observations from all kinds of automatic instruments be thoroughly investigated and discussed

In the first table below is represented the number of days in the year 1869 on which the mean direction of the wind was on or about the following 16 points of the compass :—

Points of the compass...	N	NNE	NE	ENE	E	ESE	SE	SSE
Number of days	16	11	14	18	16	13	10	15
Points of the compass...	S	SSW	SW	WSW	W	WNW	NW	NNW
Number of days	29	32	18	33	46	45	28	21

This shows how the frequency of the winds on the west side of the compass exceeds the east side; but this is seen in a more striking manner when the above days are referred to the four points of the compass only. When thus reduced they appear as follows : —

Cardinal Points	N	E	S	W
Number of days	84·5	56·5	89·5	134·5

The maximum of direction here seems to lie between the south and the west, and the minimum between the north and the east; and as I have shown in papers previously read before this Section that the greatest amount of rain falls when the direction of the wind is between the south and the west, and the least amount falls when the direction of the wind is between the north and the east the coincidence is not without significance.

In the following table is represented the mean number of degrees through which the wind veered when the mean or general direction was on or about the given 16 points of the compass.

Points of the compass...	N	NNE	NE	ENE	E	ESE	SE	SSE
Number of degrees through which the wind veered	107	124	117	148	184	154	143	103
Points of the compass...	S	SSW	SW	WSW	W	WNW	NW	NNW
Number of degrees through which the wind veered.........	123	133	192	195	207	160	163	127

If the number of degrees of range on the East and West side of the compass be added together, it will be seen that the sum of the degrees on the East side is 1080, whilst the sum of the West side is 1300, showing a ratio of excess of the West side over the East of 1·2. But if the degrees for each of the 8 points on the East side be added to the degrees of each of the 8 points on the West side the following result appears :—

Points of the compass	N S	NNE SSW	NE SW	ENE WSW	E W	ESE WNW	SE NW	SSE NNW
Number of degrees through which the wind veered	230	257	309	343	391	314	306	230

The maximum of these numbers of degrees is found in the East and West, both severally and conjointly, and the minimum in the same way in the SSE and NNW. This seems to show that the equatorial currents take a much wider sweep over the earth than the polar currents do, or rather that their oscillatory waves are more extensive. I have, below, reduced the number of degrees through which the wind has veered to the four cardinal points, and they appear as follows :—

Cardinal points	N	E	S	W
Number of degrees through which the wind has veered ...	526	578	583	692

This shows that the oscillation increases in the direction of the sun's course, and attains its maximum at the West point, or rather between the South and the West, thus that the maximum of wind frequency is similar in position to its maximum of oscillation.

The following table represents the ratio of the advance which the veering of the wind made with the sun's course, against its retrogression for each of the given 16 points of the compass :—

Points of the compass...	N	NNE	NE	ENE	E	ESE	SE	SSE
Ratio of advance with the sun's course ...	1·07	2·88	3·46	2·53	2·06	2·00	1·04	3·25
Points of the compass...	S	SSW	SW	WSW	W	WNW	NW	NNW
Ratio of advance with the sun's course ...	0·95	1·50	1·46	1·48	1·16	1 08	1·18	1·10

The mean proportion of advance which the wind makes with the sun's course on the East side of the compass, as results from the foregoing table, is nearly twice as much as such advance is on the West side, for the mean proportion of the advance on the East side is 2·28, whilst on the West side it is only 1·21. And it seems to show that the progress of the wind round the compass in the direction of the sun's course is retarded chiefly by westerly winds.

I may also state that the horizontal movement of the air has a maximum at a point similar to the maximum of wind frequency and wind oscillation, for on reducing and referring the horizontal movement of the air for 1869 to the four cardinal points, I find the mean values to be as follows :—

Cardinal points	N	E	S	W
Mean horizontal movement of the air ...	91	99	117	117

Thus the maximum lies between the South and the West.

"On Black Bulb Solar Radiation Thermometers exposed in Various Media," by G. V. Vernon, F.R.A.S., F.M.S.

Being desirous to make some comparisons of the readings of black bulb thermometers exposed in various media, I got Messrs. Negretti and Zambra to make me a set of three thermometers, in addition to the ordinary black bulb maximum in vacuo.

The glass tubes containing the thermometers were filled with hydrogen gas, carbonic acid gas, and atmospheric air, at 32° F.; the latter thermometer being described in the tables as filled with compressed air. The instruments were all alike, the glass tube enclosing them being of equal thickness. The thermometers were all compared with the Greenwich standard, and require no index error correction.

The observations were made in the years 1861 to 1865, and the period embraced was just four years. Since the latter year the observations have been discontinued, but the thermometers remain in the same position they were originally placed in.

In the tables annexed table I gives the mean monthly readings of the thermometers for each year, with the additional readings of the black bulb freely exposed, and also that of the maximum thermometer in the shade.

Looking at the yearly means, the black bulb in vacuo gives the highest mean reading, the one with carbonic acid gas comes next, followed by the condensed air one, that filled with hydrogen giving the lowest temperature.

Examination of the monthly values shows that the maximum for all the thermometers occurs in July, and the minimum in January. The minima of the enclosed thermometers read nearly all alike; with the maxima the vacuo and carbonic acid ones are nearly equal, and the same remark applies to the hydrogen one and the one filled with compressed air; the latter agrees with what Tyndal points out, that hydrogen and atmospheric air absorb heat equally.

Table 3 gives the differences of each monthly mean referred to the reading of a freely exposed black bulb thermometer.

In volume 5, page 169, of Symons's "Meteorological Magazine," there is a paper by Mr. Francis Nunes, giving comparisons of carefully made black bulb thermometers by Pastorelli, showing a considerable difference between the thermometer in vacuo and the one partially exhausted; his observations were made in October, and show a difference of 1·2° to 11·5, the vacuo thermometer being the highest of the two. Mr. Nunes also states that an enclosed thermometer without any exhaustion reads still lower, being from 0·8° to 12·8 below the vacuum thermometer.

From my observations the difference between the vacuo and condensed air thermometers is never very large, amounting rarely in individual cases to 5·0° to 6·0°, but in July, 1865, reached occasionally 10·0°: the mean difference in July only reaching 4·3°.

I am not aware of any similar series of observations to be found anywhere else, and thought it might be desirable to tabulate the values for comparison with any subsequent series that may be made.

TABLE 1.

Radiation Thermometers.—Mean Monthly Maximum in the Sun.

January.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1862	48·0	46·3	46·4	47·8	44·6	43·2
1863	......	......	......	......	......	......
1864	45·1	45·0	44·7	45·4	43·4	41·9
1865	44·3	44·1	44·1	44·2	43·6	41·3
Means......	45·8	45·1	45·1	45·8	43·8	42·1

February.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1862	54·8	53·2	53·0	54·2	49·7	46·3
1863	65·0	61·9	62·4	62·1	55·2	50·0
1864	50·5	49·3	48·9	53·6	45·5	42·0
1865	50·7	51·3	51·1	50·8	46·8	42·3
Means......	55·2	53·9	53·8	55·2	49·3	45·1

March.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1862	64·4	61·3	60·3	60·7	56·8	48·9
1863	75·7	73·6	72·1	72·1	62·8	52·6
1864	70·6	69·6	68·3	69·1	59·8	48·9
1865	65·3	64·9	65·9	63·1	57·0	44·1
Means......	69·0	67·3	66·6	66·2	59·1	48·6

April.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1862	81·6	76·1	73·6	75·3	70·8	57·5
1863	86·0	83·3	81·2	81·8	69·9	56·2
1864	85·8	83·4	81·9	82·8	76·0	60·0
1865	92 6	91·3	89·4	89·3	81·5	63·5
Means......	86·5	83·5	81·5	82·3	74·6	59·3

May.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Condensed Air.	Black Bulb Freely Exposed.	Maximum in Shade.
	°	°	°	°	°	°
1862	95·7	89·1	86·9	86·3	81·2	64·4
1863	88·4	87·1	86·1	86·6	74·8	61·2
1864	97·5	96·4	95·0	95·8	81·4	66·5
1865	93·7	88·2	87·1	87·9	79·3	65·0
Means......	93·8	90·2	88·8	89·1	79·2	64·3

June.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Condensed Air.	Black Bulb Freely Exposed.	Maximum in Shade.
	°	°	°	°	°	°
1862	88·9	86·3	84·2	84·8	69·6	58·8
1863	97·2	96·6	93·0	94·1	81·4	66·8
1864	98·9	97·5	94·9	95·5	81·2	66·2
1865	101·8	104·6	99·0	101·0	93·0	72·8
Means......	96·7	96·2	92·8	93·8	81·3	66·1

July.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Condensed Air.	Black Bulb Freely Exposed.	Maximum in Shade.
	°	°	°	°	°	°
1862	97·8	94·0	91·9	91·8	80·3	66·7
1863	101·1	100·7	98·1	98·7	87·3	70·8
1864	100·8	108·6	98·8	98·8	86·0	70·9
1865	110·6	107·2	103·4	103·8	94·4	76·6
Means......	102·6	102·6	98·1	98·3	87·0	71·2

August.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Condensed Air.	Black Bulb Freely Exposed.	Maximum in Shade.
	°	°	°	°	°	°
1862	......	......	......	......	......	......
1863	93·9	93·5	91·8	92·7	83·4	68·7
1864	96·3	94·5	91·0	92·1	80·9	68·0
1865	102·6	97·0	94·8	97·0	81·7	68·7
Means......	97·6	95·0	92·5	93·9	82·0	68·4

September.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1861	86·5	80·9	78·5	79·6	78·5	63·9
1862	83·6	81·6	80·0	80·0	74·2	62·6
1863	79·2	78·5	75·2	78·7	70·1	58·5
1864	91·7	88·4	84·6	90·1	73·3	65·2
Means......	85·2	82·3	79·6	82·1	74·0	62·6

October.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1861	72·6	69·7	68·9	70·9	64·8	60·6
1863	71·3	68·0	66·6	67·2	61·9	56·5
1863	66·0	66·2	64·6	67·5	62·0	55·9
1864	69·3	66·9	64·9	68·6	61·7	57·0
Means......	69·8	67·7	66·2	68·6	62·6	57·5

November.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1861	51·4	50·7	49·6	51·6	47·2	46·3
1862	46·7	47·0	46·8	46·4	44·9	43·7
1863	53·6	53·4	52·8	54·4	52·3	50·7
1864	53·0	52·6	52·9	52·5	49·1	48·2
Means......	51·2	50·9	50·5	51·2	48·4	47·2

December.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	Con-densed Air.	Black Bulb Freely Exposed.	Maxi-mum in Shade.
	°	°	°	°	°	°
1861	47·2	45·3	45·3	48·0	44·0	44·8
1862	49·6	49·5	49·9	49·6	48·2	48·0
1863	49·0	49·2	48·9	49·6	48·6	48·3
1864	43·2	43·5	43·5	43·4	41·9	43·2
Means......	47·2	46·9	46·9	47·7	45·7	46·1

TABLE 2.

Mean Results of the Four Years.

MONTH.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	In Condensed Air.	Blk. Bulb freely Exposed.	Maximum in Shade.
	°	°	°	°	°	°
January	45·8	45·1	45·1	45·8	43·8	42·1
February	55·2	53·9	53·8	55·2	49·3	45·1
March	69·0	67·3	66·6	66·2	59·1	48·6
April	86·5	83·5	81·5	82·3	74·6	59·3
May	93·8	90·2	88·8	89·1	79·2	64·3
June	96·7	96·2	92·8	93·8	81·3	66·1
July	102·6	102·6	98·1	98·3	87·0	71·2
August	97·6	95·0	92·5	93·9	82·0	68·4
September	85·2	82·3	79·6	82·1	74·0	62·6
October	69·8	67·7	66·2	68·6	62·6	57·5
November	51·2	50·9	50·5	51·2	48·4	47·2
December.........	47·2	46·9	46·9	47·7	45·7	46·1
Annual Means.	75·0	73·4	71·9	72·9	65·6	56·6

TABLE 3.

Differences from the Readings of the Freely Exposed Black Bulb in the Sun.

MONTH.	In Vacuo.	In Carbonic Acid Gas.	In Hydrogen Gas.	In Compressed Air.
	°	°	°	°
January	2·0	1·3	1·3	2·0
February	5·9	4·6	4·5	5·9
March	9·9	8·2	7·5	7·1
April	11·9	8·9	6·9	7·7
May	14·6	11·0	9·6	9·9
June	15·4	14·9	11·5	12·5
July	15·6	15·6	11·1	11·3
August	15·6	13·0	10·5	11·9
September	11·2	8·3	5·6	8·1
October	7·2	5·1	3·6	6·0
November	2·8	2·5	2·1	2·8
December.........	1·5	1·2	1·2	2·0
Means	9·47	7·90	6·28	7·26

"Note on the Relative Velocities of different Winds, at Southport, and Eccles, near Manchester," by JOSEPH BAXENDELL, F.R.A.S.

In November last Mr. Mackereth, F.R.A.S., had an anemometer mounted at his observatory, Eccles, by Mr. Dancer, precisely similar in construction to that mounted at the Southport Meteorological Observatory. Regular observations were commenced with it on the 19th of that month, and as Mr. Mackereth has kindly furnished me with copies of his results to the 17th of February instant, I have thought it might be interesting to compare them with the results of the observations taken at the Southport Observatory.

During the 90 days from November 19, 1871, to February 17, 1872, the total movement of the wind was 13696·4 miles at Eccles, and 29843·0 miles at Southport. The ratio of the mean velocities was therefore as 1 to 2·17, or for every 100 miles at Eccles there was a movement of 217 miles at Southport. Grouping the daily movements at both stations according to the mean daily direction of the wind at Eccles, as shown by Mr. Mackereth's automatic anemometer and referred to 16 points of the compass, we obtain the following results:—

Direction of Wind.	Total Movement. Eccles.	Southport.	Direction of Wind.	Total Movement. Eccles.	Southport.
N.	665·4	1335·8	S.	2855·1	5289·4
N.N.E.	311·5	793·1	S.S.W.	3356·7	6099·1
N.E.	121·0	144·6	S.W.	1507·2	3154·1
E.N.E.	310·3	572·1	W.S.W.	1473·4	3155·6
E.	214·0	581.2	W.	184·0	837·5
E.S.E.	1105·0	2267·4	W.N.W.	136·5	1184·2
S.E.	360·4	888·5	N.W.	72·2	550·8
S.S.E.	1023·7	2989·6	N.N.W.	0·0	0·0

Dividing these results into four groups we have:—

Total Movement of	Eccles. Miles.	Southport. Miles.	Ratio.
N., N.N.E., N.E., & E.N.E. Winds	1408·2	2845·6	1 to 2·02
E., E.S.E., S.E., & S.S.E. ,,	2703·1	6726·7	1 to 2·48
S., S.S.W., S.W., & W.S.W. ,,	9192·4	17698·2	1 to 1·92
W., W.N.W., N.W., & N.N.W ,,	392·7	2572·5	1 to 6·54

The ratios of the velocities at Eccles to those at Southport are therefore greatest with south-west and north-east winds, and least with north-west and south-east winds. The great excess of velocity of north-west winds at Southport is very remarkable.

The results of the above comparison bring out very prominently one of the causes of the great salubrity of Southport as compared with the neighbourhood of Manchester, namely, the much greater mean velocity of the wind, in consequence of which the products of decomposition, and and offensive matters generally which are injurious to health, are much more rapidly removed at Southport than at Manchester.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 5th, 1872.

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

Mr. JOSEPH SIDEBOTHAM, F.R.A.S., called the attention of members to the mass of correspondence in the papers on the origin and spread of Typhoid fever, in which it seems to be considered as proved that the fever is produced by what are termed sewer gases, and the germ theory is entirely ignored, when in all probability it is the true one. The various gases found in sewers are well known, and if produced artificially, as they are in various chemical processes either alone or mixed, are comparatively harmless, even in a more concentrated form than they are ever met with in sewers, at any rate they never produce typhoid fever. If the germ theory be correct the real agents in the spread of this and other similar diseases are germs or particles, many of them sufficiently large to be detected by the miscroscope; these are met with in sewers, but probably not generated there, and are carried, no doubt, by the sewer gases or currents of air, and whenever they find favourable conditions produce the disease. The same effect is produced when impure water is used for drinking, and this again is an argument in favour of the germ theory, as it is never contended that the danger is from any gases in the water.

It is most desirable that these rival theories should be carefully examined, as the modes of getting rid of the danger will necessarily differ widely, whichever theory be accepted; if it be the germ theory, then water-trapped drains would prevent the escape of most, if not all, the germs, but pipes to ventilate the sewers would only diffuse and spread the mischief.

February 26th, 1872.

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

Mr. MARK STIRRUP exhibited sections of shells of mollusca, showing so-called fungoid growths.

He referred to Dr. Carpenter's report on shell structure, presented to the meeting of the British Association, in 1844, in which especial mention is made of a tubular structure in certain shells, and he cites the *Anomia* as a characteristic example. In the last edition of "The Microscope," Dr. Carpenter withdraws his former explanation of this structure, and now refers it to the parasitic action of a fungus. Mr. Stirrup showed sections of this shell penetrated by tubuli from the outer to the inner layers of the shell, and it is upon the inner layer that the curious appearance of sporangia, with slightly branched filamentous processes proceeding from them present themselves.

The parasitic view is strengthened by the fact that these markings are not found on all parts of the shell, and are certainly accidental.

Professor Kölliker maintains the fungoid nature of these tubuli in shells as well as in other hard tissues of animals, as fish scales, &c.

Wedl, another investigator, considers the tubuli in *all* bivalves as produced by vegetable parasites, and that no other interpretation can be given.

This view does not seem to be borne out by the section of another shell which was exhibited, "*Arca navicula*," in which the tubuli are always present, forming an integrant part; they are disposed in a straight and tolerably regular manner between the ridges of the shell; moreover, they have neither the irregularly branched structure nor the sporangia.

ERRATUM.—In the last number of the "Proceedings," p. 99, line 9 from top for "Regnalt" read Renault.

Ordinary Meeting, March 19th, 1872.

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

"Additional Notes on the Lancashire Drift Deposits," by E. W. Binney, F.R.S., F.G.S., President of the Society.

In two previous papers, abstracts of which are printed in the Proceedings for 1870 and 1871, the author has given his views on the high level drift found on the hill sides, and the lower level beds found between Manchester and Oldham. He there shewed the difficulty of classing these deposits under Professor Hull's three-fold division of Upper and Lower Tills or Boulder Clays, divided by sands and gravels.

In the present communication he took the section of the railway from Liverpool to Manchester, kindly supplied to him by Mr. G. B. Worthington, one of our members, running nearly west and east for a distance of 30 miles, and shewed the deposits in the cuttings, and journals of shaft sinkings and bores; and he then followed the Lancashire and Yorkshire line from Miles Platting to near Todmorden, running nearly north and south for a distance of 15 miles, and described the deposits found in its sections, and neighbouring pits and bores, and noticed the singular termination of the drift near to the Rochdale Brick and Tile Works, at Summit, above Littleborough, in the Todmorden valley.

Commencing with the railway at Edge Hill a considerable deposit of Till or Boulder Clay is found at a height of 125 feet above the level of the sea. Then comes the rising ground of Olive Mount, composed of Trias, as exposed in the cutting, and reaching a height of 186 feet, but showing little traces of Till. Next succeeds a series of embankments, affording only one small cutting, chiefly over and through Till, up to Huyton, where the Trias is covered by that deposit. We then reach the Lower Coal Measures of Huyton, and the Trias to the east of them, on which little drift is seen. This part is the highest level on the line, reaching to 205 feet. The Upper Coal Measures of Whiston, the Trias of Rainhill, and the Upper and Middle Coal Measures, and Permian beds of Sutton then succeed, all affording slight traces of Till. East of Sutton we come to the Township of Parr. There, at a place called Havannah, on an elevation of 70 feet, in a bore hole, the following beds were met with :—

	ft.	in.
Soil and Clay	2	0
White Sand	2	6
Soft Clay	1	0
Dark Sand	4	6
Hard Marl	7	0
Quick Sand	6	6
Book Leaf Marl (laminated)	22	6
Gravel, Resting on Blue Metal ...	9	0
	55	0

In a boring at New Wint, near Newton race course, about half a mile to the north of the railway, at a height of 125 feet, the following deposits were found :—

	ft.	in.
Earth and Clay	5	6
Red Marl	9	6
Book Leaf Marl	1	3
Dark Stony Marl.....................	12	0
Toad Back Marl (speckled).........	1	9
Quick Sand	1	8
Toad Back Marl	12	4
Book Leaf Marl	1	0
Loam	3	0
Dark Toad Back Marl..............	9	0
Book Leaf Marl	4	0
Loam	5	0

	ft.	in.
Toad Back Marl	2	0
Loam	4	0
Dry Sand	24	0
Gravel	6	6
Brown Rock (Iron Sand)	3	6
Loam	2	6
Quick Sand	20	9
Gravel	1	3
	130	6

For these two sections I am indebted to the kindness of Mr. John Chadwick, Mining Engineer, of Haydock Green.

After passing the Newton Bridge Station, which is only about 54 feet above the level of the sea, a thin bed of reddish Till is seen covering the Trias until we reach Parkside. A considerable cutting is then found, rising to a height of 111 feet above the sea level, composed of sand, which extends to near Kenyon Junction, where the Till again comes in. This is the only appearance of drift sand seen on the line between Liverpool and Manchester. The course of the railway is then on embankments over the thick bed of Till extending all the way to Bury Lane, a little to the East of which Chat Moss begins. Near Astley Station, at a height of about 60 feet, Mr. Brockbank, F.G.S., in Mr. H. M. Ormerod's cutting, found the following beds, namely—

	ft.	in.
Peat Moss	17	0
Sandy Clay, or Loam	1	6
Till, resting on Trias	26	0
	44	6

Near to Barton Moss Station the late Mr. William Lancaster, in a bore, found as under, viz.—

	ft.
Peat	9
Till	45
Sand and Gravel	24
Red Rock (Trias)	0
	78

At Patricroft, at a height of 60 feet the Till is seen, and was found 15 feet thick in Messrs. Lancaster's coal pit, a little to the north of the line.

Then come the cuttings in the Trias at Eccles, which extend to Weaste, where the Till soon comes in at Seedley Print Works, a little to the north, where, at about 97 feet above the sea, Till was found 71 feet in thickness resting upon Trias. The Till extends through Cross-lane, past Oldfield-road to Ordsall Station, where it is succeeded by the Valley Gravel across Salford to the Victoria Station in Manchester, and it there again comes in and is found next the Workhouse, at a height of about 100 feet, as follows:—

	ft.
Till, bluish colour	9
Till, brown	2
Brown Gravel	2
Trias	0
	13

By the kindness of my friend Mr. Morton, F.G.S., I am enabled to give a general idea of the drift on the banks of the Mersey, which may be rightly described as a bed of Till, about 60 feet in maximum thickness, with a few feet of sand above and below it. Taking the cuttings on the railway as previously given, the higher parts, such as the sections through the Trias at Olive Mount and the Trias and Coal Measures of Huyton, Whiston, Rainhill, and Sutton, although only attaining an elevation of 205 feet above the sea, we have seen that there is little drift covering those strata. The deep cutting between Parkside and Kenyon Junction, attaining an elevation of 112 feet, is the only place where the sands are found apparently lying over the Till, but they cannot now be there seen so as to ascertain whether they overlie or intercalate with it. From Kenyon Junction to Ordsall, Till with Valley Gravels, sometimes covering it, underlies the whole district, with the exception of the Trias near Eccles.

The term marl is commonly used for Till, or Boulder Clay, over the greater part of Lancashire. The only places where fossil shells have been found between Liverpool and Todmorden, so far as at present known, are in the Till south of St. Helens, and in the same deposit at Astley Hall, where *Turritella communis* and *Nassa reticulata*, and some fragments of shells have been met with. For specimens from the latter place we are indebted to Mr. H. M. Ormerod.

Having thus tracked the drift from the banks of the Mersey to Manchester from West to East, we will follow the Lancashire and Yorkshire Railway in a northerly direction through Newton, Middleton, and Blue Pits to Todmorden, or at least to the Rochdale Brick and Tile Works, near the Summit Lock on the canal; for at this point, about 650 feet above the level of the sea, the last traces of the drift were visible, so far as we could see.

Leaving the Victoria Station, the line crosses the valley gravel of the Irk, and runs over Till all the way to Miles Platting, where at an elevation of 183 feet the following beds occurred:—

	ft.	in.
Till	45	0
Sand and Gravel	10	6
	55	6

After going on the level for a short distance, the cuttings through the Till in Newton and Moston are reached. In the 2nd paper read before the society, the section in the Moston coal pit close to the line at page 103 is given, which shows drift beds to the thickness of 184 feet. In a cutting near the colliery a little sand is seen on a level with the rails, and with this exception the Till may be said to continue all the way from Miles Platting to the Slacks Vitriol Works, a little to the north of which the section given at page 184 in the paper before alluded to is met with. After the embankments near the Middleton

Junction are passed, the cuttings expose sand and gravel through Boarshaw, Three Gates, Thornham, and Blue Pits, to Rochdale.

At Boarshaw, about a quarter of a mile to the east of the line, a bore made at an elevation of 450 feet showed the following beds:—

	ft.
Soil	1
Sand and Gravel	5
Marl	15
Sand	35
Marl	13
Sand	10
Marl	3
Hard Sand	161
	243

At Three Gates in Thornham, about half a mile north of the last bore, at an elevation of 460 feet, the drift was as follows :

	ft.	in.
Soil	1	0
Sand	1	0
Marl	10	0
Dry Sand	13	6
Marl	10	6
Quick Sand	33	0
Gravel	1	0
Marl	1	0
Quick Sand	9	0
Marl	21	0
Quick Sand	1	6
Marl	3	0
Dry Sand	5	0
Marl	5	6
Sand	71	0
	187	0

The two last sections did not go through the drift beds; but at a few hundred yards to the north of the last bore, and at about the same elevation also in Three Gates, the following beds were found:—

	ft.	in.
Soil	1	0
Light Marl	4	6
Sand	0	6
Blue Marl	5	8
Sand	11	0
Brown Marl	10	4
Sand	17	0
Blue Marl	7	0
Sand	2	6
Brown Marl	7	0
Sand	4	6
Marl	33	6
Loam	2	0
Marl	2	6
Loam	21	0
Sand	50	6
Hard Stone (Boulder)	1	6
Stony Marl	2	6
Hard Stone (Boulder)	1	0
Stony Marl	30	0
Book Leaf Marl	4	6
Mixture	7	6
Brown Rock		
	227	8

The elevation of the bore hole was 460 feet above the level of the sea, and about a quarter of a mile to the West of Tandle Hill, which rises to an elevation of 750 feet, and is composed of sand and loam to the top, so probably the drift beds here may attain the great thickness of 510 feet assuming that the coal measures at the bore and under the hill are on the same level, a thickness much greater than has been generally supposed to be found in the county. For these interesting journals of bores we are indebted to the kindness of Mr. Clarke, of the Middleton estate office.

About a mile to the west of the railway at Blue Pits station, Mr. Livesey, Mining Engineer, in sinking the Captain Fold Pit, near Heywood, found the following beds at an elevation of about 400 feet.

	ft.
Marl and Sand	6
Loam	9
Strong Marl	9
Loam	1
Sand	17
Gravel	10
Marl	72
Broken Metals	
	124

A little further to the north of the last named locality, and at about the same elevation, Messrs. Roscow and Lord, in sinking, found at Greave :

	ft.	in.
Soil	1	0
Loam and Sand	63	5
Stony Marl	77	9
Sandy Gravel	13	10
	156	0

This information was kindly furnished by Mr. James Stott.

Returning to the railway at Rochdale, few sections of the drift had been obtained near the town, where it must be of great thickness in the middle of the valley of the Roach, but at Mayfield in Butterworth, to the east of the line, at an elevation of about 500 feet, the following bore holes were made in the drift without reaching the underlying coal measures —

No. 1 Bore.

	ft.
Soil	3
Sand	4
Marl	54
Gravel	6
Sand	
	67

No. 2 Bore.

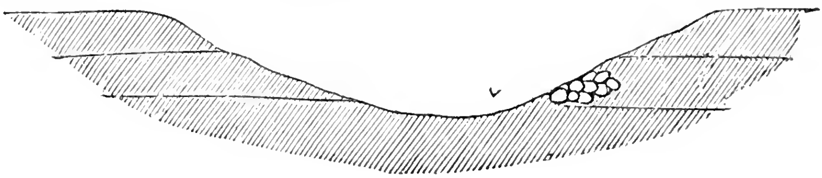
	ft.
Marl	63
Gravel	9
Marl	
	72

On the west side of the valley of the Roach, at the Nook Colliery, was found, according to Mr. Livesey

	ft.	in.
Clay	5	9
Gravel	2	9
Marl	13	6
Black Stone		
	22	0

For about a mile and a half from Rochdale station the line runs over embankments, and then two cuttings through the Till are met with near to Bellfield. After these nothing is seen on the line until it enters the lower coal measures at the south side of the Summit Tunnel and continues through them all the way to Todmorden; but following by the side of the canal, the Till is traced to the Rochdale Brick and Tile Company's works, where it is seen about 12 feet in thickness lying 50 feet above the water in the canal, which Mr. Eadson, the Engineer of the Company, informs me is 603 feet above the sea. This deposit of Till, which lies in a somewhat sheltered place, is of a dark blue colour, and contains greenstones, granites, porphyries, and other foreign rocks. In most of its characters it resembles the ordinary Till of Lancashire except that it contains more rocks, and those of a generally larger size, than are usually met with in that deposit. It is remarkable that this bed of drift, although seen and cut through on the hill side about 50 feet above the level of the valley, the latter below and indeed all the way to Todmorden afforded so far as we could discover, no more Till. In a paper read before the Manchester Geological Society in 1842, and published in its Transactions of the following year, the author stated that he had little doubt but that some of the most ancient portions of the drift had passed the *Pennine Chain* through the valley of Todmorden to Hebden Bridge, by the Summit Valley above Littleborough. No doubt that some drift has passed, as we have ourselves found granites and foreign rocks at Hebden Bridge and at other places in the valley of the Calder, but up to this time, so far as we know, no deposit of Till has been found to the north of the patch now described.

Professor Hull, F.R.S., in a letter in the "Geological Magazine," Vol. III., p. 474, alludes to this part of the valley near where the Till is situated as affording no evidence of having been excavated by the stream flowing in it at the present time, and he notices the remarkable flat watershedding in the valley. Mr. A. H. Green, F.G.S., in his excellent Memoir on the Geology of North Derbyshire and the adjacent parts of Yorkshire, at p. 131, when speaking of the passage of the drift across the *Pennine Chain*, says, "The valley of the Calder cuts right across the ridge; so far as we know no drift is found in it at the summit level, but at Hebden Bridge and at Elland boulders of granite and other foreigners are found, and at the latter place in fair plenty." The accompanying wood cut, Fig. 1, is a section



across the valley near the Brick and Tile Works, showing the position of the patch of Till and the bottom of the valley, above 320 feet in depth, which is a watershed on a flat more than a mile in length, free from Till, so far as our observation went, the greater part of the water flowing to the German Ocean, but some little finding its way down to the Irish Sea. That Till did once occupy the bed of this valley near the Brick and Tile Works is pretty certain, or else the deposit on the sheltered hill side would scarcely now remain to tell its tale.

There can be little doubt of the valley of Todmorden, at least that part of it at the summit is an ancient one, formed long anterior to the period when the Till was deposited, and that the latter once occupied it and was afterward swept out on the rising of the land, as is probable from the small patch left near to the Brick and Tile Works.

Concluding Remarks.

From the sections of drift given in this communication it is clear that these deposits lie on a very irregular surface of underlying carboniferous and triassic rocks, for, while we find little or no drift on strata only 205 feet above the sea level at Rainhill; at Tandle Hill, near Three Gates, above 35 miles to the north-west, we find 510 feet of drift on Coal Measures at an elevation of 233 feet; and, again, 12 feet of that deposit at an elevation of 650 feet near the Rochdale Brick and Tile Works at Summit.

How it is that the drift does not reach to so great an elevation at the southern entrance of the Todmorden valley as it does at the places 1,300 or 1,400 feet high, shown in the first part of these notes, is difficult to account for, without we suppose that the land in the former case has not been raised so much as in the latter since the deposition of the drift, or, what is more probable, that the latter has been removed since.

The sections of drift now given, extending from near the sea to almost 50 miles inland, give us no data so as to enable us satisfactorily to class all the more ancient deposits found in Lancashire under an Upper and a Lower Bed of Till, divided by an intervening bed of Sand or Gravel.

Ordinary Meeting, April 2nd, 1872.

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

Mr. S. C. Trapp and Mr. G. C. Lowe were appointed Auditors of the Treasurer's Accounts.

Ordinary Meeting, April 16th, 1872.

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

Among the Donations announced were a number of MS. Journals and Papers of the late Mr. Thomas Heelis, F.R.A.S., presented by Dr. Crompton and Mr. John Heelis. On the motion of Mr. Baxendell, seconded by Professor Reynolds, it was unanimously resolved that the thanks of the Society be given to Dr. Crompton and Mr. John Heelis for their valuable donations.

The Rev. Joseph Freeston, was elected an Ordinary Member of the Society.

The President said that too much attention could not be called to the drains connecting dwelling houses with main sewers. Of course in all modern houses it is supposed that such communications are effectually trapped, so as to prevent sewage gases gaining access to the houses. However, it is to be feared many of the so called traps are traps to catch and transmit disease, and not to stop it. He had himself, at his residence in Crumpsall, a drain from a sink-stone communicating with the sewer, and for the last few years it had acted moderately well, except during sudden falls of the barometer, when smells would get into the house in spite of the traps. During the past summer a servant having found some sewage gases escaping into the yard from the eyes communicating with the sewer, trapped them. When he (the President) returned home last autumn he found the yard quite free from smells, but his house full of them, the traps in the yard having forced them inwards. No time was lost in cutting the pipes communicating with the sewer, so as to allow the refuse water to discharge itself into the open air and fall into a stench trap communicating with the sewer. This has effectually stopped all smells from sewage gases entering his house. The connection of of house drains with main sewers is no doubt a fertile source of disease, and in some cases even the means of transmitting it from house to house.

Mr. Richard Weaver, Sanitary Engineer and Chemist, 20, Nile Street, Leicester, had lately informed him that he (Mr. Weaver) had some seven months ago visited Sunderland, then suffering from a smart attack of small-pox. The sanitary officer and chairman of the Health Committee stated that the sewers had excellent ventilation. This excellent ventilation consisted of six openings into chimney stacks, for the most part at the lower extremites of sewers. Now, until the fallacy was pointed out, the responsible authorities considered six openings, promiscuously selected, sufficient for the ventilation of probably fifty miles of sewers and drains, many of them on very steep ground, and the tide flowing up twice in twenty-four hours.

Mr. Weaver found, as he expected, the epidemic most severe on the outskirts and suburbs, in places of fine situation, and open country. Here was street upon street where the sewage had spared scarcely a house; and in almost all was a more or less powerful odour of sewer gas. Now this was remarkable, and the explanation he discovered, after some trouble, although the authorities could tell him nothing of it, that many of these streets had a special sewer laid down in front of the houses, with a branch run under the floors of each building, which were filled up with ashes, and the pipe left open for the purpose of removing sub-soil water! The lower end of each sub-soil sewer joined the mains, contact being supposed to be broken by a syphon, but as these were never looked at from the day of being laid, and as no water flowed from the cellars, in dry weather the syphon speedily became untrapped, and an uninterrupted flow of gas proceeded into the houses.

A very good proof of this being the mode of propagation of the disease was furnished in one half of a street, that is one side of it, being without any drainage whatever and had not a single case of small-pox. Now here the privies and slops overflowed the yard and lane and the stench was most unbearable, yet this side escaped. Opposite, all was much cleaner to the eye, but the sewage gas was within the houses and so was the epidemic. So much for our vaunted sanitation!

Now assuming this statement of Mr. Weaver's to be true, it appears that in some cases the germs or particles of disease are communicated by drains and sewers from house to house, and that untrapped or badly trapped ones are far worse than having no drains at all.

"On a new Theory explanatory of the Phenomena exhibited by Comets," by DAVID WINSTANLEY, Esq.

An explanation of the phenomena exhibited by cometary bodies seems to have been generally sought for amongst the most hidden of nature's operations, indeed inventors of theories would appear to have taken it as an axiom that the extraordinary and imposing aspects which are frequently presented by the heavenly bodies in question can only be explained by the operation of natural laws which here we do not know, by the existence of chemical substances which here we have not got, or by the presence elsewhere of conditions which here we do not find. To me it does not seem that the causes of cometary appearances are of necessity deeply hidden, nor that the invention of new natural laws, new chemical substances or new conditions of matter offers us a more philosophical or even a more handy means of accounting for those appearances than without them we already possess.

It is undoubtedly in the presence and the configuration of their tails that we recognise the greatest visible differences from the planets which comets exhibit. But these visible differences curious and interesting as they are when present are sometimes wholly wanting, ofttimes merely rudimentary, and when existing are continually altering their dimensions and their forms. There are, however, two points in which comets constantly differ from the other members of our system, and these points are to be found in the smallness of their mass and the eccentricity of their orbital paths. It is in these ever present points of dissimilarity that I apprehend we shall find the cause of those visible, those varying, and those incidental differences from the planets, with which the term comet has become inseparably associated. It has not been observed that the smallest comets are most remarkable for their phenomena or their aspects. On the contrary the larger bodies of the class have always presented the most striking appearances, whence I infer that though these appearances are beheld only in connection with bodies of comparatively trivial mass, yet that insignificance of mass is not the primary element in the formation of the phenomena under consideration. The eccentricity of their orbits however having been a noticeable feature in connection with all the most remarkable comets, it is in this particular and the circumstances which accompany it, that I think the clue will be found to a solution of the enigma of their aspects. The most obvious difference from the planets which we might expect in the case of a comet on account of the smallness of its mass would be the feeble coercion of the elastic power of its gaseous parts and the consequent voluminous development of its atmosphere, whilst the eccentricity of its orbit would undoubtedly give rise to enormous changes in temperature of the particles composing it. It is in this extension of atmosphere and in the suddenness and violence of these thermal changes that I think it possible to find an explanation of almost every one of those appearances which are peculiar to comets as the ordinary and every day phenomena of their meteorology.

Suppose for instance we have a planetary body composed of such materials as the earth is made of and as the spectroscope indicates as entering into the composition of the sun, and suppose this planetary body to be in comparison with with our globe extremely small in mass, and located at such a distance from the sun as to be sensibly affected by his rays, say for instance within Saturn's orbit, and suppose further that it is retained at that distance until such changes as would be produced by the temperature to which it is there subjected are fully realised. We should then have a central mass of more or less solid material surrounded by an attenuated atmosphere of such substances as are gaseous at the particular temperature there prevailing and under the particular pressure exercised by the gravitation of the central mass. Now let us suppose our planetary body to be moved to another position considerably nearer to the sun, and so subjected more largely to the influence of his rays. An augmentation of its atmosphere would immediately be commenced. Materials non-volatilisable at its previous temperature would be raised into the gaseous form. The volume of its atmosphere would be increased whilst the planet's coercive power over its elasticity would be diminished. But let us suppose our planetary body to be once more replaced in its former position and subjected to the lesser of the two temperatures we have been considering. The solar heat will now no longer be able to maintain all that matter in the gaseous form which has been evaporated at the shorter of the two distances from the sun. A condensation will accordingly be commenced through a greater or less extent of the cometary atmosphere, and a more or less dense nebulous mass will surround the central stellar point. This nebulosity will be again evaporated into transparent gas upon the removal of the body it surrounds to its second position nearer to the sun. But the atmospheric condensation into cloud-like mist which follows the removal of our little planet from the influence of the solar rays would also result from the removal of those solar rays from that little planet, such for instance as would be caused by the interposition of one of the planets. Under *these* circumstances a precipitation of misty material would take place, a precipitation which would as before be dissipated at the termination of the eclipse.

A comet, however, is not circumstanced as our hypothetical planet has been. It is not placed at some given distance from the sun and allowed to remain there until the maximum thermal effect has been produced, and then removed elsewhere. It is continually altering its distance from the sun, and, apart from any axial rotation it may have, is continually presenting a fresh aspect to the operation of the solar heat. Vapourised materials issue from its heated surface in jets like steam, and rise towards the sun into the cooler atmosphere above, where they lose a portion of their heat, become partially condensed, and form a canopy of cloud, which, when viewed from the side by the inhabitants of another planet, presents the appearance of a crescent with horns turned from the sun of a hemisphere or a sphere of nebulous matter, according to the amount and aggregation of the misty particles. As the comet approaches its perihelion this misty canopy is dissipated as transparent gas into the upper and surrounding regions of its atmosphere by the ever increasing power of the sun, whilst fresh jets of steam arise from the heated surface of the central mass and replenish the stratum of clouds. It is not difficult to find an interpretation of the existence of a number of these cloudy strata floating in the comet's atmosphere in concentric rings around its central mass in the presence of atmospheric ingredients of different chemical constitution, or in supplies of vapour furnished from the same source at different intervals of time as indicated in the alternate violent action and total cessation of the steamy jets which have been observed to take place. But whilst all this is going on upon the anterior or sunward side of the comet, there is quite another state of affairs on the opposite side. There the planetary mass and its cloudy canopies project their shadows and their shades into a vast conoidal space beyond, a space in which total and partial eclipses of the sun prevail, where the influence of the solar rays is felt with mitigated force, and where, consequently, a misty precipitation is formed, which becomes illuminated in the penumbra by the direct rays of the partially eclipsed sun, and throughout its whole extent by the scattered beams which penetrate the bank of filmy clouds floating over the central planetary mass, and stretching away in a direction from the sun, forms that illuminated appendage known as the cometary tail.

It will be perceived, however, that though condensation would be commenced, where the temperature was sufficiently mitigated, throughout the whole of that coniodal space, darkened by the intervention of the planet and its clouds, yet, when once commenced, the inner particles of cloud being largely protected from further radiation by those external to them, the sum total of condensation would be almost confined to an annular space near the circumference of the shadow, in short, the misty cloud would have the form of a hollow cone, which would account for the frequently observed apparent division of the tail into two lateral branches, for this hollow envelope being oblique to the line of sight at its borders a greater depth of illuminated matter would there be exposed to the eye.

As the comet proceeds along its path it will project a newer shadow at an angle from that which it has already cast, the mist formed in which latter will be dispelled by the unimpeded action of the solar rays, whilst another portion of the comet's atmosphere will suffer partial condensation, thus causing the formation of a new tail and the dissipation of the old one to take place simultaneously, and accounting for the enormous sweep which the tail makes round the sun in perihelio in the manner of a rigid rod, and in seeming defiance of gravitation and all mechanical law.

The extent to which condensation in the cometary atmosphere will take place will obviously depend, amongst other things, on the difference of temperature within and without the shadow, and on the length of time during which that difference of temperature is allowed to operate. Now the further from the nucleus we go the fainter and the more diffuse the shadow will become; and apart from this, as well as in consequence thereof, the less the difference of temperature within and without that shade, and the longer the time required to effect a condensation. Accordingly the axis of the conoidal envelope will lag behind the axis of the shadow, the more so as we recede from the nucleus, thus producing the observed convexity on the tail's orbital preceding side.

The further we are from the nucleus, however, and for the same reason, the longer will be the time required to evaporate the mist already precipitated, and the further, therefore, will be the point at which the mist is cleared from that at which it was condensed, thus accounting for the retrograde curvature of the posterior edge of the appendage, and for the excess of this curvature over that of the opposite side.

The angular separation of the front and rear edges of the tail will clearly be regulated, amongst other things, by the angular capacity of the shadow in which that tail is formed, which increases with the comet's proximity to the sun. Accordingly we should expect this angular separation to be at its greatest in perihelio, which as a matter of fact has been observed to be the case. Particular attention was called to this phenomenon in the instance of Donati's comet in 1858, and beautiful plates illustrative of it are given in the 30th volume of the Astronomical Society's memoirs by Prof. Challis and Mr. Warren De la Rue.

The fact that the maximum length and splendour of a comet's tail is attained not at but after the passage of the perihelion is only what we might reasonably expect, for, as we know, time is required in which to produce any physical change, and consequently that augmentation of the cometary atmosphere resulting from the heat received in perihelio must necessarily be produced some time after that heat has been received, and therefore after the perihelion passage.

The diminution in size which the nucleus of a comet undergoes as it approaches the sun, and the subsequent expansion which takes place as it recedes from it, a diminution and expansion which are contemporaneous with, but reversed in order to, the dilation and contraction of the tail, follow as a corrollary to the theory I have laid down, and seem to me strongly to indicate that the tail is really a material appendage of the comet, and not an effect produced by it upon any medium through which it may be supposed to move.

It may be said in objection to my theory that comets are not made up of such chemical substances as I have instanced in the case of the hypothetical planet, to which I would reply, "Nor need they be." The theory in question only requires that they should be composed, at any rate in part, of materials evaporable by heat and whose vapours are condensible by cold, and this I think, apart from being an almost self-evident proposition, the spectroscope has shown to be a fact in the instances of the small comets examined by its aid. It indicates, as I understand, the existence of heated gaseous matter about the nucleus, and of liquid or solid material in a state of infinitesimal division in the substance of the tail.

The six-tailed comet of 1744 will, I have no doubt, be pointed to as one whose phenomena it is difficult to explain in accordance with the theory I have advanced. I would ask those who feel disposed to raise this objection to examine the evidence upon which it is affirmed that the comet in question was really possessed of a multiple tail. To my own thinking that evidence is so far from being conclusive that it would be premature to offer an explanation of the phenomenon before the appearance of another comet, unmistakably presenting the peculiarities attributed to that of 1744.

There are instances on reliable record in which comets have been known to present two tails curved in opposite directions, others in which the solitary appendage has shown no sign of curvature, and some in which two appendages have existed at the same time, but separated by a larger angle than seems consistent with the meteorological theory. These instances, however, form the small exception and not the rule, and may, moreover, be explained as merely the results of perspective.

I think I have now said sufficient to enable those who hear me to form an opinion as to whether the theory I have propounded is or not likely to prove a satisfactory explanation of some of the more striking of cometary phenomena. The theory is one which, as I take it, explains more and assumes less than is common with such theories. Besides those I have already named, there are other points which I conceive it fully to account for, but upon which it is quite impossible for me to touch in the brief space to which I feel I ought to confine my present remarks. There are points upon which I am of opinion that the application of quantities is practicable, and the theory itself I not only believe to be true, but the truth of it I conceive to be capable of numerical verification. To these and many other matters I hope to invite your attention on some other occasion, if you consider my present treatment of the subject as justifying any further expenditure of your time.

Annual Meeting, April 30th, 1872.

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

Monsieur A. Trécul, Member of the Institute of France; Professor W. P. Schimper, of the University of Strasburg; Professor Julius Sachs, of Wurtzburg; H. C. Watson, F.L.S.; Professor T. H. Huxley, F.R.S.; John Stenhouse, LL.D., F.R.S.; Professor Adolph Quetelet, of the Royal Observatory, Brussels; and the Rev. Humphrey Lloyd, D.D., F.R.S., Provost of Trinity College, Dublin, were elected Honorary Members of the Society.

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

The Council refer with pleasure to the very satisfactory condition of the Society's finances as shown by the Treasurer's account, the general balance on the 31st of March last being £340 0s. 3½d. against £287 19s. 1½d. on the 31st of March, 1871.

The number of ordinary members on the roll of the Society on the 1st of April, 1871, was 169; of these two have resigned, and one has been declared a defaulter; eight new members have since been elected, and the number on the roll on the 1st of April instant was, therefore, 174.

The Council have received from Mr. R. D. Darbishire, the Secretary of the Natural History Museum Commissioners, and Member of the Council of Owens College, a letter dated the 22nd instant, communicating the particulars of a 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.

By deed of declaration of trust, dated 29th January, 1868, the Natural History Society provided for the transfer to the Owens College, as the future Trustee of the Museum on behalf of the public and the professors and students of the College, of the Society's collections and property, upon there appearing, to the satisfaction of the interim commissioners then appointed, sufficient ground for believing that the College would be effectually enlarged, placed upon a public basis, and well housed in new buildings. When this satisfaction should have been declared, the property was to be vested in the College upon Trust for sale, and out of the proceeds the sum of £1,500 was to be payable by the Trustees of the enlarged College to Trustees to be appointed for that purpose by the Council of the Manchester Literary and Philosophical Society, on such conditions as shall be agreed upon by the same Council and the Trustees of the enlarged College (now called Governors) as will provide for the application of the said sum of £1,500 in the hands of the said Manchester Literary and Philosophical Society for the promotion of Natural History in Manchester. The commissioners met on the 10th instant, and after examining proposals received from the College for a temporary exhibition of the Museum in the new College buildings now in process of erection in Oxford Road, decided upon completing the arrangement with the College. The Trustees of the College will therefore 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 has been agreed between the Commissioners and the College that the College shall pay over £60 per annum as interest at 4 per cent on £1,500 until the principal shall have been paid over. It will be one of the first duties of the new Council to take steps in respect to this communication.

The following papers and communications have been read at the Ordinary and Sectional Meetings of the Society during the Session now closing:—

1871.

Oct. 3.—"On the High Death Rates of Manchester and Salford," by E. W. BINNEY, F.R.S., F.G.S., President.

Oct. 9.—"Notices of Several Recently-discovered and Undescribed British Mosses," by G. E. HUNT, Esq.

"Notes on Dorcatoma Bovistæ," by JOSEPH SIDEBOTHAM, F.R.A.S.

Oct. 17.—"On the Oxychlorides of Antimony," by W. CARLETON WILLIAMS, Student in the Laboratory of Owens College. Communicated by Professor H. E. ROSCOE, F.R.S.

Oct. 31.—"On the Discoveries made in the Victoria Cave," by W. BOYD DAWKINS, F.R.S.

"Note on the Chromium Oxychloride described by Herr Zettnow in Poggendorff's Annalen der Physik und Chemie, No. 6, 1871," by T. E. THORPE, F.R.S.E.

"On Aurine," by R. S. DALE, B.A., and C. SCHORLEMMER, F.R.S.

"Species Viewed Mathematically," by T. S. ALDIS, M.A.

Nov. 6.—"On *Tricophyton tonsurans*," by Mr. JOHN BARROW.

Nov. 7.—"On Changes in the Distribution of Barometric Pressure, Temperature, and Rainfall under Different Winds, during a Solar Spot Period," by JOSEPH BAXENDELL, F.R.A.S.

Nov. 14.—"On the Aurora of November 10th, 1871," by E. W. BINNEY, F.R.S., F.G.S., President.

"On the Origin of our Domestic Breeds of Cattle," by WM. BOYD DAWKINS, F.R.S.

Nov. 28.—"Encke's Comet, and the Supposed Resisting Medium," by Professor W. STANLEY JEVONS, M.A.

"On Cometary Phenomena," by Professor OSBORNE REYNOLDS, M.A.

"On the Rupture of Iron Wire by a Blow," by JOHN HOPKINSON, B.A., D.Sc.

Nov. 28.—"Observations upon the National Characteristics of Skulls," by S. M. Bradley, F.R.C.S., Lecturer on Comparative Anatomy, Royal School of Anatomy and Surgery, Manchester. Communicated by Professor H. E. Roscoe, F.R.S.

Dec. 4.—"On a Plant of *Cereus grandiflorus* (Mill)," by R. D. Darbishire, B.A., F.G.S.,

"On *Xenodochus carbonarius* (Schl.), by the Rev. J. E. Vize, M.A.

"Experiments for Eradicating *Tricophyton tonsurans*," by Mr. John Barrow.

Dec. 5.—"On the Distribution of Rainfall under Different Winds at St. Petersburg, during a Solar Spot Period," by Joseph Baxendell, F.R.A.S.

Dec. 12.—"The Illness of the Prince of Wales and its Lessons," by Edmund John Syson, L.R.C.P.E., &c.

"Account of a Remarkable Discovery of Prehistoric Relics in Ehenside or Gibb Tarn, near Braystanes Station, near St. Bees, Cumberland," by R. D. Darbishire, B.A., F.G.S.

Dec. 26.—"Remarks on Cotton and Sugar nearly a Century ago," extracted from the MS. Journal of the late Mr. George Walker, by E. W. Binney, F.R.S., F.G.S., President.

"On the Inverse or Inductive Logical Problem," by Professor W. S. Jevons, M.A.

1872.

Jan. 9.—"On a Specimen of *Stauropteris Oldhamia*," by E. W. Binney, F.R.S., F.G.S., President.

"On the Influence of Gas and Water Pipes in determining the Direction of a Discharge of Lightning," by Henry Wilde, Esq.

"Once again—the Beginning of Philosophy," by the Rev. T. P. Kirkman, M.A., F.R.S., Hon. Member of the Society.

Jan. 15.—"On Nemosoma Elongata," by Joseph Sidebotham, F.R.A.S.

Jan. 23.—"On a Crystal of Selenite from the mud dredged out of the Suez Canal," by E. W. Binney, F.R.S., F.G.S., President.

"On Mineral Wool, and on the Utilisation of Slag," by W. Brockbank, F.G.S.

"A Study of certain Tungsten Compounds, by Professor H. E. Roscoe, Ph.D., F.R.S., &c.

Feb. 5.—"On the Theories of the Origin and Spread of Typhoid Fever," by Joseph Sidebotham, F.R.A.S.

Feb. 6.—"On the Magnetic Disturbances and the Aurora of February 4th, 1872," by J. P. Joule, D.C.L., F.R.S., V.P.

"On the Aurora of February 4th," by Mr. Thomas Harrison.

"Note on the Destruction of St. Mary's Church, Crumpsall, on the 4th January, 1872, by Fire from a Lightning Discharge," by Joseph Baxendell, F.R.A.S.

"On a Group of Crystals of Calcite and Sulphide of Iron surrounding Stalactitic Bitumen," by W. Boyd Dawkins, F.R.S.

"On the Boiling Points of the Normal Paraffins and some of their Derivatives," by C. Schorlemmer, F.R.S.

Feb. 20.—"On a Specimen of *Zygopteris Lacattii*," by E. W. Binney, F.R.S., F.G.S., President.

"Experiments on the Polarization of Platina Plates by Frictional Electricity," by J. P. Joule, LL.D., F.R.S., V.P.

"On an Electrical Corona Resembling the Solar Corona," by Professor Osborne Reynolds, M.A.

"On the Electro-Dynamic Effect the Induction of Statical Electricity causes in a Moving Body. The Induction of the Sun—a probable cause of Terrestrial Magnetism," by Professor Osborne Reynolds, M.A.

Feb. 26.—"On Shells of Mollusca showing so-called Fungoid Growths," by Mr. Mark Stirrup.

Feb. 27.—"Results of Observations Registered at Eccles, on the Direction and Range of the Wind for 1869, as made by an Automatic Anemometer for Pressure and Direction," by THOMAS MACKERETH, F.R.A.S., F.M.S.

"On Black Bulb Solar Radiation Thermometers exposed in Various Media," by G. V. VERNON, F.R.A.S., F.M.S.

"Note on the Relative Velocities of Different Winds at Southport, and Eccles, near Manchester," by JOSEPH BAXENDELL, F.R.A.S.

Mar. 5.—"Further Experiments on the Rupture of Iron Wire," by JOHN HOPKINSON, B.A., D.Sc.

Mar. 19.—"Additional Notes on the Lancashire Drift Deposits," by E. W. BINNEY, F.R.S., F.G.S., President.

Apr. 16.—"On the Trapping of Sewers," by E. W. BINNEY, F.R.S., F.G.S., President.

"On a New Theory explanatory of the Phenomena Exhibited by Comets," by DAVID WINSTANLEY, Esq.

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

The Council notice with regret that the alteration made last year in the terms of admission of Sectional Associates has not yet had the effect anticipated, no increase having since taken place in the number of Associates. Nevertheless they think it desirable to continue the system of electing Sectional Associates during another year.

The Librarian reports that there has been a slight increase in the number of the societies exchanging their publications with the Society, there being at this date in

England	86	Switzerland	9
Scotland	12	Denmark	2
Ireland...............	10	Sweden...............	5
British India	8	Norway	4
Australia and Tasmania	5	Italy..................	14
		Austria & Hungary	14

Canada	5	Russia	8
United States	28	Spain	2
France and Algeria	56	Portugal	2
Germany	57	Batavia	2
Belgium	5	The Brazils & Chili.	2
Holland and Luxembourg	16	Total	352

against 249 at a corresponding period last year.

The 4th volume of the Society's 3rd series of Memoirs, as well as vols. VIII.—X. of the "Proceedings," will be distributed in the course of the summer to all the Home and Foreign Societies with whom publications are exchanged. The eleventh volume of the Proceedings has been distributed by post in numbers, as published, to all the British Societies and Honorary Members, the Council having directed this to be done at the beginning of the session, so as to give early publicity to the proceedings of the Society.

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

FROM MARCH 31ST, 1871, TO MARCH 31ST, 1872.

Dr.

	£ s. d.	£ s. d.	£ s. d.
1871. April 1.—To Balance in the Bank of Heywood Brothers and Co.		379 13 3	
,, ,, Cash in hands of Treasurer		7 0 10½	386 14 1½
,, Members on the Roll April 1st, 1871, 169 at 42/-	354 18 0		
,, Deduct Compounders 4 } ,, Ditto Members deceased 0 } ,, Ditto in Arrear March 31st, 1872 12 }	33 12 0		
		321 6 0	
,, Subscription Arrears paid up 1870-1		11 11 0	
,, 7 Members Elected in 1871, April to December	14 14 0		
,, 1 Ditto ditto January to April	1 1 0		
	15 15 0		
Less on Arrear	2 2 0	13 13 0	
,, 4 Admission Fees Arrears	8 8 0		
,, 8 Admission Fees	16 16 0		
	25 4 0		
Less on ditto in Arrear	2 2 0	23 2 0	
,, 5 Associates of Microscopical Section at 10/-		2 10 0	
,, 1 Ditto ,, Arrear		0 10 0	372 12 0
To *Sale of Publications:* Memoirs			3 14 6
To *Sundry Income:* Sectional Contributions— Microscopical		2 2 0	
Physical and Mathematical		2 2 0	
Interest Allowed by Bankers		6 2 2	10 6 2
			£773 6 9½

	£ s. d.
Compound Fund	£98 15 0
General Balance	340 0 3½
	£438 15 3½

Cr.

	£ s. d.	£ s. d.
March 31, 1872. By *Charges on Property:*		
Chief Rent	12 7 3	
Fire Insurance	3 7 6	
Inhabited House Duty	6 7 6	
Property Tax	4 5 0	26 7 3
,, *House Expenditure:*		
Water, Gas, and Coal	16 14 4	
Cleaning and Petty Expenses	4 8 10	
Repairs, &c.	1 0 0	
Tea and Coffee at Meetings	16 18 2	39 1 4
,, *Administrative Charges:*		
Wages of Keeper of Rooms	54 6 0	
Attendance on Sections	4 4 0	
Postage and Parcels	15 10 10	
Printing, Stationery, and Receipt Stamps	13 18 6	87 19 4
,, *Publishing:*		
Memoirs, Printing, and Engraving	61 17 7	
Printing Proceedings	51 2 3	
Editing Memoirs and Proceedings	50 0 0	162 19 10
,, *Library:*		
Periodicals, Binding Books, &c.	16 1 9	
Subscription to Ray Society	2 2 0	18 3 9
		334 11 6
By Balance in Bank of Heywood Brothers and Co.	439 14 9	
Less Balance due to Treasurer	0 19 5½	438 15 3½
		£773 6 9½

10*th April*, 1872.
THOMAS CARRICK, TREASURER.

Audited and found correct, April 15th, 1872.
S. CLEMENT TRAPP.
GEO. CLIFF LOWE.

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

On the motion of Mr. R. S. Dale, seconded by Mr. D. Winstanley, it was resolved unanimously:—

"That the system of electing Sectional Associates be continued during the ensuing Session."

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

President.

JAMES PRESCOTT JOULE, D.C.L., 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., &c.
Rev. WILLIAM GASKELL, M.A.

Secretaries.

HENRY ENFIELD ROSCOE, B.A., Ph.D., F.R.S., F.C.S., &c.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.

THOMAS CARRICK.

Librarian.

CHARLES BAILEY.

Other Members of the Council.

PETER SPENCE, F.C.S., M.S.A.
HENRY WILDE.
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.

"Corrections of the Nomenclature of the objects figured in a memoir 'On some of the Minute Objects found in the Mud of the Levant,' &c., published in Vol. VIII. of the Memoirs of the Literary and Philosophical Society of Manchester," by Professor W. C. WILLIAMSON, F.R.S.

"On Arsenic from Alkali Works," by H. A. SMITH, F.C.S. Communicated by Professor H. E. ROSCOE, F.R.S.

Some time ago the author laid before the Society the results of several analyses of the amounts of arsenic contained in different species of pyrites, and in several of the products in the manufacture of which the acid was employed At that time he carried his analyses as far as the carbonate of soda, in which no arsenic was found. The present paper is supplementary to the former, and he now endeavours to to show that not only does the arsenic remain in the various products of alkali manufacture but even escapes to the atmosphere.

When the salt used for the production of Hydrochloric acid is treated with Sulphuric acid, containing Arsenic, the Arsenic present becomes converted into the trichloride. This compound is said to be completely decomposed by contact with water, so that, after passing along with Hydrochloric acid gas through the condensing towers, it would scarcely be expected that any traces of the Arsenic originally present would be found in the escaping gas. The author finds this, however, to be the case. A considerable quantity of the Arsenic trichloride escapes the action of the water in the condensing towers, and passes, along with a very small proportion of the Hydrochloric acid gas, to the chimney.

A deposit found in the flue, about 20 feet long, leading from the saltcake furnace to the condensing towers; the coke contained in the towers themselves; the gas in the flue leading to the chimney; and the smoke escaping to the chimney were all submitted to analysis, and were all found to contain arsenic.

The results are gathered together in the following tables :—

TABLE I.

Deposit in Flue leading from Salt-Cake furnace to Condensing Tower.

	Arsenic Trioxide per cent.
Mean of 9 Analyses..........................	= 43·434

The total numbers in this case were found to agree very closely, varying only from 39 per cent to 47.7 per cent. This Flue had been working for some years.

TABLE II.

Coke.

From Condensing Towers.

	Arsenic Trioxide per cent.
Mean of 3 Analyses	= 2·886

In this case 10 lbs. of coke was used for each analysis, and was digested well, first with distilled water and then with pure Hydrochloric Acid. The towers had been in use for about a year.

TABLE III.

Air in Flue.

Leading from Condensing Tower to Chimney.

Amount of air taken for each analysis = 500 cubic feet,
Amount of air passing = 31,722 cubic feet per hour.
The mean of 12 analyses is here given.

Arsenic Trioxide per 1,000 cubic feet. grains.		Arsenic Trioxide per hour. grains.		Arsenic Trioxide per day. grains.
0·158	....	5·012	....	115·134

The arsenic will probably escape either as Arsenious Acid or as Arsenic Trichloride. If as the latter, it may be decomposed on coming in contact with the atmopheric moisture into Arsenious and Hydrochloric Acid.

$$(2Ascl_3 + 3H_2O = As_2O_3 + 6Hcl).$$

TABLE IV.

Specimens of Air.

Taken 10 feet from bottom of Chimney.

Amount of air taken for each analysis = 500 cubic feet.

	Arsenic Trioxide per 1,000 cubic feet.
Mean of 9 analyses	= 0·086.

The author did not know the amount of air passing in the chimney, so he only calculated the amount of Arsenic Trioxide in grains per 1,000 cubic feet.

The method employed for collecting the Arsenic Trioxide contained in the two last two Tables was very simple. The air was drawn through three bottles containing respectively Water, Hydrochloric Acid, and Nitrate of Silver. The gas was allowed to bubble very slowly through the solutions. The bottles containing them were capable of holding 40 ounces and were filled about half full.

The idea of Arsenic being present in the atmosphere surrounding chemical works is by no means new. The fact of its existence in large amounts in the ore from which the greater proportion of our vitriol is made leads one to suppose that it must find its way into the atmosphere at one place or another, but the author believes that this is the first time the comparative amounts have been brought forward.

"On Animal Life in Water containing Free Acids," by H. A. SMITH, F.C.S. Communicated by Professor ROSCOE, F.R.S.

At a time when so much is being written concerning animal life, its origin, and the conditions under which it can exist, it was thought it might be interesting to find out to what extent it was influenced by the presence of free acid in the water in which it existed, and also to see to what extent free acid prevented its origination.

The animals upon which the experiments were tried were the rotifers (rotifer vulgaris).

A certain amount of air was washed with distilled water and life allowed to originate in the solution, so that it could be seen at once what influence the amount of acid usually found in air had upon the life.

As a rule it required five days to bring the rotifers to perfection. The method of experiment was very simple. After animal life had been procured in the solution a known amount of the various acids used was then added, and allowed to stand one day, this was repeated till enough had been added to destroy life.

The results of these experiments are embodied in the following tables :—

TABLE I.

SULPHURIC ACID ADDED.

Time allowed to stand. Days.	Total Acidity. Grms. per Litre.	Remarks.
5	0·065	Animal life very abundant. Rotifers in very active condition.
6	0·084	No perceptible difference in appearance of life.
7	0·097	Brownish shade evident in water. Want of clearness in portion examined. Small 'clots' of vegetable matter visible. Rotifers languid, seemingly disinclined to move.
8	0·153	Life continued for about an hour, all traces then disappeared. The water presented the appearance of being filled with decomposing and decaying organic matter, which was floating about in 'shreds.'

TABLE II.

HYDROCHLORIC ACID ADDED.

Time allowed to stand. Days.	Total Acidity. Grms. per Litre.	Remarks.
5	0·0085	Same as in Table I.
6	0·0109	No perceptible difference in the appearance of solution.
7	0·018	No difference observable.
8	0·019	Life almost immediately extinct. Fluid still clear. Bodies of rotifers seen floating in it, but of a dull opal-like colour, and being rapidly acted upon by the acid, seemingly becoming "shredded."

TABLE III.

SULPHUROUS ACID ADDED.

Time allowed to stand. Days.	Total Acidity. Grms. per Litre.	Remarks.
5	......	Life very abundant.
6	0·002	Rotifers more active, causing great disturbance in liquid.
7	0·004	Life sluggish. Rotifers not inclined to move.
8	0·01	After 3 hours all life extinct. No obvious action on the bodies of animals.

It is very interesting to compare these three tables. The order of deleterious influence on animal life being first

Sulphuric, then Hydrochloric and Sulphurous acids in order, the action of the two latter being much more distinctly marked than the action of the former.

In making observations on the amount of free acid required to prevent origination of life it is found that the order of acid is the same as above, but that the line is much more sharply drawn.

TABLE IV.

Experiments on the amount of Free Acid contained in Water in which Animal Life can originate.

SULPHURIC ACID ADDED.

Time allowed to stand. Days.	Total Acidity. Grms. per Litre.	Remarks.
8	0·070	Life abundant.
20	0·074	Little or no life.
26	0·080	No life.

TABLE V.

HYDROCHLORIC ACID ADDED.

Time allowed to stand. Days.	Total Acidity. Grms. per Litre.	Remarks.
5	0·0085	Life abundant.
8	0·009	No life.

Water acidified with 0·0025 grms. Sulphurous acid per litre was allowed to stand exactly under the same conditions as the former to see if life could originate in water containing that amount of acidity, but after standing twenty-one days no life was visible.

It is interesting to notice in these last two tables, and the remark on Sulphurous acid, the sharp line of demarkation between the amount of acid contained in water in which life can originate and that which totally prevents origination.

In the case of Sulphuric acid we find that the small amount of 0·010 grms. per litre in addition to the ordinary acidity completely prevents it, whilst, in the case of Hydrochloric acid, 0·005 grms. per litre is sufficient. In the case of Sulphurous acid the author could not get life to originate in water containing any of that acid.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 26th, 1872.

Joseph Baxendell, F.R.A.S., President of the Section, in the Chair.

The following gentlemen were elected officers of the Section for the ensuing year: —

President.
JOSEPH BAXENDELL, F.R.A.S.

Vice-Presidents.
E. W. BINNEY, F.R.S., F.G.S. ALFRED BROTHERS, F.R.A.S.

Secretary.
G. V. VERNON, F.R.A.S., F.M.S.

Treasurer.
THOMAS CARRICK.

April 23rd, 1872.

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

"Results of Rain Gauge Observations made at Eccles, near Manchester, during the year 1871," by Thomas Mackereth, F.R.A.S., F.M.S.

The rainfall of the past year, as will be seen from a table presented below, has several peculiarities. The first is that for the first six months of the year the rainfall was in the respective months alternately below and above the average fall. April usually has the least rainfall, but for this year the fall is one of the heaviest. The second peculiarity is that the rainfall was above the average to the end of September, and below it to the end of the year, so far below it as to leave the total rainfall for the year below the average more than an inch. The number of days of rainfall in the first three months of the year was far below the average, but the number of wet days of the summer months almost as much exceeded the average. The summer therefore may be properly characterised as a thoroughly wet one. This had a very injurious effect upon fruit. Through the amount of cloud and moisture present in the atmosphere the sun's rays were deprived of the heating power they usually exercise.

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

Quarterly Periods. Average of 11 Years.	Quarterly Periods. 1871.	1871.	Fall in Inches.	Average of 11 Years	Differences.	Quarterly Periods. Average of 11 Years.	Quarterly Periods. 1871.
Days	Days.						
		January	1·410	2·566	—1·156		
50	33	February	2·927	2·350	+0·577	7·365	5·668
		March	1·331	2·449	—1·118		
		April	3·637	2·120	+1·517		
45	49	May	1·982	2·046	—0·064	6·657	9·053
		June	3·434	2·491	+0·943		
		July	3·428	2·630	+0·798		
51	57	August	1·934	3·002	—1·068	9·653	9·713
		September	4·351	4·021	+0·330		
		October	4·729	4·231	+0·498		
55	51	November	1·519	3·179	—1·660	10·594	8·727
		December	2·479	3·184	—0·705		
201	190		33·161	34·269	—1·108		

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. I have measured rainfall at these times from a gauge with a 5in. square receiver and 3ft. from the ground, now for four years, and heretofore I have found that the night fall almost regularly exceeded the day fall during the winter months. This year only two of those months show an excess of night fall over the day. During last summer the excess of the day fall over that of the night affords additional evidence of the cause of the cold wet summer we experienced last year. The excess of the day fall over the night, and that too chiefly in the spring and summer months, was 4·136 inches. The greatest day falls occurred in April and July.

1871.	Rainfall from 8 p.m. to 8 a.m	Rainfall from 8 p.m. to 8 a.m	Difference between Night and Day Fall.
January	0·863	0·534	—0·329
February	1·262	1·700	+0·438
March	0·938	0·388	—0·550
April	2·208	1·365	—0·843
May	1·235	0·730	—0·505
June	1-594	1·749	+0·155
July	2·043	1·312	—0·731
August	1·298	0·624	—0·674
September	2·137	2·134	—0·003
October..............	2‘603	2·071	—0·532
November	1·043	0·471	—0·572
December............	1·193	1·203	+0·010
	18·417	14·281	—4·136

In the next table I present the average day and night fall for four years. This table shows as previous ones have done, that on an average the night fall exceeds that of the day in the coldest months of the year without exception. There is another noticeable feature in this average result that appeared in the three years' average, namely, that the maximum of greatest night fall happens in February and again in December. Curious enough, too, in both the three and the four years' averages June and August have an excess in the night rainfall.

Average of Four Years from 1868 to 1871.

	Rainfall from 8a.m. to 8p.m	Rainfall from 8p.m. to 8a.m	Difference between Night and Day Fall.
January	1·357	1·383	+0·026
February	0·963	1·526	+0·563
March	1·154	1·042	—0·112
April	1·358	0·963	—0·395
May	1·192	0·478	—0·714
June	0·813	0·975	+0·162
July	0·885	0·650	—0·235
August	1·061	1·351	+0·290
September	1·836	1·831	—0·005
October	2·795	2·719	—0·076
November	1·379	1·514	+0·135
December	1·817	2·274	+0·457
	16·610	16·706	+0·096

"Rainfall at Old Trafford, Manchester, in 1871," by G. V. Vernon, F.R.A.S., F.M.S.

The total amount of rainfall in 1871 was 33·228 inches against 29·551 inches in 1870. The total amount was 2·390 inches below the average of the last 78 years. The fall occurred upon 182 days against 155 days in 1870, and upon 6 days less than the average of the last 10 years.

During the two first quarters of the year the rainfall was in excess, but considerably below the average in the last two quarters, but especially so in the last quarter.

January, February, April, July, September, and October, had a rainfall in excess of the average of 78 years. The excess in April was remarkable, this month having the smallest mean rainfall, but last year the excess was fully 75 per cent.

March, May, June, August, and November, had a rainfall below the 78 years' average. The falls for August and November were unusually small, the fall for August not reaching one half its usual average, and that for November being deficient of about two thirds its usual amount.

In a table annexed I have tabulated the days upon which rain fell during the last ten years, and the figures show that it by no means follows that the months in which the least rain falls have the fewest wet days. Beginning with the month in which rain falls upon the fewest days, we have the following order: May, July, March, April, June, November, August, February, January, September, December, October. April, in which the least rain falls, comes fourth instead of first; November, the wettest month except October, comes sixth, evidently showing very heavy falls on fewer days; August and February come next one another, although the former month has about half as much rain again; December and October are nearly equal, the latter—the wet month of the year—carrying off the palm as regards the number of days on which rain falls. The number of days on which rain falls is a very important one, as floods are often caused by heavy rainfall falling continuously over a few days during a comparatively dry month. August and November would be evidently months in which to look for floods, from the fact that with a rainfall not far below that of October, rain falls on much fewer days; this remark refers especially to November.

Looking at the annual number of days on which rain falls here, viz. a ten years' average of 188 days out of the 365, it appears that we have rain on rather more than half the days of the year.

OLD TRAFFORD, MANCHESTER.

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

Quarterly Periods.		1871.	Fall in Inches.	Average of 78 Years.	Difference.	No. of Days Rainfall in 1871.	Quarterly Periods.		Difference.
1870.	1871.						78 Years.	1871.	
41	38	Jan. ..	3·300	2·515	+0·785	13	7·204	7·588	+0·384
		Feb. ..	2·732	2·401	+0·331	17			
		March.	1·556	2·288	—0·732	8			
35	44	April. .	3·517	2·050	+1·467	21	7·164	8·255	+1·091
		May .	2·075	2·303	—0·228	8			
		June. .	2·663	2·811	—0·148	15			
32	52	July . .	3·546	3·505	+0·041	25	10·285	8·967	—1·318
		August	1·600	3·510	—1·910	11			
		Sept. . .	3·821	3·270	+0·551	16			
47	48	Oct. ..	4·514	3·885	+0·629	18	10·965	8·418	—2·547
		Nov. . .	1·407	3·784	—2·377	10			
		Dec. ..	2·497	3·296	—0·799	20			
155	182		33·228	35·618	—2·390	182	35·618	33·228	+2·390

DAYS ON WHICH RAIN FELL, 1862—1871:

Month.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	Means.
January ..	17	17	12	18	22	15	21	21	18	13	17·4
February..	10	16	13	17	20	12	18	21	14	17	15·8
March	18	12	13	13	17	15	18	10	9	8	13·3
April	19	17	9	8	7	25	16	14	10	21	14·6
May	20	14	10	21	9	9	9	17	9	8	12·6
June......	22	22	20	7	16	13	8	9	16	15	14·8
July	21	7	9	15	13	15	6	8	10	25	12·9
August ..	12	25	15	18	26	16	14	10	8	11	15·5
September.	18	24	21	3	28	19	10	26	14	16	17·9
October ..	23	22	13	20	13	22	24	19	23	18	19·7
November.	14	18	19	16	20	8	15	22	12	10	15·4
December.	24	21	17	8	23	19	29	20	12	20	19·3
Total ..	218	215	171	153	214	188	188	197	155	182	188·1

it b

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Annual Meeting, May 6th, 1872.

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

The following Report of the Council, and Treasurer's Account for the past year, were read and passed:—

Your Council have to report that the following papers have been read during the past session:

1871.

Oct. 9.—"Notices of several recently discovered and undescribed British Mosses."—Mr. G. E. HUNT.

"Notes on *Dorcatoma bovistæ.*"—Mr. JOSEPH SIDEBOTHAM, F.R.A.S.

Nov. 6.—"On *Tricophyton tonsurans.*"—Mr. JOHN BARROW.

Dec. 4.—"The flowering of *Cereus grandiflorus.*"—Mr. R. D. DARBISHIRE, B.A., F.G.S.

"On the occurrence of *Xenodochus carbonarius*, Schl., near Welshpool."—Rev. J. E. VIZE, M.A.

"Further Notes on *Tricophyton tonsurans.*"—Mr. JOHN BARROW.

1872.

Jan. 15.—"On *Nemosoma elongata.*"—Mr. J. SIDEBOTHAM, F.R.A.S.

Feb. 5.—"The Origin and Spread of Typhus Fever."—Mr. J. SIDEBOTHAM, F.R.A.S.

26.—"On Shells of Mollusca, showing Interior Traces of Fungoid Growth."—Mr. MARK STIRRUP.

The number of Ordinary Members of the Section is 38, and of Associates 12.

The funds of the Society, as will be seen from the accompanying balance sheet, are in a satisfactory state.

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

1871.	£	s.	d.	1871.	£	s.	d.
To half cost of Linnean Transactions	10	10	0	By Balance	34	3	0
,, Parent Society for use of Rooms.........	2	2	0	,, Subscriptions	24	10	0
,, W. Roscoe, for Teas.......................	4	6	6	,, Interest from Bank	0	10	4
,, Chas. Simms & Co., Printing Circulars..	4	2	0				
,, J. E. Cornish, Microscopical Journal ...	0	16	0				
,, Balance..........	37	6	10				
	£59	3	4		£59	3	4

Examined and found correct,

(Signed) Samuel Cottam.
A. Brothers.

1872.

May 4th.—By Balance......................£37 6 10

The Election of Officers for the Session 1872–3 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.
CHARLES BAILEY.

Treasurer:

HENRY ALEXANDER HURST.

Secretary:

SPENCER H. BICKHAM, JUNR.

Of the Council:

HENRY SIMPSON, M.D.
JOHN BARROW.
W. BOYD DAWKINS, F.G.S., F.R.S.,
THOMAS COWARD.
ROBERT B. SMART.
WALTER MORRIS.
ALFRED BROTHERS, F.R.A.S.

The following is the list of Members and Associates:

List of Members.

Alcock, Thomas, M.D.
Bailey, Charles.
Barrow, John.
Baxendell, Joseph, F.R.A.S.
Bickham, Spencer H., Jun.
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.
Lynde, James Gascoine, Mem. Inst. C.E., F.G.S., F.R.M.S.
Maclure, John Wm., F.R.G.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.

Bradbury, C. J.
Hardy, John.
Hunt, G. E.
Hunt, John.
Labrey, B. B.
Linton, James.
Meyer, Adolph.
Peace, Thos. S.
Plant, John, F.G.S.
Ruspini, F. O.
Stirrup, Mark.
Waterhouse, J. Crewdson.

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

VOL. XII.

SESSION 1872—73.

MANCHESTER:

PRINTED BY THOS. SOWLER AND SONS, RED LION STREET, ST. ANN'S SQUARE.

LONDON: H. BAILLIÈRE, 219, REGENT STREET.

1873.

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.

INDEX.

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

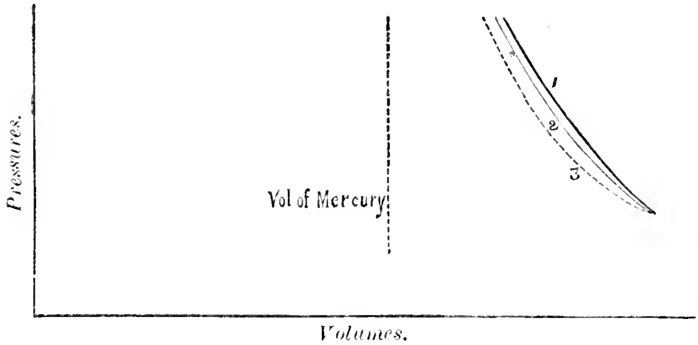
A new observation on the amalgam has recently been made in America by Professor C. A. Seeley,* who found, by subjecting it to varying pressure that its volume changes, apparently in accordance with Mariotte's law. He employed simply a glass tube fitted with a plunger, and did not measure the pressures 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 Limekiln-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 ex a-vating 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	13 to 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.)
Length of Chamber in feet.	10	20	30	40	50	60	70	80	90	100	110	120	130	140	

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 44½ 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, Baeda'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 occured 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.

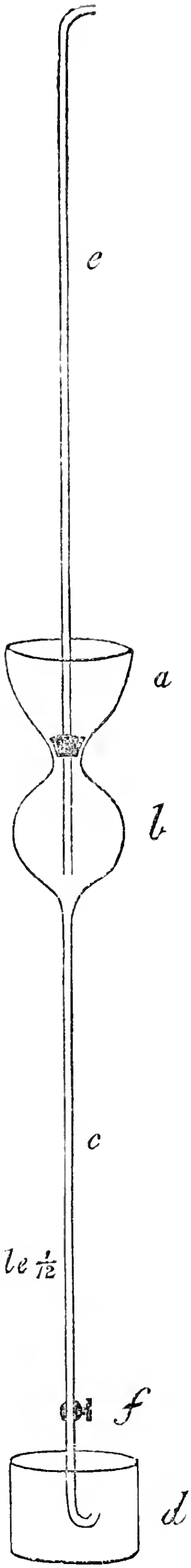
If after immersion for say ten minutes in either of these acids a piece of iron or steel be tested, its tensile strength and resistance to torsion will be found to have diminished. Exposure to the air for several days or gentle heat will however completely restore its original strength. On breaking a 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 verticcllate 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 metavanadic 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."

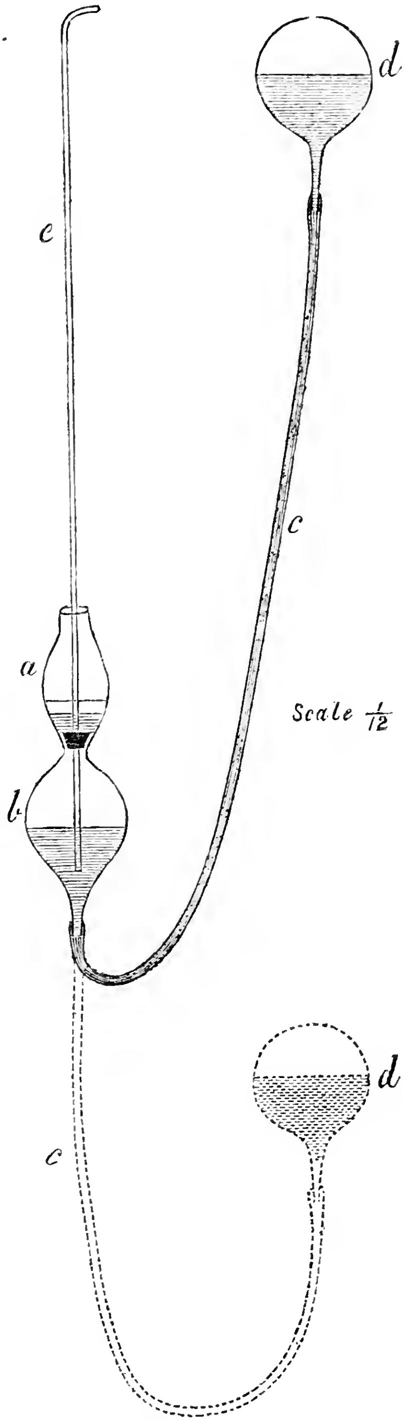
The exhauster described in the last number of the Proceedings has been further improved by dispensing with the glass tube *e*, and its stop-cock *f*. This is effected by attaching the base of the globe *b* to a strengthened indiarubber pipe, connected at the other end to a glass vessel of rather larger 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.

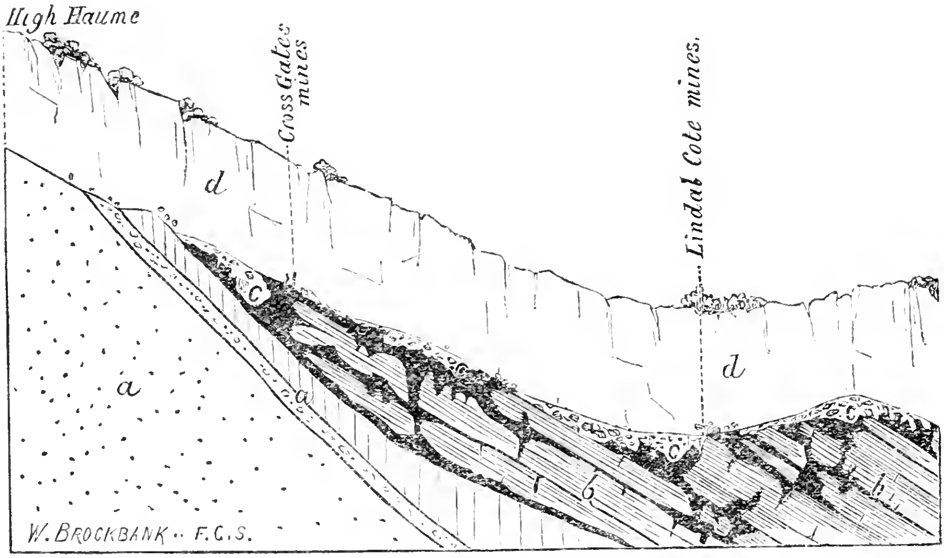
The occurrence of the superficial deposits, as shown in the foregoing section, is, I believe, to be explained by the theory of glacial action, and is evidently a part of the great change wrought upon the surface, by the agency of ice, during the "glacial epoch"; coeval with the boulder drift. The great 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.

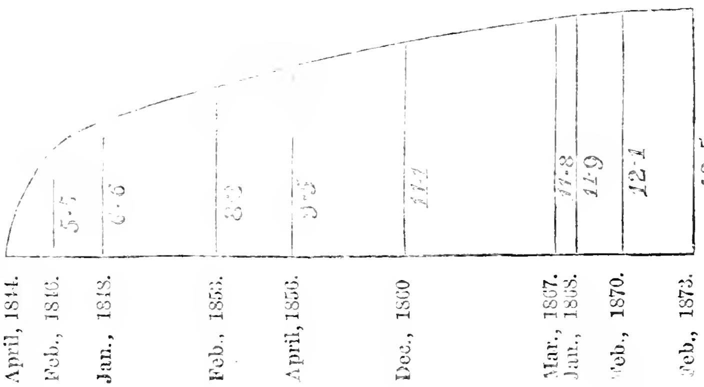
E. W. BINNEY, V.P., F.R.S., said that shortly after the meeting of the Society on the 21st January last, when he exhibited the singular fossil plants, which were quite new to him at the time, and which he thought would have to be placed in a new genus, he had received excellent transverse and longitudinal 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 $\frac{1}{12 \cdot 5}$ 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.	50·50990	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	241·31005	·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......	·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 facsimile 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 of 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,
		January ..	4·255	2·537	+1·718	22			
38	56	February..	3·018	2·409	+0·609	18	7·240	10·048	+2·808
		March	2·775	2·294	+0·481	16			
		April......	2·975	2·062	+0·913	9			
44	50	May	2·145	2·301	−0·156	17	7·226	12·020	+4·794
		June	6·900	2·863	+4·037	24			
		July	7·658	3·557	+4·101	17			
52	59	August....	2·784	3·501	−0·717	19	10·376	17·480	+7·104
		September	7·038	3·318	+3·720	23			
		October ..	4·404	3·891	+0·513	22			
48	63	November..	3·774	3·784	−0·014	21	10·967	11·144	+0·177
		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.

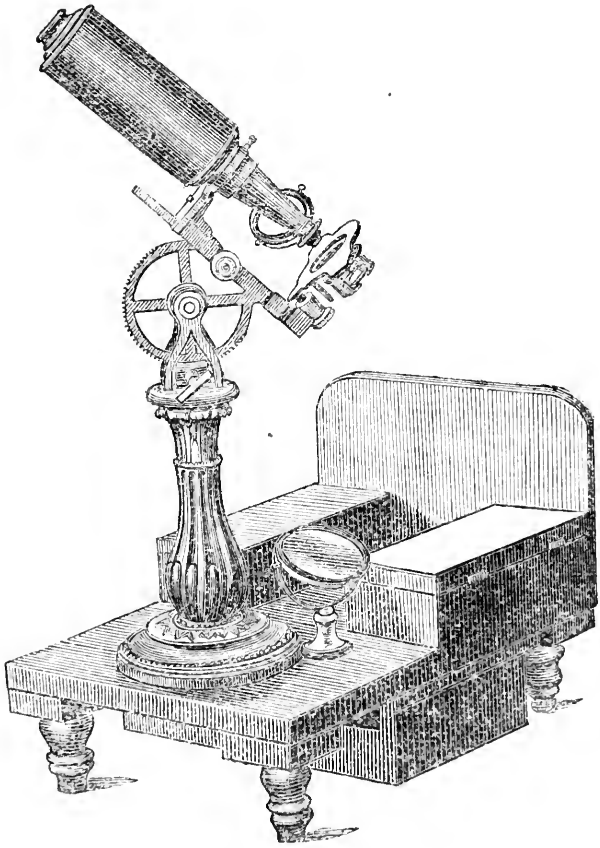
JOSEPH SIDEBOTHAM, F.R.A.S., in the Chair.

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 1*st*, 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.**

			£ 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

		£ 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.		Differences.
				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.	
	1872.	1872.				
January..	4·096	3·996	+·100	2·823	2·805	+·018
February.	2·849	2·711	+·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.

The following is the list of Members and Associates:

List of Members.

Alcock, Thomas, M.D.
Bailey, Charles.
Barrow, John.
Baxendell, Joseph, F.R.A.S.
Bickham, Spencer H., Jun.
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.

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