PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

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NOTE.

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

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PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 7th, 1873.

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

Mr. Samuel Broughton was elected Treasurer of the Society in place of the late Mr. Thomas Carrick.

"Atmospheric Refraction and the last rays of the Setting Sun," by DAVID WINSTANLEY, Esq.

It is recorded in the Proceedings of this Society that a letter dated from Southport and written by Dr. Joule was read at the meeting held on the 5th October, 1869. In that letter it is remarked that "Mr. Baxendell noticed the fact that at the moment of the departure of the sun below the horizon the last glimpse is coloured bluish green." Dr. Joule also observes that on two or three occasions he had himself noticed the phenomenon in question and that "just at the upper edge where bands of the sun's disc are separated one after the other by refraction, each band becomes coloured blue just before it vanishes."

During the past eighteen months the writer, from his residence in Blackpool, has had frequent opportunities of observing the setting sun, and has noticed the phenomenon of the final coloured ray certainly more than fifty times. To the naked eye its appearance has generally been that of a green spark of large size and great intensity, very similar to one of the effects seen when the sun shines upon a well cut diamond. The colour however is by no means constant, being often, as in the case of Mr. Baxendell's observation, bluish green, and at times as mentioned by Dr. Joule, quite blue. The period of its duration too is likewise variable. Sometimes it lasts but half a second, ordinarily perhaps a second and a quarter, and occasionally as much as two seconds and a half.

When examined with the assistance of a telescope it becomes evident that the green ray results at a certain stage of the solar obscuration, for it begins at the points or cusps of the visible segment of the sun, and when the "setting" is nearly complete extends from both cusps to the central space between, where it produces the momentary and intense spark of coloured light visible to the unaided eye.

From the fact of the green cusps being rounded I apprehend that irradiation contributes to the apparent magnitude of what is seen. The range of colour too as seen in the telescope is more varied, and the duration of the whole phenomenon more extended, than when the observation is made only with the naked eye.

Of the objective nature of the phenomenon it is needless to offer evidence; for it needs to be but seldom seen to preclude the idea of an optical illusion. That the waters of the ocean have nothing to do with the production of the colour is made manifest by its visibility when the sun "sets" behind the edge of a well defined cloud. On the 14th and 15th of June, for instance, it was seen at upper contact of the solar limb with clouds. On the earlier date in question a thin band of cloud stretched across the setting sun, and under a power of fifteen diameters the green effect was seen at upper contact with the cloud and again at final disappearance below the horizon. On the later date it was again seen at upper contact with each of several filaments of cloud and again at final disappearance. And on several other occasions the writer has observed the effect when the disappearance of the sun has taken place at an elevation of six or eight degrees behind a heavy bank of clouds.

Respecting the increased range of colours seen when the phenomenon is observed with telescopic aid, I may mention that on the 28th of June the sea was calm and the sky quite cloudless at the setting of the sun. Of the final coloured rays fifteen diameters showed the first to be a full and splendid yellow, which was speedily followed by the usual green, and then for a second and a half by a full and perfect blue. Respecting the increased duration of the colour I have found that when the atmosphere is sufficiently favourable to allow a power of 60 diameters being employed with a three-inch object glass, the green effect is seen at that part of the sun's limb in contact with the horizon even when one half the sun is still unset, and of course from then till final disappearance.

The different colours seen, together with the order of their appearance, are suggestive of the prismatic action of the atmosphere as the cause of their production, and the interception of the horizon or the cloud as the cause of their separation.

Assuming the correctness of this view, it becomes evident that an artificial horizon would prove equally efficacious in separating the coloured bands, and also that if employed during an inspection of the sun's lower limb, the least refrangible end of the spectrum would be disclosed. Accordingly I introduced into an eyepiece of my telescope a blackened disc of metallic copper, having a slit cut in it of about the one hundred and fiftieth of an inch in width, and proceeded to make an observation in July when the sun was about one half of its meridian height. The blinding glare however of that portion of the sun seen through the slit rendered the observation futile. By projecting a large image of the sun into a darkened room I was enabled to get the whole of the spectrum produced by the prismatic action of the atmosphere in a very satisfactory manner. In this case a semicircular diaphram was used, so placed that its straight edge divided the field of view into equal parts, from one of which it obscured the light. The diaphram was placed as before in the focus of the eyepiece, and by rotating it every portion of the sun's limb could be in turn examined, and that too in the centre of the field, so as to be equally subjected to the minimum of the peculiarities of the instrument. When the sun's lower limb was allowed to descend into the field of view the first rays were intensely red. After a momentary duration they gave place in succession to orange, yellow, and green, which were then lost in the ordinary refulgence of the sun. The upper limb gave green, blue, and finally purple, which latter colour I have thus far never seen upon the natural horizon. It should be remarked that the colours seen were vivid and unmistakeable, and each one of them easily detained at will or the whole phenomenon recalled by the adjusting screws of the instrument. I apprehend that the results here given sufficiently prove that atmospheric refraction is the cause of the coloured rays seen at the moment of the sun's departure below the horizon. I have however thought it worth while to examine the light proceeding from the moon's limb by the aid of the artificial horizon and of course by direct observation. The results were decisive and satisfactory, the spectral colours being easily observed. The green effect I have also frequently seen on the departure of the moon beneath the edge of a dark and well defined bank of clouds. Telescopic aid has however in every instance been required.

The rapid changes in colour observable in the case of almost any large fixed star at an elevation of twenty or thirty degrees above the horizon, and which changes vary between red, green, and blue, may I think be fairly attributed to the same cause as the colour in the sun's final ray. Particles of dust floating in the air act, I apprehend, for the moment in the capacity of diaphram or horizon, and thus enable the eye to perceive even in the light of the stars the prismatic action of our atmosphere.

Ordinary Meeting, October 21st, 1873.

EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S., Vice-President, in the Chair.

W. BOYD DAWKINS, F.R.S., exhibited a fragment of a post struck by lightning on 2nd June, 1873. It formed one of three about 8 feet high and 15 feet apart in the garden of 11, Norma Road, Rusholme, and stood under a cherry tree of which the stem was 10 feet away. It was completely shattered, fragments being driven as far as the walls of the house, 25 yards off, and the downward direction of the loose splinters implied that the explosive force was exerted from below upwards, instead of from above downwards. People in the Dickenson Road observed what they termed a "thunderbolt" fall, as they thought, on the house, and some of the inhabitants describe it as a flame of light followed immediately by a crash of thunder. It is very probable that the explosion was produced by an electric current passing from the earth upwards, and not *vice versa*.

Professor REYNOLDS attributed the shattering of the post to the explosive or repulsive action of an electrical discharge of unusual intensity.

Mr. BAXENDELL thought it was most probably due to the sudden conversion of a portion of the moisture in the post into steam of high tension by the heating action of the electrical discharge, and mentioned instances in which condensed vapour was said to have been seen rising from trees immediately after they had been struck by lightning.

"On the Relative Work spent in Friction in giving Rotation to Shot from Guns rifled with an Increasing, and a Uniform Twist," by OSBORNE REYNOLDS, M.A., Professor of Engineering, Owens College, Manchester, and Fellow of Queens' College, Cambridge.

The object of this paper is to show that the friction between the studs and the grooves necessary to give rotation to the shot *consumes more work with an increasing* *than with a uniform twist;* and that in the case of grooves which develope into parabolas, such as those used in the Woolwich guns, the waste from this cause is double what it would be if the twist was uniform. I am not aware that this fact has ever been noticed. It must not be confounded with the questions already at issue respecting the Woolwich or French system of rifling guns. The advocates of the gradually increasing twist maintain that it relieves the pressure between the studs and the grooves at the breech of the gun, where it would otherwise be greatest, while the opponents argue that in order to obtain this otherwise advantageous result, the bearing surface of the studs has to be so much reduced that they are not so well able to withstand the reduced pressure as they are to withstand the full pressure with the plane grooves. Now I bring forward a collateral point, which has no bearing on the previous question, but which is, in itself, of sufficient importance to influence the decision in favour of one or other of these systems. I show that apart from any undue wedging or shearing of the studs, that with nothing but the legitimate friction, the amount of work wasted in imparting rotation to the shot is nearly twice as great with the parabolic as with the plane grooves. This is important, for, although the magnitude of this waste does not appear as yet to have been the subject of direct inquiry, it will be seen from what follows that with the plane grooves it amounts to more than one per cent of the whole energy of the shot, and, consequently, with the parabolic grooves it will amount to two per cent of the energy of the shot; this is, to say the least, important as regards the effect of the discharge; and when we consider that all the work spent in friction is spent in destroying the gun and the shot, we see that it becomes a matter of the very greatest importance whether the gun spends one or two per cent of its power on self-destruction. It was established as a fact in the trials of 1863–5, that the guns with an increasing twist gave a lower velocity than those with the uniform twist. In the trial with the two seven-inch guns made especially to test this point, the difference of velocity was such as to make three per cent difference in the energy of discharge—a result somewhat greater than what would have been due to the legitimate friction, unless the coefficient of friction between the studs and the grooves was excessively high from some cause, such as the cutting of the studs into the grooves. However, it would seem that the conclusions at which I have arrived are in accordance with actual experience, and help to explain what was otherwise to a certain extent anomalous.

Although these conclusions cannot be definitely proved without the aid of mathematics, they may be shown to be true (or reasonable) under certain circumstances, as follows:

The work spent in friction will, both with the parabolic and plane grooves, be equal to the coefficient of friction multiplied by the mean pressure on the studs and again by the length of the grooves (or by the length of the gun—nearly). Now, the coefficient of friction and the length of the gun are the same in both cases; hence this work will be proportional to the mean pressure on the grooves throughout the gun. Again, if the pressure on the parabolic grooves is constant (which it is the object of these grooves to make it), then the mean pressure in both cases will be inversely proportional to the angle which the shot turns through while in the gun. This follows directly from the fact that the speed and consequently the energy of rotation with which the shot leaves the gun is the same in both cases; for this energy is nearly equal to the mean pressure multiplied by the arc through which the studs turn,* and hence the mean pressure is equal to the energy divided by the arc.

We have then the work spent in friction proportional to the mean pressure; and the mean pressure inversely proportional to the angle turned through by the shot in the gun; therefore *the work spent in friction is inversely proportional to the angle turned through by the shot in the gun.*

Now, the angle turned through with parabolic grooves is

* This is always true for plane grooves, but it will only be true for parabolic grooves when the pressure on the studs is constant all along the grooves.

half the angle turned through with plane grooves (by a property of the parabola); hence the work spent in friction with the parabolic grooves is double what it is with the plane grooves. This may be shown mathematically as follows:—

I. *To estimate the actual work spent in friction with plane grooves.*

Let μ = coefficient of friction.
i = the inclination of the grooves.
K = work spent in friction.
Σ = the energy of discharge or the striking force with which the shot leaves the gun.

$$\text{Then, } K = \frac{\mu\, i}{2} \times \Sigma.$$

For if R = the mean pressure on the grooves, l = the length of the gun, then $K = \mu R\, l \sqrt{i^2 + 1}$ . . . . (1)

$$\text{And the energy of rotation} = \frac{i^2}{2}\Sigma = \frac{R\, l\, i}{\sqrt{i^2+1}}.$$

$$\therefore \frac{i^2}{2}\Sigma = \frac{R}{\sqrt{l^2+1}} l\, i \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

$$\therefore K = \frac{\mu\, i}{2}\Sigma.$$

Hence (with a gun making one turn in 35 diameters) where $i = \frac{1}{11}$ and $\mu = \cdot 3$.

$$K = \frac{\cdot 3\,\Sigma}{22} = \cdot 013\,\Sigma.$$

The equation $K = \frac{\mu\, i}{2}\Sigma$ shows, what is otherwise quite obvious, that with the plane grooves the work spent in friction is independent of the distribution of the pressure within the gun, and is proportional only to the energy of discharge; and hence will be the same, whether the powder is quick or slow, provided the shot leave with the same velocities.

This, however, is not the case with the parabolic grooves. It is obvious that the friction will involve the law of pressure in the gun. Consequently, we cannot calculate this work unless we make some assumption with regard to the law of pressure.

II. *To estimate the actual work spent in friction with parabolic grooves when the pressure on the studs is constant.*

Let $x = \frac{y^2}{b}$ be the equation to the developed grooves, and let s be the length of the grooves. Then, if we assume that $\frac{dy}{ds} = 1$, and that K_b (the work spent in friction with the parobolic grooves) $1 = \mu R l$, we have the work of rotation

$$= \int R \frac{dx}{dy} dy$$

$$= \frac{l}{2 b \mu} K_b$$

And the work of rotation $= \frac{i^2 \Sigma}{2}$

$$\text{Since } i = \frac{l}{b},$$

$$\therefore K_b = \mu i \Sigma,$$

And for plane grooves $K = \frac{\mu i \Sigma}{2}$

$$\therefore \frac{K b}{K} = 2.$$

An expression for this work might have been obtained without assuming $\frac{dy}{ds} = 1$, but so long as i is less than $\frac{1}{10}$ the difference is very small.

Hence we see that on this assumption the work spent in friction with the parabolic grooves is twice as great as with the plane grooves. This assumption is not an unreasonable one, for the declared object of the increasing twist is that it may equalise the pressure of the studs on the grooves throughout the gun. However, it is not to be supposed that this object is always attained, for one kind of powder has a different law of force from another. It is necessary therefore to consider other laws of force. We cannot obtain a general expression which will include all, but we may examine several laws of force which will enable us to see how far the law of force affects the results.

In all cases the force diminishes from the breech to the muzzle, and the law may be roughly expressed by $P = \frac{\lambda}{a + y}$

where y is the distance of the shot from the breech, and a and λ are constants for each class of powder.

Although with this value of P the equations of motion cannot be solved rigorously, an approximate solution may be found as follows:—

III. *To find the ratio of work spent in friction with parabolic and plane grooves when the law of force is*

$$P = \frac{\lambda}{a+y}.$$

The equations of motion are

$$\frac{1}{2}\frac{d}{ds}(v^2) = P\frac{dy}{ds} - \mu R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$R = P\frac{dx}{ds} + \frac{v^2}{\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Neglecting the μ R as small in (1), and taking ρ (the radius of curvature) $= b$, we have if $\frac{dy}{ds} = 1$

$$\frac{v^2}{2} = \int P\,dy = \lambda \log. \frac{a+y}{a}.$$

$$\int R\,dy = \int P\frac{y}{b}dy + \frac{2\lambda}{b}\log.\frac{a+y}{a}$$

$$\text{or } K_b = \mu\int R\,dy = \frac{\mu\lambda}{b}\left\{(2l+a)\log.\frac{a+l}{a} - l\right\}$$

And for plane grooves ρ is infinite and $\frac{dx}{dy} = i$

$$\therefore K = \mu\int R\,dy = \mu\lambda i \log.\frac{l+a}{a}$$

$$\therefore \frac{K_b}{K} = 2 + \frac{a}{l} - \frac{1}{\log.\left(1+\frac{l}{a}\right)}$$

If $a = 0$ so that $P = \frac{\lambda}{y}$

$$\frac{K_b}{K} = 2.$$

If a is very great, so that $P = \frac{\lambda}{a}$

$$\frac{K_b}{K} = \frac{3}{2}$$

And the former law more nearly expresses the condition of most guns.

IV. *If we take a law of force.*

$$P = \frac{e^{-2\mu \tan.^{-1}\frac{y}{b}}}{b^2 + y^2}$$

the equations of motion can be solved.

And if $i = \frac{l}{b}$, $\theta = \tan.^{-1} i$.

$$\text{We get } K_b = \frac{e^{-2\mu\theta}}{2\mu b} \left\{ e^{2\mu\theta} - 1 - 2\mu\theta + \mu^2 \log.(1 + i^2) \right\}$$

$$\text{And for the plane curve } K = \frac{i}{2b} e^{-2\mu\theta} \left\{ e^{2\mu\theta} - 1 \right\}$$

$$\text{And therefore } \frac{K_b}{K} = \frac{e^{2\mu\theta} - 1 - 2\mu\theta + \mu^2 \log.(1 + i^2)}{i\left(e^{2\mu\theta} - 1\right)}$$

From which, for any given value of θ, we may obtain the actual value of $\frac{K_b}{K}$.

When θ is small, so that where we may neglect high powers without error $\frac{K_b}{K} = \frac{3}{2}$.

Which result is in exact accordance with those previously obtained, for, it must be noticed, that with this law P is nearly constant. Hence we arrive at the following conclusions :—

(1) That when the pressure of the powder is constant,

$$\frac{\text{Work spent in friction with parabolic grooves}}{\text{Work spent in friction with plane grooves.....}} = \frac{3}{2}$$

(2) That when the pressure diminishes rapidly the above ratio = 2.

(3) That this ratio may have any values between these two, but that it cannot go beyond these limits.

Mr. BAXENDELL read the following extract from a letter he had received from the PRESIDENT :—

You will see that I have put a little drying apparatus to the short limb of my syphon barometer. I believe that a long open tube attached to the short end by a bit of indiarubber tube will do just as well. This I am going to try, and also to exclude the air more perfectly than I find it is in the instrument at the Rooms. The principal fear was that the sulphuric acid would slowly act on the mercury. I think the barometer has been put up long enough to decide this, and I feel convinced that the plan will succeed.

Ordinary Meeting, November 4th, 1873.

R. Angus Smith, Ph.D., F.R.S., &c.. Vice-President, in the Chair.

Mr. Joseph R. Bridson, Mr. James Watkins, and the Rev. William Marshall, B.A., were elected Ordinary Members of the Society.

The Chairman said that the death of Dr. Calvert, one of the most distinguished members of the Society, called for more attention than he was able to give it, but this meeting of the society could not be allowed to pass over without a few words regarding the loss which all chemists must feel. It will no doubt be the pleasant duty of some of his friends to prepare a more detailed account of his labours. Meantime he (the Chairman) would express the opinion of all who knew Dr. Calvert by saying that a more diligent student of chemistry has rarely if ever been found in any country. It has been remarked that Dr. Calvert's knowledge of the literature of science was something marvellous. This was doubtless owing to his devotion to the subject and his untiring activity and strength. The memoirs which he has written are too numerous to be characterized at present, and already several journals have given the heads of the most important. As a medium of communication between scientific men, manufacturing and professional, in France and England, he was no doubt the means of doing much good in both countries. In the former country he felt almost as much at home as in the latter, having lived there from the time he was fourteen years of age until he was twenty-eight, and having married a French lady. It was this intimate connection with France which gave him his French accent, which he could never entirely get rid of. He was however entirely English by birth. His habits contracted there may have led also to that excessive activity which seems to have told at last in a very unexpected manner upon his health, as he seemed powerful and of sound constitution. The fatigues he underwent during a visit to the Vienna Exhibition, and the climate there, must, however, be blamed, so far as we can hear, for the evils both direct and indirect which produced a fatal result.

Dr. Calvert had, shortly before his death, completed a revised edition of his lectures on some departments of manufacturing chemistry. His practical experience, combined with his profound knowledge of the theory of the subject, must render such a work of great value, and keep his name for a long time fresh among chemists.

His later investigations, especially those connected with germs or the beginnings of life, were of a purely scientific character.

He may be said in every sense to have been a successful man until that illness which destroyed a life which promised to be very long. He had many friends, and amidst an excessive amount of work he was able to be obliging and kind to a large number.

He was a Fellow of the Royal and the Chemical Societies and of several foreign Academies, and in the scientific circles of London and Paris he was as well known as in his adopted city, Manchester, where many lament him as a man who knew little of him as a chemist.

"On the Bursting of Trees and Objects struck by Lightning," by Professor OSBORNE REYNOLDS, M.A.

The results of the experimeuts referred to in this Paper were exhibited to the meeting.

The suggestion thrown out by Mr. Baxendell at our last meeting—that the explosive effect of lightning is due to the conversion of moisture into steam—seemed to me to be so very probable that I was induced to try if I could not produce a similar effect experimentally.

1. I first of all tried to burst a thin slip of wood by discharging a jar through it, taking care so to arrange the wood that the discharge should be of the nature of a spark and not a continuous discharge; this was done by making the wood to form part of a discharging rod with balls on the ends.

This experiment was successful in the first attempt, although the results were on a small scale.

It should be mentioned that the wood had been damped with water.

This experiment was repeated with larger pieces of wood with various results.

2. It then occurred to me to try with a glass tube. This I did at first with a very small tube, passing wires from the ends of the tube until they were within $\frac{1}{2}$ inch of each other.

The small tubes burst both with and without water.

3. I then used a larger tube (about $\frac{1}{10}$ inch bore), using it in a similar manner. The discharge without water produced no effect on this, even when repeated several times, but when the tube was full of water (with the ends open) the first discharge shattered that part of the tube opposite the gap in the wire. This tube was bent in the form of a syphon, and the water stood about 1 inch beyond the gap in the wire on each side of it.

4. I then tried a stronger tube which I had been using for insulation. It had a bore of $\frac{1}{8}$ inch and was $\frac{3}{8}$ inch in external diameter. It was capable of sustaining a pressure of probably 10,000, and certainly 5,000 lbs. on the square inch, that is to say, a pressure of from 2 to 5 tons

per square inch. It was about 14 inches long and bent in the form of a square-ended siphon. The gap in the wire was about $\frac{1}{2}$ inch, and the water extended about $1\frac{1}{2}$ inches on each side of the gap. The ends of the pipe were open, and the jar charged in the same manner as before with about 100 turns of a 12 inch plate machine. The surface of the jar is about $\frac{1}{2}$ a square foot, and the discharge when effected with the common rod took place through about 2 inches of air.

This tube was shivered at the first discharge. That part opposite the gap and for some way beyond is completely broken up into fragments which present more the appearance of having been crushed by a hammer than of being the fragments of a pipe burst under pressure. Some of the fragments show that the interior of the pipe has been reduced to powder.

These fragments were scattered to some feet on all sides, but there was nothing like an explosion. I held the pipe in my hand at the time of the discharges, and the sensation was that of a dead blow. There was no noise beyond the ordinary crack of the discharge.

The manner in which this pipe was destroyed clearly showed that a larger one might have been broken. But as it was two o'clock and my fire was out, I did not continue the experiments.

It is not easy to conceive the precise way in which a pressure of probably more than 1,000 atmospheres could be produced and transmitted in a pipe of water the ends of which were open. It might have been caused by the sudden formation of a very minute quantity of steam, or by the expansion of the water; but whichever way it was, its effect was due to its instantaneous character, otherwise there would have been an explosion,

When we consider the great strength of this pipe (which might have been used for a gun without bursting), and when we see that it was not only burst but that the interior of the glass was actually crushed by the pressure, and all this by the discharge of one small jar, we must cease to wonder at the bursting power of a discharge from the clouds.

The Rev. W. N. Molesworth, M.A., said, that in answer to an appeal that had been made to him by the chairman, he would bring under the notice of the society some Roman and Celtic antiquities, to which he thought that sufficient attention had not been given in this country.

1. He believed that few people in Manchester were aware that on the side of Blackstone Edge, and within a little more than fourteen miles from their city, there was a Roman road, which was one of the most perfect and remarkable that existed anywhere in the world. A thin coating of earth and heather had so completely preserved it that in parts it was as fresh and new in its appearance as if it had been quite recently constructed. It was composed of two distinct ways, each just broad enough for a rather narrow cart to pass along it, so that it would seem that in the construction of this road our railway system had been anticipated, and that there was what may be called a double line, one for ascending and the other for descending carriages. These two ways were separated by stones nearly a yard wide, and in which a deep groove was cut, apparently designed for an aqueduct. At the top there was what was described in the ordnance map as a fort, but what he believed to be merely the quarry from which the stone used in the construction of the road had been obtained. Mr. Molesworth stated that he had inquired of many antiquarians both in this country and in France, but had not been able to meet with any one who had seen a Roman road that resembled that which he had described.

2. The next monument to which he wished to call attention was the Cité de Limes, or de Lenies, otherwise called

the Camp de César, about two miles to the north east of Dieppe. It is a vast Celtic camp of triangular form, capable of containing a population larger than that of the city of Manchester. It is bounded on one side by inaccessible cliffs of great height; on the second side, where it is partly defended by nature, with an earthen wall about 36 feet high; and on the third side, which is less defended by nature, with an earthen wall about 56 feet high, and as thick as a railway embankment. The two walls together are probably about three miles in length. They are supposed to have been constructed by the Celts, and the space they enclosed to have served as a place of refuge for the inhabitants of the neighbouring country, who, when invaded, probably fled into, taking with them their families and all their moveable possessions. Mr. M. stated that he had seen a great number of flint implements that had been found within the enclosure. There is a very good model of it in the museum of Dieppe. There are two similar monuments in Normandy: one at Caudebec, on the Seine. It is in the form of a very long isoceles triangle, the two sides of which are inaccessible cliffs, and the base protected by a wall similar to that just described.

3. Mr. Molesworth also referred to the vitrified camp which exists in the neighbourhood of St. Brieux in Brittany. This camp is enclosed by a wall of granite, which has been completely calcined by the action of fire. It was supposed by the Archæological section of the Congrès Scientifique of France who visited it that it had been occupied by persons whose conduct had rendered them so obnoxious to the inhabitants of the country that they were determined to destroy their camp so completely as to prevent it from affording them shelter at any future time. It was very extraordinary that in these times in which the fort was thus burnt heat could have been obtained sufficiently intense to destroy the nature of the granite. In answer to an inquiry as to

whether the effects described might not have been produced by long exposure, Mr. M. stated that he believed that the French archæologists who visited it were unanimously of opinion that it had been acted on by fire. There were also some vitrified camps of a similar character in the north of Scotland.

Dr. Angus Smith said that he could certainly confirm from personal observation the opinion that Mr. Molesworth had given, by saying that vitrified forts generally, and probably always, are the result of premeditated firing. He had seen none in France, but their chief seat was in Scotland, where there were many. He had spent some time in examining Dan Macuisneachan or Macsniochan in Argyleshire, and having seen the vitrified matter resting on the unaltered rock below, there was no room for belief that internal fire, to which some persons attributed the combustion, had ever interfered. Although Mr. Molesworth had not seen actual vitrified matter at the fort he visited at St. Brieux, but had found only granite which had evidently been acted on by fire, he (Dr. S.) had specimens where the vitreous matter had evidently been ready to drop at the moment of its cooling and congealing. Many tons of vitrified matter may be seen on the Top of Noath, north of Aberdeen, and on Knockfarrel, near Strathpeffer, and indeed in many places. On one in Hungary, described by Dr. Stuart of Edinburgh and others, the layers of charred wood are seen alternating with the stones. None of this kind were known to Dr. Smith as being in Scotland. He had analysed the vitrified matter of a small fort in Bute, and one on West Loch Tarbert, Argyle, and found an increase of bases over the matter of the stones not vitrified. The ashes of the combustible would no doubt help to vitrify the surfaces and so cause adhesion, whilst the heat which penetrated the whole mass often broke up even sandstones, and bent or made brittle almost every species of the mixed rocks used. There seems however to have been an inclination to have some basaltic rocks to assist fusion. The vitrified walls are often very extensive and the plan is systematic; no conceivable siege fires or other violent and fitful work would produce the result. But as he (the speaker) had written three papers more or less bearing on this subject, he need not say more.

Ordinary Meeting, November 18th, 1873.

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

Mr. Arthur William Waters, F.G.S., Professor Arthur Gamgee, F.R.S., and Mr. Arthur Schuster, Ph.D., were elected Ordinary Members of the Society.

"On the Bursting of Trees and Objects struck by Lightning," by Professor Osborne Reynolds, M.A.

In a paper on this subject read at the last meeting of this society I stated that the tube which was burst by a discharge from a jar would probably withstand an internal pressure of from 2 to 5 tons on the square inch; and I made use of the expression the tube might be fired like a gun without bursting. These statements were based on the calculated strength of the tube, and with a view to show that there was no mistake, I have since tried it in the following manner.

I made 3 guns of the same tube. No. 1, which was 6 inches long, had its end stopped with a brass plug containing the fuze hole. No. 2 and No. 3 were 5 inches long and had their breeches drawn down so as only to leave a fuze hole. These tubes were loaded with gun-powder and shotted with slugs of wire which fitted them, and which were all $\frac{3}{4}$ inch long.

No. 1 was first fired with $\frac{1}{2}$ inch of powder, the shot penetrated $\frac{1}{4}$ inch into a deal board, and the gun was uninjured.

No. 2 was then fired with $1\frac{1}{2}$ inches of powder, and the shot went through the 1 inch deal board and $\frac{1}{2}$ inch into some mahogany behind, thus penetrating altogether $1\frac{1}{2}$ inches; the tube, however, was burst to fragments. Some of these were recovered, and although they were small they did not show cracks and signs of crushing like those from the electrical fracture.

No. 3 was then fired with $\frac{3}{4}$ inch of powder, and the shot penetrated $\frac{1}{2}$ inch into the deal board.

It was again fired with 1 inch of powder, and the shot penetrated 1 inch into the deal.

Again it was a third time fired with $1\frac{1}{4}$ inches of powder, when it burst and the shot only just dented the wood.

These experiments seem to me to prove conclusively the great strength of the tube and the enormous bursting force of the electrical discharge.

"On the Colour of Nankin Cotton," by EDWARD SCHUNCK, Ph.D., F.R.S.

Among the numerous varieties of cotton existing in commerce there is one which cannot fail to strike the most unpractised eye, in consequence of the peculiar colour, varying from a pale yellow or rather fawn to a brown or reddish-brown, which it exhibits. This kind of cotton is generally called "Nankin" cotton in consequence of its having been used at an early period by the Chinese for the manufacture of the fabric called nankin or nankeen, the peculiar colour of which is so well known as to need no description. Specimens of raw cotton of the colour referred to from other countries, such as India, America, the West Coast of Africa, and the shores of the Mediterranean are, however, found in all extensive collections, so that it cannot be considered as a product peculiar to China. In Malta it is, I am informed, especially abundant, more so than the ordinary white kind. Whether it is produced by a peculiar variety (not to say species) of the cotton plant or whether the colour is owing to peculiarities of climate, soil, or method of culture influencing the plant is, on the other hand, a question not easily determined. Considerable doubt indeed prevails as to the number of species embraced by the genus Gossypium and the characters by which they are distinguished from one another, some authorities admitting only four species, whilst others describe more than twenty. Among the former is Dr. Forbes Royle, who says*: "The result of our investigation of the species of the genus Gossypium is, that there are at least four distinct species which may be easily distinguised, and that the great mass, probably the whole of the cotton of commerce, is yielded by three of these species and their varieties." Attempts have been made to distinguish the various species of Gossypium according to the colour of the cotton produced by them, but as might be anticipated with little success, since the colour of the organs of plants seems to be one of the least persistent of their characteristics. Anyone, indeed, examining a collection of specimens of cotton fibre must see that there are few marked distinctions between them as regards colour, none being absolutely white, and the greater number exhibiting various shades of cream colour, verging to fawn. Nankin cotton may be considered as being placed, as far as colour is concerned, at the extreme end of the scale, at the other end of which we find Sea Island and other almost pure white kinds. Several authorities assert, it is true, that Nankin cotton is produced by one species of the genus only, viz., *G. religiosum*, but others say it is found on more than one species. Among the latter I may again quote Dr. Forbes Royle, who says: "*G. religiosum* of Linnæus seems to be distinguished from other species only by having tawny-coloured cotton; but we have seen that both the common Indian cotton, the Chinese cotton, the arboreous species, and *G. barbadense* all occasionally produce nankeen-coloured cotton, and that, therefore, it cannot be considered as characteristic." Referring to "China cotton" the same author says†: "The specimen in *Herb. Hook.*, from Mr. Fortune, is

* On the Culture and Commerce of Cotton, &c., p. 151.

† On the Culture and Commerce of Cotton, &c., p. 143.

less hairy than most Indian specimens, though clothed with a number of short hairs. Mr. Fortune states, in a note with some specimens that he sent to Dr. Lindley, from China, that the white-coloured and the nankeen-coloured cotton are yielded by the same species and even by the same plant, and that the two kinds are separated by the Chinese. Besides India and China, this species is cultivated in Persia, Syria, Asia Minor, and the Islands of the Mediterranean, as well as in the north of Africa and the south of Europe. The kind yielding the nankeen-coloured cotton in Malta is probably a variety." Fortune, in his Travels,* makes the following statement regarding the cotton plant of China: "The Chinese or Nanking cotton plant is the *Gossypium herbaceum* of botanists, and the '*Mie wha*' of the northern Chinese. It is a branching annual, growing from one to three or four feet in height, according to the richness of the soil, and flowering from August to October. . . . . The yellow cotton, from which the beautiful Nanking cloth is manufactured, is called '*Tze mie wha*' by the Chinese, and differs but slightly in its structure and general appearance from the kind just noticed. I have often compared them in the cotton fields where they were growing, and although the yellow variety has a more stunted habit than the other, it has no characters which constitute a distinct species. It is merely an accidental variety, and although its seeds may generally produce the same kind, they doubtless frequently yield the white variety, and *vice versâ.* Hence specimens of the yellow cotton are frequently found growing amongst the white in the immediate vicinity of Shanghai; and again a few miles northward, in the fields near the city of *Poushan,* on the banks of the Yang-tse-Kiang, where the yellow cotton abounds, I have often gathered specimens of the white variety." The opinion here expressed is confirmed by Parlatore,† who affirms,

* Two Visits to the Tea Countries of China, vol. I., p. 199.

† Le Specie dei Cotoni. Firenze, 1866.

without hesitation, that the plant bearing red or reddish cotton is merely a variety either of *G. arboreum* or *G. hirsutum.*

Mr. Thomas Clegg, of this city, who is familiar with the properties of the various kinds of cotton and well acquainted with their commercial value and places of growth, has kindly lent me some of his specimens for exhibition on this occasion, and in a communication received from him he has given me some information regarding Nankin cotton which will no doubt be of interest to the meeting.

Mr. Clegg says: "I found Nankin cotton abundantly at Malta, many parts of Tunis, and in great quantities on the West Coast of Africa. Dr. Livingstone has sent me many samples of it, and I have frequently had specimens of it from other, but always arid, dry, and hot parts of the world. The Maltese has however always been the best. It is very short in staple, coarse and of little value in itself, especially as so little of it is produced. Being high coloured, of course if used alone it would give a high peculiar colour to the cloth, and as mixing it with whiter cotton generally stripes and spoils the cloth, it is in very bad repute. In China and Japan it gets more dusky and dark and even lower in staple. On the West Coast of Africa it seems to be hybridized and modified in colour. The seed, when cleaned from the cotton, is generally only half clothed with the fibre, the other half being black. But whether entirely Nankin-coloured or a little whiter, it is always on that coast much longer in staple, and though rather coarse, still a good useful cotton. Indeed the generality of the West Coast Cotton is a nice cream-coloured cotton, a little higher than the old Demerara cotton used to be, and of staple on an average fully equal to American bowed upland, or the lower class of New Orleans, and I hardly think can be classed as Nankin at all, though high-coloured. If it could be had in quantity it would, in my opinion, soon supersede all the lower qualities of American cotton. Nankin cotton is always, so far as I have seen, from fibrous-coated seed. As to the colour, I cannot tell, being no chemist, whether it is fast or not, but it never seems to fade with me. . . . . According to my experience it is only in very hot countries, and on a rather arid soil, that the really dark Nankin is produced. I think if experiments were tried for three or four years together, Maltese on the West Coast of Africa would resemble African, and West African seed sown at Malta would become Maltese cotton; and I almost think that West African seed which at home produces yellow-tinged cotton, would, in one or two years, in the New Orleans district, produce white cotton. And I further think that pure New Orleans seed sown at Malta in three or four years would give cotton of the red tinge of ordinary Maltese." Mr. Clegg seems therefore to agree with those who think that the variations in colour observed in cotton are entirely owing to differences of climate and soil, and are not peculiarities attaching to different species of the plant.

These remarks will suffice to give a general idea of the properties of Nankin cotton and its supposed origin. I propose in this communication to give a short account of some experiments made to ascertain the cause of the peculiar colour by which it is characterized.

The colour of Nankin cloth having been successfully imitated in this country by depositing oxide of iron on and within the fibres in the manner well known to dyers, it might be supposed that the colour of the raw cotton was due to iron in some form. The simplest experiments prove this however not to be the case. The cotton on being incinerated leaves an ash which does not contain a larger proportion of iron than that of ordinary cotton, and the colour is not removed by treating the cotton with dilute mineral acids capable of dissolving oxide of iron, whereas the colouring matter dissolves, though slowly, on boiling it with caustic soda lye. Another mode of imitating the colour consists in mordanting the fabric with alum and then dyeing with oak bark, the process resembling that by which calico is ordinarily dyed of a yellow or fawn colour. It is evident however from its resisting the action of acids, that the colour of Nankin cotton cannot be due to the presence of a lake of alumina or any other base. In order to arrive at some conclusion regarding the nature of the colouring matter, it was necessary to employ large quantities of material, for though the colour looks intense when the cotton is viewed in mass, it is in reality produced by a small quantity of substance spread over a large extent of surface, in this respect resembling the colour of the petals of some flowers. I therefore had recourse to the plan adopted on a previous occasion, and described in a paper I had the honour of reading before this Society several years ago.* A quantity of yarn made entirely of Nankin cotton (from the coast of Coromandel) was submitted to the usual process of bleaching, and the dark brown liquid obtained by treating the yarn with boiling alkaline lye was mixed with an excess of acid which produced a dark brown flocculent precipitate. This was filtered off, washed with water, and then treated exactly in the manner described in the paper just referred to. It was found to contain the same substances as the precipitate obtained in the same way from alkaline lyes with which ordinary Indian or American cotton had been treated, viz., cotton wax, fusing at the same temperature and having the same general properties as that from ordinary cotton, a white crystalline fatty acid (probably margaric acid) pectic acid, parapectic acid, and lastly colouring matters. It is to the latter that the cotton owes its colour, for this colour is removed to a great extent by treatment with alkali, while the colouring matters are thrown down from the liquid

* Memoirs, 3rd Series, vol. IV., p. 95

on the addition of acid, and I therefore examined them with more care than the other constituents of the precipitate. These colouring matters I found to be at least two in number. One of them is easily soluble in alcohol and is obtained on evaporating the solution as a dark brown, shining, transparent resin. The other is almost insoluble in cold alcohol, but dissolves in boiling alcohol, and is deposited on the solution cooling in the form of a light brown powder. Their properties are in general the same as those of the analagous colouring matters from ordinary cotton. They contain, like these, C, H, N, and O, but in somewhat different relative proportions. Their composition in 100 parts I found to be as follows:

	A. Colouring matter soluble in cold alcohol.		B. Colouring matter insoluble in cold alcohol.
C	58·22	C	57·70
H	5·42	H	5·60
N	3·73	N	4·99
O	32·63	O	31·71

The composition of the analogous colouring matters from American cotton according to previous determinations was as follows:—

	A.		B.
C	58·42	C	58·36
H	5·85	H	5·71
N	5·26	N	7·60
O	30·47	O	28·33

The difference in composition, in the first case at least, is not greater than may be expected with substances of the purity of which, in consequence of their not occurring in a crystallized state, one can never be perfectly sure. On the whole, I think these experiments justify the conclusion at which I have arrived, viz., that the colour of Nankin cotton is due to the presence of bodies which are very similar to, if not identical with, those which cause the much fainter tints of the ordinary kinds. They show too that the substances accompanying the cellulose (whether clothing the fibres, or contained in their interior) are the same with this variety of cotton as with all those previously examined.

"An Improved Method for preparing Marsh Gas," by C. SCHORLEMMER, F.R.S.

Everyone who ever had to prepare soda-lime knows that the preparation of this substance is a troublesome as well as a laborious process. Chemists will therefore hail with pleasure a paper "On the Determination of Nitrogen," by S. W. Johnson (*Liebig's Ann.*, 169, 69). He has found that in using the method of Varrentrapp and Will, soda-lime may be replaced by an intimate mixture, of about equal weights, of anhydrous sodium carbonate and dry slaked lime. It occurred to me that such a mixture might also be employed instead of soda-lime in the preparation of marsh-gas, and I found that by heating an intimate mixture of anhydrous sodium acetate with more than twice its weight of lime and sodium carbonate, a very regular and quiet evolution of marsh gas took place. The gas, thus obtained, always contains some acetone, which is easily removed by shaking it with water, or, better still, with a solution of acid sodium sulphite.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 13th, 1873.

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

The PRESIDENT delivered the following address:

It being the wish of your Council that I should open our new session by a few preliminary remarks upon our position in reference to the work which lies before us, I have not felt myself at liberty to decline acceding to their request.

I think we can scarcely meet in this hall without being in some degree stimulated to effort by the associations which cling to it. It is the hall rich in memories of White and of Perceval, of Dalton and of the two Henrys. It is the hall in which most of their discoveries were announced—discoveries which have given Manchester so distinguished a place in the scientific annals of Great Britain. It is to me a matter of no small pride and satisfaction that the present age will add its contribution to that illustrious roll of names, and that Joule and Fairbairn, Binney and Roscoe, will be remembered by our successors as men who in their several spheres of labour carried the torch of science with no uncertain grasp, and handed it on to their posterity shining with a light as brilliant as it gave forth when they received it from their distinguished predecessors.

Stimulated by these remembrances, we do well from time to time to take a conscientious view of our position in relation to the work which awaits us. We do not occupy our places in this room in the capacity of educators, but of investigators. Whatever duties of the former class may devolve upon some of us elsewhere, in this place we neither profess to be the teachers of the young, nor popularisers of science for the benefit of the adults who join in our assemblies. It is true that we may, in some humble measure, fulfil both these functions, but such fulfilments are but the collateral incidents of our position. Our proper duties are those of pioneers, endeavouring to carry light where all is as yet dark,—to dispel the thick mists of our own ignorance which still envelop us as with a pall, and which can only be dispelled by vigorous and combined efforts. It is our duty to try to discover unknown truths, to ascertain hitherto unobserved facts, and, if possible, to trace the relations which subsist between them, as well as those which they bear to facts previously known. In this search for truth no new fact is so insignificant as to be unworthy of observation, since it may prove to be the falling apple capable of suggesting a law of the universe,—or the kite-drawn spark from the passing cloud which opens out a new world of light and force. If, then, in our researches we catch a faint glimpse of some such truth, let us pursue it diligently until we succeed in bringing it into clearer view. The ultimate use of which such truth is capable must not be the primary object of our pursuit. Many of our investigations must be carried on without reference to mercenary aims. At the same time whilst we thus follow truth for her own sake, let us not ignore the vital importance of applied science, or forget that those who so apply it are honoured instruments in alleviating the toils and increasing the joys of humanity.

How the existing band of active scientific workers is to be increased is one of the problems now occupying the minds of some of our ablest thinkers. The need for such an increase, especially in our own country, is admitted by all, but men differ as to the efficiency of the means suggested. Some, whose names stand high on the roll of the learned, think that the endowment of fitting men to be set apart as discoverers, will best meet the difficulty. I confess I have many fears as to the success of such a plan. Scientific research is rarely the child of wealthy leisure. It is true that you have, here and there, a Murchison or a Lyell, whom a fortunate combination of circumstances has freed from the necessity of labouring to win the means of existence, but who were yet more fortunate in possessing the vigorous energy which rendered powerless the paralyzing influences of wealth and station. As a rule, the wealthy classes have done but little scientific work, and such as have entered the field as vigorous labourers have rarely been those whose leisure was co-extensive with their wealth. They were usually men, who, like the two Lubbocks, were actively engaged in the engrossing pursuits of the office or of the counting-house, but who still found time to labour in the scientific field and established their claim to stand abreast of the foremost in the army of workers. The fact is that too much leisure tends to enervate the mind, whilst the busy commercial or professional life stimulates the brain into ceaseless activity. This habitual vigour is restless for action, even in what with most men would be idle hours, and expends its surplus energy upon literary labours or scientific research. Such facts make me very dubious as to the advantages which would arise from the special endowment of men whose sole occupation in life should be scientific enquiry.

Except in some special departments of science accidental circumstances rather than a pre-arranged plan have much to do with determining the direction in which original investigations are carried on. Some small incident primarily directs an observer's attention to a special field of labour, and as he follows its leads the area expands before him and he ultimately finds himself deeply engaged in a career of important research. Of course I presume that early training has prepared him for the work. Now these facts appear to me to suggest the real remedy for the present scarcity of a high class of workers, and I think the times shew signs, however faint, of prospective improvement in this matter. We want a larger supply of sufficiently well endowed professorial chairs in which the obligatory duty of teaching shall stimulate a man to keep himself abreast of his age, but in which that duty shall not be so all-engrossing as to occupy the whole of his time. By such arrangements many needful things would be provided. We should have an enlarged body of men who would be competent to pursue original researches, because the previous training which their official position would involve would prepare them to recognise what the Arctic navigator would term new leads, as well as give them the intelligent power to follow them out. I am satisfied that the two kinds of mental energies respectively involved in teaching and in pursuing new investigations act and re-act upon each other. That the man actively engaged in teaching is undergoing the best of preparations for other work, whilst the original investigator is of all men most likely to fulfil well the duties of a teacher, because he is more likely than other men to be animated by zeal and enthusiasm for the work upon which he is engaged. Original research, I believe, furnishes the best of training schools for the teacher, whilst the students will benefit by drinking at the running stream instead of at the stagnant pool. That a growing demand for teachers of physical science is springing up is obvious. The grammar schools of Manchester, Bradford, and Giggleswick may be taken as examples of the change which a more enlightened public opinion is producing in the respective various parts of the kingdom. When we remember at how recent a period a small band of workers like Huxley and Brodie first insisted upon the necessity for a great change in both the primary and the higher education of the youth of Great Britain I confess I marvel at the wondrous results that have already been attained, and I look forward into the future with sanguine hope. Meanwhile we experience a pressing want of a larger army of young workers. The case is one in which the old political maxim appears to be reversed in which supply will increase demand. It appears as if there were many great and venerable educational establishments in this country which would enter heartily into the work of science teaching if they could obtain competent and judicious teachers—men who would not merely cram their pupils' minds with isolated details but who were competent to make such teaching a real educational instrument and not merely an exercise of the memory. This want appears to me to indicate the more immediate duty of government at the present time. The brief experiment tried at South Kensington, a short time ago, by Prof. Huxley, aided by Dr. Ray Lankester and Professor Michael Foster, is one which from its costliness must be prosecuted by the Government if carried on at all, because if it is to be productive of the full measure of the benefit to the nation of which it is capable, it must be carried on in a prolonged manner and on a very large scale. Such a work would, it appears to me, be accepted by the members of the House of Commons as involving a legitimate expenditure of the public finances, because it would be in strict accordance with that system of training of teachers to which they have already given their official sanction. Such a system, if regularly organised and perseveringly sustained, would tend to create an army of men competent to become original workers, and if this method was followed up by the establishment of local colleges in all the great centres of industry, there would be found permanent spheres of labour for the students thus trained. Two things needful to our national well-being would thus be attained; we should enrich the country by an extension of those scientific researches which within our own lifetime have proved to be so fertile a source of national wealth, and we should extend a scientific educational organisation which the fashionable quackeries of the day shew to be a necessary adjunct to that almost exclusively literary one which has so long monopolised the teaching functions in this country.

Mr. Charles Bailey exhibited specimens of *Carex punctata*, Gaudin, which he had collected in August last on the damp, narrow ledges of perpendicular rocks near a water-fall, on the north side of a small bay, named Waterwinch, about a mile north of Tenby, Pembrokeshire. This *Carex*, although it has long held a place in English floras, on the authority of some Cornish specimens seen by Dr.

Boott, has generally been admitted with doubt as an indigenous plant; many botanists believing that varieties of *Carex distans* had been mistaken for it. There are several localities in Ireland recorded as stations for Gaudin's plant; it has also been found recently in Scotland, and the Tenby locality above referred to re-establishes its right to be considered a British species.

November 10th, 1873.

Charles Bailey, Esq., Vice-President of the Section, in the Chair.

"Remarks upon the British locality for *Lobelia urens*," by J. C. Melvill, M.A., F.L.S.

The author being in the neighbourhood of Taunton this summer, determined to visit Axminster, the only known locality for this species in Britain, and set out for that purpose on the 1st of August.

The common where it was said to grow in 1836, has now been cultivated, and the old landmarks removed, and no trace of the plant was to be seen; however, a mile or two further on, beyond Shute Hill, he met with the lobelia in tolerable plenty, but exceedingly local. The flowers were in perfection, showing that the time of flowering mentioned in the various Botanical works is erroneous, where Autumn is stated to be the time. Mr. Melvill exhibited some dried specimens and distributed them among the members.

"On Lymexylon Navale," by Joseph Sidebotham, F.R.A.S.

Mr. Sidebotham referred to the various authors who have written about this species, the first and probably only British example of which was taken by Mr. Griesbach, in

Windsor forest, in the year 1829. It is said formerly to have been so abundant in the naval dockyards of Sweden, that Linnæus was consulted as to the best mode of stopping the injury done by it to the timber, an account of which he published in his *Iter Westrogoth.*

Last year several specimens of this insect were taken in Dunham park by Mr. Joseph Chappell, and Mr. Sidebotham, on visiting the tree, found the larvæ feeding in the wood.

In July of the present year the author visited the park again, and found various specimens, of both sexes, in the tree, and also on the wing, and expressed an opinion that the species would be found in other places now that its mode of life and habits had been investigated, of which he gave various particulars. He also exhibited specimens of the insects, and of the wood bored by them.

Mr. Sidebotham exhibited various forms of *Helix pisana,* from Tenby, and distributed specimens among the members.

Ordinary Meeting, December 2nd, 1873.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

Mr. Henry H. Howarth, F.S.A., was elected an Ordinary Member of the Society.

The following letter from HENRY BOWMAN, Esq., of Brockham Green, Surrey, dated 27th November, 1873, was read:—

Seeing the report of Prof. Reynolds' experiments on the nature of the explosive force of lightning, it occurred to me that the Society might be interested in an account of a remarkable thunderstorm which took place here on Friday the 7th inst.

I was not at home at the time, and did not hear of the storm until my return the following week.

The Vicar of this place sent a short notice of the storm to the "Times," copy of which is inclosed, together with some further particulars which he has kindly jotted down.

(COPY.)

Times of Nov. 12, 1873:

"The Rev. Alan Cheales ...... writes to us ...... a remarkable storm burst over the valley between Dorking and Reigate on Nov. 7. ...... In general we have a remarkable immunity from thunderstorms, which draw away along the Downs to the north of us. The storm came up suddenly from the N.W. ...... I have just returned from inspecting an oak tree in Betchworth Park ...... which was completely cleft in two ...... the tree has been chopped down within about 10 feet of the ground ...... the girth is about 8 feet."

My dear Sir, Nov. 20, 1873.

In reply to your enquiries relative to the very remarkable thunderstorm of Nov. 7, mentioned in the *Times* of Nov. 12, I have much pleasure in putting before you such other particulars as I have been able to gather.

There was no warning whatever, a black cloud rolled up, a little rain, and then two tremendous flashes.

The tree struck was a fine young oak, half covered with ivy, in a row with other oaks, some also having ivy on them.

E. Batchelar, painter at Dorking, 2 miles off, describes a tremendous flash of blue light, and thunder instantly.

Rev. G. R. Kensit, walking in fields about 1 mile off, describes a ball of blue fire which seemed to fall on his foot.

Martha Rapley, coming home through Betchworth Park with perambulator and two children, was only 300 or 400 yards from the tree; was so stunned and blinded that she saw nothing — perambulator seemed wrapped in flames — she did not know whether the children were dead or not until she got home. Neither injured.

J. C. Richards, Esq., of Boxhill Farm, had a sheep killed about ½ a mile off; he seemed to think it was killed by the same flash that split the tree. There was only one other flash.

The tree, unfortunately, was removed at once by Mr. Hope's woodreeve. I could not see that any part was blackened. Mr. Richards observed to me that no conceivable human force could have so split it.

Part of the fibre had been made to writhe round in a most remarkable manner.

Huge splinters (3 feet by 3 or 4 inches) were chopped out and lay a few feet off.

The tree was divided and hung on each side of the fence about 8 or 10 feet from the ground. The lightning seemed to have run down thence along the outside bark into the ground; but the marks were slight. The tree was still solid below the cleft.

I am yours faithfully,

Alan Cheales.

Ordinary Meeting December 16th, 1873.

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

Mr. James Heelis was elected an Ordinary Member of the Society.

The Chairman said that since the last meeting the Society had lost one of its most illustrious members by the death of Professor Louis Agassiz, the great naturalist, who had been an Honorary Member for above thirty years. He (the Chairman) had the honour of being personally acquainted with the deceased, having been brought into communication with him during the publication of his great work, "Recherches sur les Poissons fossiles," and having had the pleasure of supplying him with specimens for its illustration. In the Royal Society's Catalogue is a list of 130 scientific memoirs he gave to the world. The reputation of Agassiz as one of the foremost men of his day in natural history is too well known to need any tribute from me, but after a lapse of thirty years his amiable manners and his great kindness in cheerfully imparting his vast stores of knowlege to the humblest student are fresh in my memory. He was one of the kindest and heartiest of men, and his fine and manly countenance at that time was the picture of health and good nature, and truly reflected the genial soul within. In great and small matters he was equally punctual and correct. All who ever allowed him to make use of their specimens must well remember the ample acknowledgments he made and the scrupulous care with which they were returned; and some of the first living palæontologists might learn a useful lesson in this respect from the illustrious dead. With Agassiz it may be truly said that it was hard to decide whether his head or his heart most deserved our admiration. The world has to lament the death of a great and good man, and this Society one of its greatest ornaments.

"Method of Construction of a New Barometer," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

The condition of the instrument placed on the 18th of March in the Society's Hall proves that it is possible to use sulphuric acid on the top of the mercurial column without chemical action taking place. I have therefore proceeded to prepare other tubes with a view to test, by practical work, the merits of the new contrivance.

A tube of about $\frac{3}{16}$ inch bore is selected. It is first cleaned by drawing a knotted string through it. It is then bent to the siphon shape; and near the longer end it is drawn to a capillary tube. It is then washed with nitric acid; afterwards with sulphuric acid. The sulphuric acid is then drained off. Mercury is then poured into the short limb. The end of the longer limb is then attached to my mercurial exhauster. On working this the mercury rises in the tube, and, being replenished by pouring it into the short limb, soon arrives at the height due to the atmospheric pressure. It carries with it the acid left adhering to the sides, so that after a few hours half, or, what is better, one third of an inch of acid stands above the mercury. Small bubbles of air are seen to arise; but by leaving the tube in connexion with the exhauster for a day or two these finally cease. Mercury is then poured into the short limb until that in the longer rises nearly to the capillary part of the tube. This is then sealed and detached from the exhauster. Mercury is then removed from the shorter limb until it stands in the long one at a convenient height. Sulphuric acid is then introduced into the short limb until it forms a column equal to that in the longer limb. A small tube is finally attached to the short limb, and dipping a little way into a small bottle containing a small quantity of sulphuric acid, prevents the access of moist air into the short limb.

The tube thus completed possesses the following advantages: — 1st. There is the utmost facility in the movement of the column, so that the most minute changes of pressure are at once registered without any dragging. 2nd. The depression produced by capillary action is reduced to one half, so that the siphon arrangement can be satisfactorily used as affording an accurate neutralization of capillary action.

Mr. BROTHERS exhibited the plates forming the first part of the Holborn Society's photolith reproduction of Hans Burgman's *Triumphs of Maximilian I.* The designs are engraved on wood and printed on separate sheets, but the set shown were mounted so as to exhibit the artist's intention — that of a triumphal procession. This remarkable work is considered to be one of the finest specimens of wood engraving.

Mr. BAXENDELL read the following letter from Professor C. PIAZZI SMYTH, F.R.S., Astronomer Royal of Scotland: —

Referring (as the prompt and frequent publications of your Society so easily and agreeably enable one to do) to Professor Osborne Reynolds's triumphant proof, on November 18th, that his glass tubes were strong enough to act as guns,—and also that 1·5 inches depth of powder produced nothing like the force exerted by the electrical discharge; and that that electrical discharge acted by means of conversion of water suddenly into steam, as when lightning rends a tree;— may I beg to offer two remarks?

(1) The soundness of the discussions before the Society on the recent wood-struck case near Manchester, is evident by a similar conclusion arrived at by the British Associa-

tion when at Edinburgh in 1850. For a tree in the neighbourhood having been struck and specially shattered into thin plates of wood, during the week of congress, was formally examined by a deputation of the Association, and the lightning was held to have exploded the watery matter of the sap-vessels.

(2) That water is a far more powerful exploder than gunpowder if you can get it (the water) to explode at all, is now experimentally proved by Professor Osborne Reynolds's electrical experiments; and did occupy my attention many years ago, on comparing the far larger increase of space occupied by exploded water in the shape of steam, than by exploded gunpowder in the shape of its permanent gases.

The difficulty however is, to get the water to explode, and not to pass off merely into steam; a difficulty well illustrated by any and every accession of dampness to gunpowder fired in the usual way, decreasing, instead of increasing, the gunpowder's explosive force.

In order to try to explode water, at that time, I melted a large ladle full of lead; put upon the fluid and almost red-hot surface a drop of water and tried various devices to bring it under the influence of the heat; but even when forcibly attempted to be pushed under the melted lead, the water ran with vehemence up the substance of the wooden probe employed, and refused to have anything to do with the fluid lead, which consequently remained undisturbed.

But when I next took a smaller iron ladle, put a drop of water on the bottom of it, and gave therewith a little pat to the surface of the melted lead, instantly the whole contents of the great ladle were scattered to the winds, and only a few grains were recovered. Explosion of water had apparently taken place with excellent effect.

Then came a question as to repeating such an explosion at small intervals of time, in a safe manner, so as to have an explosion engine; in which, if all the heat of the coal could be used in exploding water, rather than in raising steam, a surprising economy of fuel should result.

But as no progress was made in such an engine, I can only refer to some old accounts of an explosion in a copper foundry, where the great establishment was literally blown up, it was said, by a workman simply spitting into a vessel of melted copper. The mere amount of steam raised from the saliva would evidently have been of no practicable avail for either good or evil, even if employed in the best modern expansive engine on the thermo-dynamic principles; but, as an explosive, its energy would seem to have been so vast, that I must hope for further development of the subject at the hands of the able men of science in the Manchester Literary and Philosophical Society.

"On the Destruction of Sound by Fog and the Inertness of a Heterogeneous Fluid," by Professor OSBORNE REYNOLDS, M.A.

1. That sound does not readily penetrate a fog is a matter of common observation. The bells and horns of ships are not heard so far during a fog as when the air is clear. In a London fog the noise of the wheels is much diminished, so that they seem to be at a distance when they are really close by. On one occasion during the launch of the Great Eastern the fog was reported so dense that the workmen could neither see nor *hear*.

2. It has also been observed that mist in air or steam renders them very dull as regards motion. This is observed particularly in the pipes and passages in a steam engine. Mr. D. K. Clark found in his experiments that it required

from 3 to 5 times as much back pressure to expel misty steam from a cylinder as when the steam was dry.

3. My object in this paper is to give and to investigate what appears to me to be an explanation of these phenomena; from which it appears that they are intimately connected, that, in fact, they are both due to the same cause. This explanation will be the clearer for a few preliminary remarks.

4. The nature of a fog and the manner in which the small spherical drops are suspended against their weight is well understood. So long as the fog is at rest or moving uniformly, the drops being heavier than the air tend to sink like a stone in water, and consequently they are not at rest in the air but are moving through it with greater or less velocities according as they are large like rain or small like haze. This motion is caused entirely by the difference in the specific gravity of the air and water, if the drops were merely little hard portions of air they would have no tendency to descend.

In some fogs the drops are so fine that they appear to be absolutely at rest, and will remain for a long time without any appreciable motion. The force which retards the downward motion of the drops is the friction of the air, and this is proportional to the surface of the drop and the square of the velocity. As the drops get smaller their weight diminishes faster than their surface, and consequently the friction will balance the weight with a less velocity. The exact law is that the velocity caused by the weight of a drop is proportional to the square root of its diameter.

This is the general explanation of what goes on under the action of gravity when the fog is at rest or moving uniformly, and we may make use of it to illustrate what goes on when the fog is subjected to accelerating or retarding forces.

5. If we imagine a vessel, full of such a compound as the fog is made of, to be set in motion or stopped, the accelerating or retarding force will have to be transmitted from the sides of the vessel to the fluid within it by means of pressure. These pressures will act equally throughout the fluid, and if the fluid were homogeneous they would produce the same effect throughout it, and it would all move together but the pressures will obviously produce less effect on the drops of water than they do on the corresponding volumes of air, and the result will be that the drops of water will move with a different velocity to the air—that the drops of water will in fact move through the air just as they do under the action of gravity. In fact, if the air is subject to an acceleration of 32 feet per second the effect on the drops (their motion through the air) will be the same as that due to their weight. It is easy to conceive the action between the air and the drops of water. If a mass of air and water is retarded it is obvious that the water, by virtue of its greater density, will move on through the air. This property has, in fact, been made use of to dry the steam used in steam engines. The steam is made to take a sharp turn, when the water, moving straight on through it, is deposited on the side of the vessel.

6. Owing to this motion of the water through the air it would clearly take longer with the same force to impress the same momentum on foggy air than on the same when dry. This is obvious, for at the end of a certain time the particles of water would not be moving as fast as the air, and consequently the air and water would have less momentum than the same weight of dry air all moving together: that is to say, if we had two light vessels containing the same weight of fluid, the one full of dry air and the other full of fog, and both subjected to the same force for the same

time, at the end of this time, although they would have exactly the same motion, their contents would not, for the drops of water in the fog would not be moving so fast as the vessel. Now the energy expended on each of these vessels would be the same, but, inasmuch as the effects are different, the energy acquired by the foggy air would be less than that acquired by the dry air, the difference having gone to move the water through the air: that is to say, it would require more pressure to impress in the same time the same velocity on foggy air than on dry air of the same density.

7. This then fully explains the dulness with which foggy air acquires motion. In the passages of a steam engine the steam is subjected to continual accelerations and retardations, each of which requires more force in the manner described with misty than with dry steam, and at each of which the particles of water moving through the steam destroy energy in creating eddies.

8. Although not so obvious, the same is true in the case of sound. The effect of waves of sound traversing a portion of air is first to accelerate and then to retard it. And if there are any drops of water in the air these will not take up the motion of the air so readily as the air itself. They will allow the air to move backwards and forwards past them, and so cause friction and diminish the effect of the wave as it proceeds, just as a loose cargo will diminish the rolling of a ship.

9. It is important to notice that this action of the particles of water is not analogous to their action in reflecting the waves of light.

It has been assumed as an explanation of the action of fog on sound that the particles of water break up the wave of sound by small reflections in the same way as they scatter the waves of light. The analogy however is not admissible; for in the case of light the wave length is shorter than the thickness of the drops, and the surface of the drop acts in the same way as if the drop were of large extent; but in the case of sound the wave's length may be thousands of times the thickness of the drop, and instead of the whole wave being reflected it will only be a very small portion of it. Even this portion can hardly be called a reflection; it is due to the motion of the air past the drops like the waves of sound caused by a bullet, or the waves thrown off by the bow of a ship.

10. A certain portion of the resistance which the air offers to the motion of the water through it is this—what is called in naval science *wave resistance;* but it can be shown that the proportion of this resistance to the resistance in causing eddies diminishes with the velocity, and consequently it can have very little to do with the effect of the drops of water on the waves of sound, in which the velocity of the water through the air must be very small.*

11. So far, then, I have shown the manner in which the fog diminishes the sound; it remains to consider the connection between the size of the drops and their effects. I am not aware that any observations have been made with respect to this. I do not know whether it has ever been noticed whether a fine or a coarse mist produces the most effect on sound. It does not appear, however, that rain produces the same effect as fog; and considering rain as a coarse fog, we must come to the conclusion that a certain degree of fineness is necessary.

If we examine theoretically into the relation between the size of the drops and the effect they produce, always assuming

* This reflection has nothing to do with the reverberation from clouds which occurs in a thunderstorm, which is probably due to the different density of the clouds, and takes place at their surfaces.

the same quantity of water in the air, we find in the first place that if the air is subjected to a uniform acceleration, which acts for a sufficient time for the drops to acquire their maximum velocity through the air, the effect of the drops in a given time—that is to say, the energy dissipated in a given time—is proportional to the square root of the diameters of the drops. This appears from the action of gravity. As previously stated, the maximum downward motion of the drops; and hence the distance they will have fallen in a given time and the energy destroyed is proportional to the square root of their diameters. Hence where the acceleration acts continuously for some time, as would be the case in a steam pipe, the effect will increase with the size of the drops.

This effect may be represented by a parabolic curve in which distances measured from the vertex along the axis represent the size of the drops and the corresponding ordinates represent their effect in destroying energy.

If on the other hand the acceleration alternates very rapidly then there will not be time for the drop to acquire its maximum velocity, and if the time be very short the drop will practically stand still, in which case the effect of the drops will be proportional to the aggregate surface which they expose. And this will increase as the diameter diminishes, always supposing the same quantity of water to be present.

This latter is somewhat the condition when a fog is traversed by waves of sound, so long as the drops are above a certain size; when, however, they are very small, compared with the length of the waves, there will be time for them to acquire their maximum velocity. So that starting from drops the size of rain, their effect will increase as their size diminishes, at first in the direct proportion, then more and more slowly until a certain minuteness is reached, after which, as the drops become still smaller, their effect will begin to diminish, at first slowly, but in an increasing ratio, tending towards that of the square root of the diameter of the drops.

This effect may be represented by a curve which coincides with the previously described parabola at the vertex, but which turns off towards the axis, which it finally approaches as a straight line.

This completes the investigation, so far as I have been able to carry it. The complete mathematical solution of the equations of motion does not appear to be possible, as they are of a form that has not as yet been integrated. However, so far it appears to me to afford a complete explanation of the two phenomena, and further to show, a fact not hitherto noticed, that for any note of waves of sound there is a certain size of drop with which a fog will produce the greatest effect.

"The Chemical Constitution of Bleaching Powder," by C. SCHORLEMMER. F.R.S.

In his classical research "On the Compounds of Chlorine with Bases,"* Gay Lussac has shown that the bleaching compounds formed by this reaction are not direct combinations of chlorine and a base, as Berthollet believed, but that a hypochlorite and a chloride are produced simultaneously, according to the equation

$$2KOH + Cl_2 = KOCl + KCl + H_2O.$$

When to the compounds thus formed a small quantity of a mineral acid is added, hypochlorous acid is set free, whilst by adding the acid in excess chlorine is obtained; because in the latter case the hydrochloric acid acts on the hypochlorous acid in the following way:

$$ClH + ClOH = Cl_2 + H_2O.$$

* "Comptes Rendus," XIV. 927.

As a ready method for preparing a dilute solution of hypochlorous acid, Gay Lussac recommends to distil a solution of bleaching powder with a quantity of dilute nitric acid which is just sufficient to liberate the hypochlorous acid.

According to Gay Lussac's view, bleaching powder is a mixture of calcium chloride and calcium hypochlorite, and the same view is held by most chemists. Professor Odling has however pointed out that, calcium being a dyad metal, the constitution of bleaching powder was probably $Ca\begin{cases} Cl \\ OCl \end{cases}$ or it was at the same time a hypochlorite and a chloride. Of course both views explain equally well the formation of hypochlorous acid by Gay Lussac's method. I read therefore with great surprise a paper by Goepner (Dingler's *Polytechn. Journ*, 209, 204), in which he states that bleaching powder is nothing but a simple combination of lime and chlorine, which by acids is again resolved into its constituents without the least trace of hypochlorous acid being formed. He says that, although the preparation of hypochlorous acid by this method is described in all hand books as if this experiment had been made hundreds of times, this is a mistake, and the reason why this error has maintained itself so long in chemical literature is that hitherto no reaction was known by which free chlorine and hypochlorous acid could be readily distinguished. But such a reaction has now been found by Wolters, who has shown that when chlorine-water is shaken with an excess of mercury only mercurous chloride is formed, while with aqueous hypochlorous acid it yields a brown crystalline oxychloride of mercury, which is readily soluble in hydrochloric acid, and thus offers a ready means of the qualitative as well as quantitative determination of hypochlorous acid in the presence of free chlorine.

In employing this reaction for detecting hypochlorous acid in the liquid which was obtained by distilling bleaching powder with a small quantity of hydrochloric or sulphuric acid, Goepner could not find a trace of hypochlorous acid, but only free chlorine.

I have already mentioned that he says the preparation of hypochlorous acid by this method is described in the books as if this experiment had been repeated hundreds of times. Now this experiment has been repeated many hundred times in our laboratory only. Professor Roscoe shows it every year in his lectures, and all our students in the course of their practical work perform it, and find that the perfectly colourless distillate is a much more powerfully bleaching agent than freshly prepared chlorine-water. This is quite sufficient to show that the liquid contains hypochlorous acid. But why did Goepner fail in detecting it? Perhaps it was the fault of the analytical method? To decide these questions I prepared hypochlorous acid by distilling solutions of bleaching powder with dilute nitric and sulphuric acid and shook the colourless distillates with mercury. In every case the brown oxychloride was formed in quantity and possessed all the properties which Wolters has assigned to it, while by shaking chlorine-water with mercury only calomel was formed. From a careful perusal of Goepner's paper I was unable to find the cause of his failure.

Another argument against the existence of a hypochlorite in bleaching powder is, according to Goepner, the following. The chlorine which is used in the manufacture of bleaching powder always contains free hydrochloric acid, and thus in bleaching powder more calcium chloride will always exist than would correspond with Gay Lussac's formula. Now when bleaching powder is exhausted successively with small quantities of water, the excess of calcium chloride is always found in the first solutions, whilst those following contain calcium and chlorine in the proportions corresponding to the empirical formula $CaOCl_2$. This fact, however, only proves that bleaching powder is not a mixture of calcium chloride and hypochlorite, but that the bleaching compound contained in it has the constitution which Professor Odling has assigned to it.

Professor Williamson has shown that an aqueous solution of hypochlorous acid may also be obtained by suspending finely divided calcium carbonate in water and passing chlorine into the liquid until the carbonate is dissolved and then distilling the solution. In this reaction the compound Ca(OCl)Cl is probably also first formed and acted on by an excess of chlorine in the following way:—

$$Ca\left\{\begin{matrix}Cl\\OCl\end{matrix}\right. + Cl_2 = Ca\left\{\begin{matrix}Cl\\Cl\end{matrix}\right. + Cl_2O.$$

Ordinary Meeting, December 30th, 1873.

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

Mr. E. W. Binney, F.R.S., F.G.S., said that in the Liverpool papers a good deal of discussion had lately taken place on the rapid growth of the bar at the entrance into the Queen's Channel. This is a subject not only of local but of national interest. Having had occasion to pass over the bar frequently in his passage to the Isle of Man, he had his attention directed to the matter, and his friend our worthy President had supplied him with a copy of an interesting article from the *Liverpool Albion* of the 18th November last. Adam Evans appears to attribute this evil to the emptying of mud into the Mersey, and the writer in the newspaper states as follows: "Nothing is so important to the prosperity of the port of Liverpool as the maintenance of its entrance channels at sufficient depth. Several times lately the question has been named at the Dock Board, and more particularly at the meeting of the Board last week. It appears that Admiral Evans, acting Conservator of the River Mersey, has been for a long time urging the construction of steam barges to take the deposits from the docks (which deposits consist not of soluble kinds only, but also of coal, coal dust, and all other matter which happens to fall overboard from the ships in the docks), and to convey them outside the port. That this is no new view of Admiral Evans's is shown by the fact that in his report for 1843, as to the impropriety of discharging mud and other matter from the flats into the river, he pointed out the dangers attending the practice; and if at that time he considered the system bad, how much more so is it now? The deposits then were about 200,000 tons annually; now—the acreage of the docks having increased from about 100 to 500 or 600 acres—they are probably at least 600,000 tons annually, and all this refuse is discharged into the river. To elucidate the matter we may give the following extracts from Admiral Evans's report of 1843 relative to the causes which to the extension of the Victoria Bar, and of the effects of depositing mud and other substances in the bed of the river: "The extension of Victoria Bar is occasioned by a greater quantity of detritus being held in suspension by the ebb than by the flood tide, which causes the point of deposit to continually extending outwards to where the stream of ebb is exhausted. Although this to a certain extent is of common occurrence at the mouth of all estuaries, yet the excess of matter held in solution by the ebb over the flood tide in the Mersey is in a great measure to be attributed to the habit of throwing into the river the whole of the mud and sand 'daily' extracted from the Liverpool docks, amounting from January, 1843, to January, 1844, to 213,000 tons! This enormous quantity is taken out of the docks by steam dredges, discharged into mud flats, which are towed out by steamers every tide, day and night, except on Sundays, and emptied into the middle of the river at the worst possible time of tide—namely just before high water, so that part of the mud is taken up the river to feed the Pluckington and other banks; part is precipitated to the bottom, taking with it the sand held in suspension by a full tide; and the remainder is filtered through the different seaward channels, leaving a portion of its silt in them.

This practice, so mischievous to the navigation of the Mersey, and detrimental to the best interests of Liverpool, is continued under the sanction of a local Act of Parliament (6th of George IV., cap. 117), which was passed in the year 1825. Since the passing of that Act the dock space of Liverpool has not only been doubled, and annually increasing, but the ships are of a much larger class, requiring a greater depth of water both in the docks and channels. If, therefore, the Act of Parliament above alluded to applies to the docks made and deepened since its passing in 1825, to those docks now in progress, and to all that may be hereafter be constructed, the amount of clay and mud thrown into the river will be enormous, endangering the navigation at the very time, and in proportion as the increase both in size and number of the ships requires the channel to be kept as deep and clear of deposit as possible.' In consequence of this report the practice of discharging mud boats on the flood tide was abandoned, and they have since been discharged during the first quarter of ebb. That the views then adopted by Admiral Evans were right is shown by the present condition of the main or Queen entrance channels into Liverpool, which are now only about seven to eight feet on the bar at low water spring tides, instead of about twelve feet, so that small tug boats cannot now get in and out where comparatively large steamers used to be able to pass. An illustration of this fact was afforded the other day, when one of the Isle of Man boats had to wait some time before she could cross the bar. With regard to Rock Channel, the depth of water on the bar is now ouly 18 inches at low water spring tides. It may be thought that in going back to Admiral Evans's report for 1843 we have travelled out of our way to manufacture arguments against the present system of depositing mud in the Mersey; but if we return to the Admiral's report for 1872, we find the views of 1843 confirmed."

No doubt the throwing in of about 2000 tons of mud into the Mersey near Liverpool every working day is a thing which ought not to be allowed by the conservator of that river, but it is highly probable that this is only one of the causes of the decreased depth of water over the bar of the Queen's channel. It is now pretty well known that encroachments and embankments on estuaries and river courses cannot be made without some detriment to such places. The natural flow of water is generally the best for cleansing and sweeping out a river or estuary, and such agency may be helped and diverted, but it can never be opposed or obstructed with impunity, Owing to the want of reliable experiments it is not yet well known at what speed a current of water ceases to cut a bed of clay and begins to lift up the course of such water. All we know broadly is that mountain streams deepen and low country streams raise their beds.

The estuary of the Mersey and its deep channels have been kept open chiefly by the ebb tide and the back waters of the Mersey, Weaver, and other streams. Of late years the shores on both sides of the Mersey have been extended into the water way by the construction of new docks and other works so that the channel is now considerably less in width than it formerly was, and consequently less water enters the river from the sea and flows up it than used to do, and thus diminishes the power of the ebb tide. True it is that the contraction of the river has made the current of the ebb for some time stronger opposite to Liverpool, and thereby increased the scour and cutting power there, but the materials removed soon fall to the bottom when the water course becomes wider and the current less, so this increased scour will not compensate for the loss of water caused by the obstructions to the flood tide but actually increase, to a considerable extent, the deposits of the removed materials in the vicinity of the bar. It is quite clear that the emptying of dock mud into the river is an evil, but what is it when compared to the solid matter brought down daily and in freshes by the inland streams feeding the Mersey.

My object in making these remarks is to direct attention to the subject of the lowering of the depth of water over the Queen's Bar, which is of the utmost importance to steamers leaving and arriving at the tidal harbours about high water and reaching the bar at near low water. This is especially the case with the Isle of Man steamers during the greater part of the year. No doubt that particular winds may at one time increase and at another diminish the sand on the bar, but it appears pretty certain that for some considerable number of years the water has gradually become shallower at the entrance of the Queen's Channel. The true cause of this evil has first to be ascertained and then no doubt the authorities at Liverpool will lose no time in remedying it.

Mr. BINNEY also exhibited to the meeting a very perfect specimen of a fossil fish, measuring from head to tail 14 inches. It was discovered in the cannel seam of the Pirnie Coal Company at Methill in the county of Fife. He considered it to be a small *Magalichthys Hibberti.* Although fragments of this fish are very frequently met with in the coal measures, it is not often that a specimen is found in so good a condition.

Ordinary Meeting, January 13th, 1874.

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

Mr. Rooke Pennington, of Bolton, was elected an ordinary Member of the Society.

A drawing was shown representing some further improvements of Dr. Joule's mercurial air exhauster described in the Proceedings of February 18, 1873.

In the section represented by Fig 1, W W is a wooden frame; P a pulley for raising or lowering a flask of mercury held in a wooden box, M, working in a slide; s s s s are india rubber stoppers; E is the exhauster; *t*, *e* the entrance and exit tubes; *g* the gauge; *f* a funnel to admit sulphuric acid; B, B moveable brackets to support any apparatus.

In Fig 2 the exhauster is drawn to a larger scale. *t*, *e* are the entrance and exit tubes, fitting tightly in an india rubber disc *a*, which disc is kept tightly pressed against the exhauster by means of the ring, *b*, *b*. The mercury is represented sunk below the entrance tube, as is the case when the moveable flask is in its lower position. On raising the flask by means of the pulley, the mercury rises in the exhauster and forces any air it may contain into the upper part of the exhauster by raising the india rubber plug. The air then makes its exit through the pipe *e*. This latter is also used for withdrawing the acid which gradually accumulates.

Fig 3, also drawn to a large scale, represents a convenient means of introducing sulphuric acid for removing aqueous vapour, or to let air into the apparatus. The orifice at the bottom of the funnel is about $\frac{1}{100}$ of an inch diameter to prevent violent action.

It may be useful to mention that the junctions are made with black india rubber tube fastened by softened iron wire.

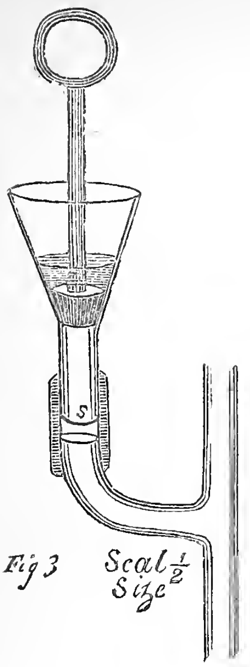


Fig 3 Scale $\frac{1}{2}$ Size

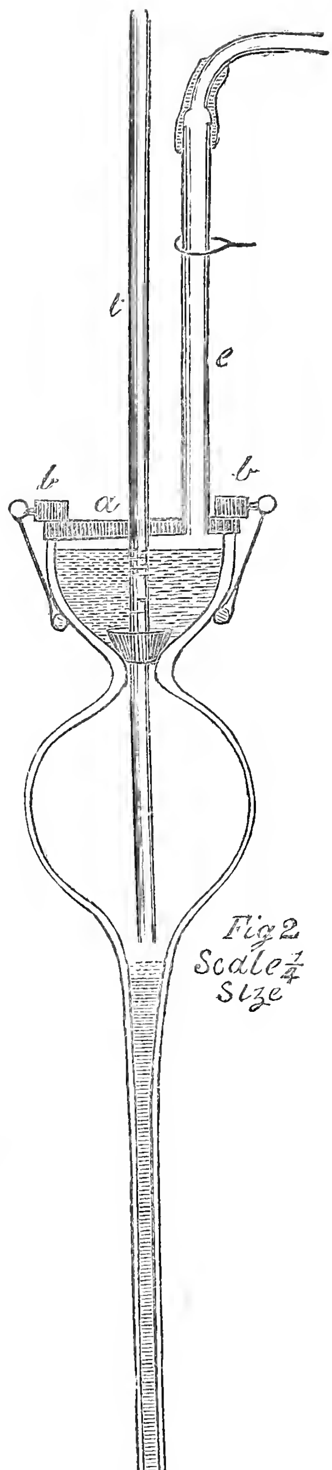


Fig 2 Scale $\frac{1}{4}$ Size

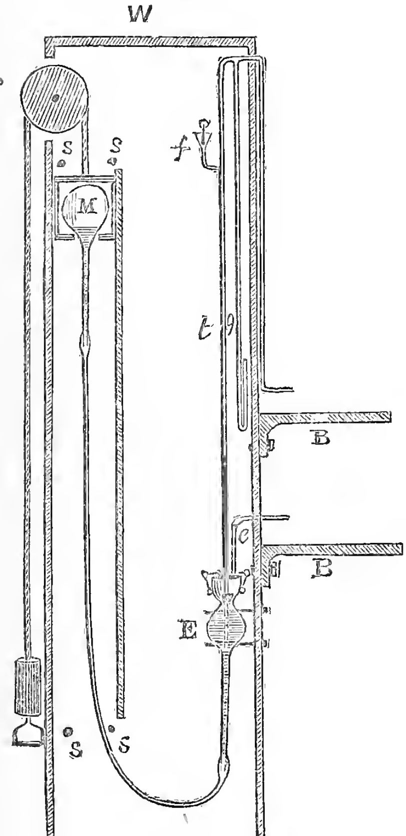


Fig 1. Scale $\frac{1}{24}$

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

In a paper published in the Proceedings of the Society for March 4th, 1873, I mentioned that if a piece of steel wire be immersed in hydrochloric or sulphuric acid for ten minutes or more, and then well washed with water and dried, that on breaking it bubbles were not seen to rise through the moisture on the surface of the fracture as was the case with *iron* wire. Subsequent experiments made under the microscope with a power of 250 diameters, however, show that very small bubbles are given off with great rapidity, sometimes from the whole, sometimes from part only of the fractured surface.

This difference in the behaviour of iron and steel is most likely connected with the difference of molecular structure. Thus the fracture of a steel wire containing say ·75 per cent of carbon, when seen under the microscope presents a tolerably flat surface, composed of innumerable small, sharp crystalline points, while that of iron is rough, more or less fibrous or mossy, and the fibres do not end in sharp points. These fine crystalline points in the steel, as is well known, must facilitate the evolution of bubbles; consequently they are very small, rapidly given off, and hence invisible to the naked eye, whilst the absence of these points in iron causes the small bubbles to collect into larger ones, which are readily seen.

The less carbon a steel contains the more its fracture will resemble iron, so in a steel containing only ·21 per cent. of carbon, small bubbles may sometimes be seen by the naked eye.

About 5 oz. of iron wire ·125 in. diameter, after 10 days' immersion in hydrochloric acid 1·20 s.g., was well washed in water, dried and placed in a glass tube heated to a temperature of a little over 100° C. by a sand bath. Each end of the tube was connected with a bottle containing nitrate of silver solution. A current of air was then slowly drawn through the tube for two to three hours without forming any precipitate however of chloride of silver; but the surface of the iron was covered with a coating of oxide, or in all probability, oxichloride of iron.

Thick pieces of iron ·450 in. diam. were found to redden blue litmus paper slightly when applied to the fracture after the iron had been immersed 12 hours in hydrochloric or sulphuric acid.

Mr. J. Carter Bell communicated a series of meteorological observations which had been made daily during the months of May, June, July, and August, 1873, at Tumaco, New Granada, S. America, by Mr. G. Wilcynski.

"On Crystalline Sublimed Cupric Chloride," by S. Carson, (Student in the Chemical Laboratory of the Owens College).

Cupric Chloride prepared by burning copper in dry chlorine gas or by heating the anhydrous salt to 200° is obtained either as a brown sublimate or as a brownish yellow powder.

A mass of a copper compound crystallized in needles, many exceeding 5mm. in length, as found in the decomposer of the Deacon-Chlorine process was forwarded to Professor Roscoe, by Mr. Worsley, of the Netham Chemical Works. The crystalline mass was collected from the space above the marbles, impregnated with copper sulphate at the top of the compartments. The temperature of this space is always necessarily a little lower than that of the spaces between the marbles where the action takes place, and to this is attributed the deposition of the compound.

I have made several analyses of these crystals, the results of which show that they consist of anhydrous cupric chloride, mixed with a small quantity (about 2 per cent) of an insoluble oxychloride. The following is a mean of several analyses of the soluble portion:—

	Calculated.	Found.
Copper	47·172	46·909
Chlorine	52·828	53·091
	100·000	100·000

The formation of this crystalline sublimate appears to take place as soon as the temperature of the marbles covered with sulphate of copper reaches about 800°. In all probability a formation of copper chloride is constantly occurring as a necessary step in the decomposition of the hydrochloric acid and air, and when the temperature reaches the volatizing point of the chloride these crystals appear.

As the greatest amount of decomposition of the hydrochloric acid takes place close upon the temperature at which the chloride sublimes, the formation of this salt and the consequent loss of copper has been a fertile source of annoyance to the manufacturer.

Mr. Deacon has recently completely overcome this difficulty by thc addition of sodium sulphate to the copper sulphate with which the marbles are impregnated.

The presence of this salt prevents any formation of copper chloride, sodium chloride being volatilized, and copper sulphate remaining behind. This reaction is well seen by the change of colour from green to blue produced when a solution of sodium sulphate is added to one of cupric chloride.

"Memorandum on Brown-stapled Cotton," by Major R. Trevor Clarke. Communicated by Dr. E. Schunck, F.R.S.

The nankeen colour in cotton staple may be considered, I think, as a normal variational state, rarely met with in the present day, but very probably natural to the wild forms now nearly extinct and very imperfectly known. The two plants which have the best claim to be considered wild species that I have met with, namely, the Polynesian plant of Nuttall, and the Santo Paulo one of Mr. Aubertin, have both of them yellow cotton. The coloured state however is common to several cottons, probably to all. We find it in the hill cotton of Assam and in samples from China (Ning-Po), both *Goss. herbaceum* proper, and I am told it is not unfrequently found in the fields of this species in India and elsewhere.

In India however the sort actually in cultivation is the yellow form of *G. hirsutvm* (Orleans), and my samples from Malta are of the same kind. The only use that I know of for this staple is to make the cotton blanket clothing of the Afghans, and I think also the Kabyles use it for the same purpose.

It seems to be an object of regard, and even veneration, amongst the aborigines of various countries. In Peru the country people weave a striped cloth, white and yellow, from it, and the bodies of their ancient princes the Incas were found to have been buried enveloped in the rich brown wool of a coloured form of the large native plant.

The paler brown staple alluded to as coming from Africa is the produce of a coarse kind of Egyptian, and is of stronger quality than most. The curious kidney cotton also assumes this appearance, as I have samples of it from Parahyba del Norte, and have raised plants from its seeds. In all cases the staple suffers an unfavourable change when assuming this state, and is invariably more or less weak and short.

The occurence of the yellow state in so many different kinds will go far to account for the diversity in quality alluded to.

A tendency to brown coloration seems peculiar to the genus; it shows itself in the dark hue of the expressed oil, and when injury to the seed and capsule, by insects or otherwise, has occurred, the extravasation has resulted in stains upon the, properly, white fibre.

In the green seed capsule will be found two distinct series of colorific glands; one yellow, soluble in alcohol, the other purple, soluble in water; the brown is probably made up of these two.

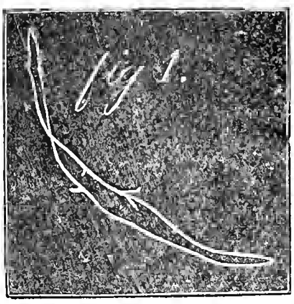
"On the Graphical Representation of the Movements of the Chest-Wall in Respiration," by ARTHUR RANSOME, M.D., M.A.

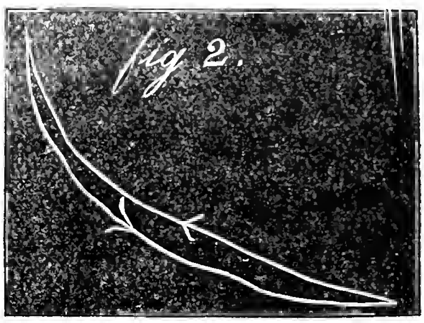
A stethograph has been constructed which gives an accurate tracing of the course described by any point on the chest-wall, in the forward and upward directions, *i.e.* in the vertical plane at right angles to the anterior surface of the chest.

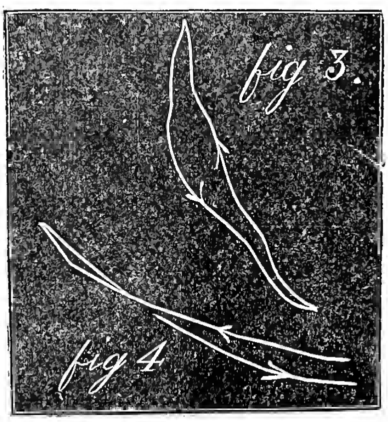
With this instrument many tracings have been made both in health and disease. The following, chiefly from the anterior ends of the third ribs, are selected as specimens of the results obtained, and to illustrate the conclusions drawn from them.

It has been observed—

1. That the anterior end of the rib takes a different course in its ascent from that of its expiratory descent. (Figs. 1 and 2.)





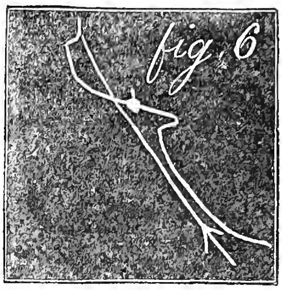


2. That in most cases the uppermost line is that of inspiration.

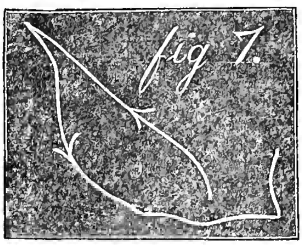
3. That this course is liable to variation in consequence of the action of the will. (Figs. 3 and 4.)

4. That in the action of *coughing*, after the inspiratory stroke, there is a slight forward bulging

of the rib, perhaps from the compression of the air in the lungs by the action of the Diaphragm, then a downward fall for a space of about 0·2 in., and afterwards an almost horizontal in-drawing of the rib (Fig. 5) quite unlike anything that could be produced by the simple angular movement of the rib.



5. That in *sneezing* the course of the rib is similar to that taken in the act of coughing, except that there is no forward bulging at the commencement of expiration (Fig. 6), and in the latter part of its course the rib only drops about 0·15 in. for 0·8 in. of in-drawing.



6. That in the dead subject, the movement of the end of the ribs, when simply raised and depressed (fig. 7), approximates to a segment of a circle.

7. That as age advances there is an approach to the form of curve traced by the unyielding rib, and the upward and downward strokes are more nearly alike.

From the above facts it follows that the horizontal in-drawing of the rib, in the spasmodic actions of coughing and sneezing, is impossible without an inbending of the rib itself, and that the variations in healthy breathing are chiefly to be ascribed to the influence of the muscles of forced expiration, which produce more or less bending of the rib during their action.

In disease a comparative feebleness of the respiratory track is to be noticed, and the want of elasticity of the chest is evidenced by the tendency to similarity in the upward and downward course of the ribs.

In acute Phthisis there is a degree of tremulousness in the tracings, and in Pleurisy is seen the effect of adhesions in the very small extent of forward push on the affected side.

The tracing of the cough of Phthisis is, however, similar in its form to that of the healthy chest (fig. 5) though much smaller and more feeble.

Ordinary Meeting, January 27th, 1874.

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

Mr. John Watts, Ph.D., was elected an Ordinary Member of the Society.

"On a Source of Error in Mercurial Thermometers," by THOMAS M. MORGAN, Student in the Laboratory of Owens College.

While engaged in distillation, a fact has come under my observation which, although it has been noticed before, does not appear to be very generally known, and has not so far as I have seen been recorded.

The thermometer, which was placed in a Wurtz tube so that the column of mercury was entirely surrounded by the vapour of the distilling liquid, was found after some days to indicate three degrees too little—a discrepancy caused by volatilization from the surface of the column of mercury and condensation on the upper part of the tube. By causing the mercury to flow to the end of the tube and back, the condensed portion was gathered up and the correct temperature indicated. It has since been observed that after each day of distillation, with liquids boiling between 60° and 100° C., a quantity of mercury equal to 1° or 1°·5 volatilizes, and that this quantity is scarcely perceptible when condensed on the surface of the bore. The thermometer in use was about the ordinary size with a scale of 360°.

I am informed that Geissler sometimes encloses a little hydrogen in his thermometers in order that volatilization may not go on so rapidly.

"Notes on fossil Lithothamnia (so-called Nulliporæ)," by ARTHUR WM. WATERS, F.G.S.

The organisms on the table before us have been assigned very various places in the organic or inorganic kingdoms at different times. The recent ones have perhaps most often been placed among the corals, while the fossil forms have passed more frequently as concretionary.

After shortly reviewing the opinions of some of the leading naturalists of this century, Mr. Waters drew attention to a paper of Rosanoff, published in 1866,* on the Melobesiacea, which he divides into three sections, the Melobesia, Lithophyllum, and Lithothamnium. In this paper full description is given as to their mode of growth and their reproduction. Since then Gümbel† has published in 1871 an excellent monograph on the fossil forms under the title, "Die sogenannten Nulliporen," in which he establishes 15 species. This paper has been largely used in preparing the present notes.

As most present will know, they attain their greatest development in the the Leithakalk, a meiocene formation which is principally, in some cases almost entirely, composed of this algæ. Unger says he has never seen any which contains less than two thirds. Thus formations of vast extent and many hundred feet thick, perhaps in some places thousands, are so largely composed of this limestone seaweed. But it is in no way confined to the Leithakalk, being also very abundant in the eocene, especially the upper division; the so-called granit-marmor, or Bavarian marble, a numulitic formation, is very largely composed of this concretionary-looking body. In North Italy it abounds in the eocene formations which are so largely developed in the Veronese and Vicentin. In many places the formation is some hundred feet, muc more than half composed of the

* Mem. de la Soc. Imp. des Sc. nat. de Cherbourg, t. xii.

† Abhand. d. R. Bay Ak. der Wissen, x., part i., 1871.

Lithothamnium. It occurs abundantly in Hungary and Switzerland. The so-called pisolithic limestone of Paris is according to Gümbel about eight tenths stone algæ; also M. Mario, Astrup; the pleiocene of Castel Arquato; and in fact it seems to be found in most of the tertiaries on the continent. It is further found in the chalk at Maestricht, and in the jurassic sponge beds at Schwabenbergs.

Doubtless when geologists have paid more attention to the question we shall find it has a very wide distribution, and has formed rocks of equal importance in other parts of the world.

Taking the Eastern Alps, with which I have a fair acquaintance, viz., the Bavarian, Austrian, and North Italian Alps, in which, as you know, the tertiary deposits attain extraordinary importance, I do not think it is too much to say that one sixth of the whole tertiary rocks of this district are composed of Lithothamnium. This is not given in any way as an exact calculation, but merely to give an idea of its importance.

The recent Melobesiacea seem to have a very general distribution. They grow at but moderate depths.

The Lithothamnium belongs to the family Melobesiacea of the order Florideæ or Rhodospermeæ; a large number of this order are limesecreting, but these (Lith.) differ very materially from the Corallina, since in the former the carbonate of lime is deposited in and between the cell structure, while with the Corallina the plant is filiform, and the lime is deposited round this, entirely encrusting it. It seems the structural difference is sufficient to separate them into two entirely distinct classes.

Gümbel, after saying how the stems and branches of a species are subject to variation as well as the superficial characteristics, says, "But by far the most important and numerous species can scarcely be distinguished by any other method than by the form and relative size of the cells, which can only be made out by transparent sections." We must be very careful how we accept this, for in a piece which I prepared from Torbole, on the Lake of Garda, a drawing of which is shown, we find the same plant producing distinctly three sizes of cells at right angles to one another, in the proportion 5 : 3 : 2.

According to Rosanaff the form of the young is always orbicular.

Gümbel gives the average of various analyses of the Lithothamnium nodosum from Vienna and M. Mario.

Lime	47·14
Magnesia	2·66
Alumina and Oxide of Manganese	2·55
Phosphoric Acid	0·06
Carbonic Acid	40·06
Insol. in Acid	4·96
Water and Loss	2·57
	100·00

The chief point to notice is the large amount of magnesia, representing 5½ per cent carbonate of magnesia. Gümbel points out the important bearing the large amount of magnesia which plants and animals can take up from the water may have upon the question of the formation of dolomitic rocks.

The fossil Lithothamnia are often much altered in colour. One rock on the Lake of Garda might almost be described as a black rock with white spots; an infiltration having taken place from the outside, turning all but the central portion a dark color, so that each piece when broken through shows the centre white, the rest dark. I bring this before your notice to show the great care that is required when judging from lithological characteristics, as no one would at first sight, and often not without microscopical examination, consider such a pure white rock and one almost black to be composed of the same material.

In conclusion, the object of this paper is to draw attention to the great masses of these bodies and the importance of always noticing their occurrence in geological formations, since it should be a very material help in regard to the climate, and the conditions of the coasts and currents, besides being of great stratigraphical assistance; nor is it of less importance to note carefully the growth of recent ones, for only through a knowledge of the present can we interpret the past.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 8th, 1873.

CHARLES BAILEY, Esq., Vice-President of the Section, in the Chair.

Mr. R. D. DARBISHIRE, F.G.S., exhibited a collection of shells and fragments of shells, lent for this purpose by Miss M. H. Farington, of Worden Hall. The collection was in itself one of remarkable extent and beauty, as a representative of the fossil fauna of the Drift, and had lately been the subject of a Note read at a meeting of the Geological Society of London. The specimens were found in the Worden gravel pit near the Leyland Station, near Chorley, in a bed of shingle 240 feet above the sea level, shewed 42 species. Amongst these one, Fusus craticulatus, was decidedly Arctic, and the only form of that character. Cytherea chione, and Cardium rusticum were Southern, as was also Mactra glauca. These latter connected the list with that of the Macclesfield Cemetery beds. Fusus propinquus and Fusus antiquus, var. contrarius, which latter appeared here for the first time in English lists of this date, are found in the so-called manure gravels of Wexford.

He also exhibited similar collections made by J. Millard Reade, Esq., F.G.S., in the cuttings of the railway near Edgehill, and in cuttings in Toxteth park, both near Liverpool, 39 species at the former and 29 at the latter place. The Edgehill list includes, like the Leyland list, Saxicava norvegica, Cytherea chione, and Cardium rusticum.

The two latter species occurred in comparison with other species "frequently"; and had now been noted at Edgehill in "glacial" clay and at Leyland and Macclesfield in gravels. Neither of them had yet been recognised in the Blackpool beds, from which also a small collection was exhibited.

January 19th, 1874.

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

Mr. PERCIVAL exhibited specimens in fruit of *Hypnum heterophyllum*, gathered this month on wet rocks, Turton, near Bolton, a species which has very rarely been found in fructification.

Mr. SIDEBOTHAM exhibited a specimen of silicious nummulitic rock, from the banks of the Euphrates, the polished section of which exhibited the structure of the shells in a remarkable manner.

JOSEPH SIDEBOTHAM, F.R.A.S., read a paper on "The similarity of certain Crystallised substances to Vegetable forms."

The author alluded to the many well known dendritic markings in shale and slate, which are often mistaken for sea-weeds and mosses, and to the leaf-like forms in various crystallisations. He then called special attention to some experiments carried on by the late Mr. Petschler and himself, on the crystallisation of bichromate of ammonia, nitrate of silver, and other salts, when in combination with gelatine and other colloid substances. The results of these experiments were brought before this society in the year 1861, but so far as he was aware the subject had not been pursued further.

The author then called attention to the formation of verdegris on insect pins, in old Entomological collections. This substance makes its appearance where the pins pass through the thorax of the insects, and in length of time grows into a considerable mass of flocculent matter, of a brilliant green colour, and often breaks up the insects and also destroys the pins. It consists mainly of acetate or formiate of copper in combination with fatty or oily matter.

On examination of various specimens under the microscope, they were found to present a great variety of forms, filamentous and ribbon-like structure, often resembling various fungi, in some cases so nearly, that it was difficult to believe that the fibres and fruit-like forms are not really organic bodies.

Drawings, and specimens under the microscope, were exhibited, and the author expressed his opinion that these bodies were simply crystals, modified in their formation by the oil contained in the insects, with which the crystals is in some way combined. Some of the specimens exhibited were taken from insects collected twenty-five years ago.

An interesting discussion followed the paper, in which the Chairman, Mr. Plant, Mr. Rogers, and other members, took part.

PHYSICAL AND MATHEMATICAL SECTION.

October 14th, 1873.

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

Mr. Samuel Broughton was elected Treasurer of the Section, in place of the late Mr. Thomas Carrick.

"Mean Monthly Barometric Readings at Old Trafford, Manchester, from 1849 to 1872," by G. V. VERNON, F.R.A.S., F.M.S.

Having a tolerably complete register of barometric readings, and knowing of no published normal values for any station in or near Manchester, I have reduced and tabulated the monthly means for the period above named.

The barometer with which the observations were made was a standard by Negretti and Zambra, No. 266, and which had been compared at Greenwich.

All the observations have been corrected for capillarity and index error to reduce them to the Greenwich standard, and then reduced to 32° F.

In the earlier part of the series I have unfortunately some months deficient, but in the latter part of the series I have been able to fill up the gaps by the use of the very careful series of observations made at Eccles by my friend Thomas Mackereth, Esq., F.R.A.S., by applying corrections determined by comparing the months in which the observations were simultaneous.

I found that the two series of observations were nearly identical after allowing for difference of level.

The mean annual pressure for the entire series was 29·780 inches, which, corrected for an altitude of 123 feet above the mean sea level, and assuming the mean annual temperature to be 50° F., gives us 29·912 inches as the mean yearly value reduced to sea level.

If we take the complete period of the last eleven years we have a mean reading of 29·838 inches. which is 0·074 inches below the value given for the entire period 1849 to 1872.

In the earlier series the month of August was generally wanting in the observations except for a few years, but this month having a reading generally in excess of the yearly value would tend if anything to make the annual value somewhat higher, and the result would tend to show that during recent years the pressure has been lower than during the earlier part of the series.

The variation between the highest and lowest mean yearly readings during the last eleven years is 29·828 inches (1870) and 29·624 inches (1872), or equal to 0·204 inches: this would appear greater than probability would point out, 1872 having had an exceptionally low mean reading; leaving 1872 out, we have a difference of 0·118 inches between 1870 and 1866.

Looking at the mean monthly values, we find that the maximum occurs in June and the minimum in January; the order beginning with the minimum is January, October, March, December, November, September, February, July and April equal, also May and August equal. If fine weather depends upon barometric pressure, the above shows that May and August should be the finest months as a rule, and January and October the worst: May is next to the driest month, April, and October bears off the palm for being the wettest, at least in this district.

The remarkable year for rain, 1872, would appear to have been also remarkable for deficient atmospheric pressure, as the table annexed will show, every month except August having had a pressure below the average, and August was only a few thousandths of an inch above the average.

1872.	Difference from Mean. Inches.
January	– 0·315
February	– 0·205
March	– 0·139
April	– 0·032
May	– 0·050
June	– 0·107
July	– 0·017
August	+ 0·006
September	– 0·161
October	– 0·195
November	– 0·291
December	– 0·367

Comparison of these departures below the average with the separate monthly values of the rainfall for 1872 does not show that the greatest rainfall occurred in those months in which the pressure was the most below the average, but the almost constant depression throughout the year certainly does so. It is probable that if a longer and more complete register could be examined, that 1872 had a lower mean pressure than any year over a very long period, just as it had a greater rainfall than any year for a very long period, say 80 years.

BAROMETER MEANS AT 32° F., CORRECTED FOR INDEX ERROR, BUT STILL REQUIRING THE REDUCTION TO THE LEVEL OF THE SEA.
HEIGHT OF BAROMETER CISTERN 123 FEET ABOVE MEAN SEA LEVEL.

Year.	Jan.	Feb.	Mar.	April	May.	June.	July.	Aug.	Sep.	Oct,	Nov.	Dec.	Mean.
	ins.	ins.	ins.	ins.	ins.	ins.	ins.	ins.	ins.	ins.	ins.	ins.	
1849	....	....	....	29·549	29·784	29·885	29·779	....	29·830	29·758	29·704	29·843	
1850	29·876	29·752	30·069	29·580	29·740	29·890	29·856	....	29·958	29·715	29·775	29·889	
1851	29·540	29·883	29·573	....	29·930	....	29·720	....	30·079	29·701	29·837	30·137	
1852	29·524	29·873	30·072	30·109	29·819	29·527	29·885	....	29·793	29·718	29·420	29·463	
1853	29·527	29·598	29·801	29·702	29·843	29·710	29·671	....	29·820	....	29·923	29·857	
1854	29·562	30·042	30·160	30·027	29·648	29·7[illegible]8	29·789	....	30·004	29·682	29·689	29·7[illegible]4	
1855	30·044	29·665	29·548	29·954	29·729	29·850	29·718	....	29·994	29·475	29·896	29·709	
1856	29·456	29·877	30·069	29·612	29·686	29·865	29·797	29·766	29·637	30·003	29·936	29·599	
1857	29·656	29·925	29·723	29·648	29·835	29·894	29·829	29·878	29·845	29·717	30·001	30·085	
1858	30·145	29·898	29·787	29·804	....	29·950	29·799	29·854	29·843	29·846	29·810	29·727	
1859	29·963	29·772	29·758	29·629	29·904	29·812	29·968	....	29·680	29·536	29·804	29·604	
1860	29·464	29.898	29·622	29·859	29·779	29·599	29·870	....	29·761	29·810	29·748	29·527	
1861	30·011	29·656	29·561	30·053	29·983	29·823	29·554	....	29·666	29·861	29·520	29·959	
1862	29·667	29·950	29·522	29·839	29·735	29·700	29·715	29·800	29·888	29·669	29·831	29·894	29·760
1863	29·567	30·101	29·727	29·814	29·889	29·720	29·994	29·710	29·599	29·583	29·817	29·864	29·782
1864	29·957	29·775	29·503	29·951	29·885	29·778	29·873	29·950	29·726	29·737	29·612	29·878	29·802
1865	29·392	29·741	29·784	29·994	29·729	30·092	29·790	29·730	30·100	29·470	29·714	30·053	29·799
1866	29·663	29·524	29·546	29·799	29·870	29·782	29·795	29·619	29·501	29·965	29·742	29·728	29·710
1867	29·508	29·787	29·696	29·573	29·782	29·976	29·733	29·796	29·905	29·737	30·173	29·891	29·796
1868	29·735	29·918	29·775	29·791	29·835	30·009	29·950	29·738	29·735	29·796	29·851	29·290	29·785
1869	29·779	29·741	29·745	29·842	29·712	29·949	29·930	29·994	29·564	29·897	29·708	29·631	29·791
1870	29·812	29·735	29·938	29·989	29·894	29·981	29·863	29·865	29·892	29·534	29·639	29·793	29·828
1871	29·665	29·804	29·859	29·643	29·947	29·820	29·664	29·868	29·784	29·761	29·864	29·902	29·798
1872	29·375	29·599	29·620	29·773	29·763	29·716	29·788	29·819	29·640	29·522	29·479	29·396	29·624
Means	29·690	29·804	29·759	29·805	29·813	29·823	29·805	29·813	29·801	29·717	29·770	29·763	

"Results of Meteorological Observations taken at Langdale, Dimbula, Ceylon, during the years 1868–72," by EDWARD HEELIS, Esq. Communicated by JOSEPH BAXENDELL, F.R.A.S.

The approximate situation of the place of observation is 6° 57′ N.; 79° 42′ E.; and elevation 4,600 feet above sea level.

MEAN TEMPERATURE.

	1869.	1870.	1871.	1872.	Mean.
January	...	63·78	64·13	65·52	64·48
February	...	65·27	64·79	65·88	65·31
March	...	67·26	66·84	66·84	66·98
April	...	68·36	68·14	67·73	68·08
May	...	68·33	67·65	69·03	68·34
June	...	66·15	64·66	66·55	65·78
July	...	64·48	62·99	65·78	64·42
August	64·02	64·73	65·55	65·40	64·92
September	64·55	64·07	64·12	64·66	64·35
October	64·25	63·85	65·21	64·63	64·48
November	64·59	64·08	65·77	65·18	64·90
December	65·06	64·32	65·64	65·34	65·09
Means......		65·39	65·45	66·04	65·59

MEAN DAILY RANGE OF TEMPERATURE.

	1869.	1870.	1871.	1872.	Mean.
January	...	15·73	14·94	17·49	16·05
February	...	20·39	21·89	19·24	20·51
March	...	21·19	18·62	23·05	20·95
April	...	21·49	18·15	15·80	18·48
May	...	14·56	12·76	16·71	14·68
June	...	11·44	7·45	9·56	9·48
July	...	9·55	7·08	10·12	8·92
August	8·95	10·63	11·10	9·43	10·03
September	9·29	9·81	11·55	8·74	9·85
October	12·46	11·07	15·70	9·52	12·19
November	13·95	17·27	13·41	13·13	14·44
December	17·15	16·43	15·53	14·52	15·91
Means......		14·96	14·01	13·94	14·29

Rainfall.

	1868.	1869.	1870.	1871.	1872.	Mean.	Mean No. of Rainy Days.	Greatest Fall in 24 hours.
	in.	in.	in.	in.	in.	in.		in.
January	...	2·48	10·03	8·98	0·65	5·53	13	2·48
February	...	1·30	2·15	1·40	1·34	1·55	7	1·07
March	...	1·49	2·43	2·12	0·93	1·74	11	1·30
April	...	7·94	2·93	6·01	9·46	6·58	15	2·96
May	4·68	4·93	3·46	7·85	5·34	5·25	17	2·17
June	11·60	20·02	11·44	21·67	15·64	16·07	25	5·10
July	9·27	18·19	10·52	34·05	8·69	16·14	25	3·77
August	5·53	10·54	8·79	11·04	9·54	9·09	24	1·50
September ...	13·52	14·38	16·99	9·17	24·29	15·67	25	5·85
October	8·57	17·92	16·62	9·87	12·85	13·16	24	3·14
November ...	4·56	9·17	6·85	8·56	10·42	7·91	20	2·06
December......	6·55	6·37	2·48	2·55	3·58	4·31	15	1·42
Sums		114·73	94·69	123·27	102·73	103·00	221	

Difference between Mean Maximum Temperature in the Sun and Mean Maximum Temperature in the Shade.

	1871.	1872.	Mean.
	°	°	°
January	...	27·4	27·4
February	...	31·1	31·1
March	...	27·6	27·6
April	...	28·8	28·8
May	31·6	25·4	28·5
June	18·6	15·6	17·1
July	17·0	15·7	16·3
August	25·6	14·8	20·2
September ...	24·4	14·5	19·4
October.........	29·2	21·8	25·5
November ...	24·6	30·9	27·7
December......	30·0	27·6	28·8

In a letter dated 22nd June, 1873, Mr. Heelis says, "The monsoon came in this year some 10 days before its usual time, a very unusual circumstance, as in the six previous years I have never known it to vary more than 2 days before or after the 1st of June. This year it burst on the 23rd of May, as far as I could determine, but so mildly that for some weeks I doubted its being really the monsoon. Since this date we have not had a single day without rain."

Erratum.—In the abstract of Dr. Ransome's paper "On the Graphical Representation of the Movements of the Chest-wall in Respiration," given in the last number of the Proceedings, the references to three of the figures are erroneous. The figure representing the act of coughing is No. 6, that of sneezing No. 7, and the motion of the dead rib is given in fig. 5.

Ordinary Meeting, February 10th, 1874.

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

"The Northern Range of the Basques," by W. BOYD DAWKINS, M.A., F.R.S., F.S.A.

The northern extension of the Basque race from their present boundary, in ancient times, is demonstrated by the convergent testimony of history, ethnology, and the researches into caves and tombs.

The Evidence of History as to the Peoples of Gaul and Spain.

In the Iberian peninsula the Basque populations (Vascones) of the west are defined from the Celtic of the east by the Celtiberi inhabiting modern Castille. In Gaul the province of Aquitania extended as far north, in Cæsar's time, as the river Garonne, constituting the modern Gascony, to which was added, in the days of Augustus, the district between that river and the Loire, a change of frontier that was probably due to the predominence of Basque blood in a mixed race in that area similar to the Celtiberi of Castille. The Aquitani were surrounded on every side, except the south, by the Celtæ, extending as far north as the Seine, as far to the east as Switzerland and the plains of Lombardy, and southwards, through the valley of the Rhone and the region of the Volscæ, over the Eastern Pyrenees into Spain. The district round the Phocæan colony of Marseilles was inhabited by Ligurian tribes, who held the region between the river Po and the Gulf of Genoa, as far as the western boundary of Etruria, and who probably extended to the west along the coast of Southern Gaul as far as the Pyrenees. They were distinguished from the Celtæ, not merely by their manners and customs, but by their small stature and dark hair and eyes, and are stated by Pliny and Strabo to have inhabited Spain. They have also left marks of their presence in Central Gaul in the name of the Loire (Ligur), and possibly in Britain in the obscure name of the Lloegrians. Their stature and swarthy complexion, as well as the ancient geographical position conterminous with the Iberic population of Gaul and Spain, confirm this conclusion. The non-Aryan and probably Basque population of Gaul was therefore cut into two portions by a broad band of Celts, which crosses the Eastern Pyrenees, and marks the route by which the Iberian peninsula was invaded.

The ancient population of Sardinia is stated by Pausanias to be of Libyan extraction, and to bear a strong resemblance to the Iberians in physique and in habits of life, while that of Corsica is described by Seneca as Ligurian and Iberian. The ancient Libyans are represented at the present day by the Berber and Kabyle tribes which are, if not identical with, at all events cognate with the Basques. We may therefore infer that these two islands were formerly occupied by this non-Aryan race, as well as the adjacent continents of Northern Africa and Southern Europe.

The Basque Population the Oldest.

The relative antiquity of these two races in Europe may be arrived at by this distribution. The Basques, or Ligurians, are the oldest inhabitants, in their respective districts, known to the historian; while the Celts appear as invaders, pressing southwards and westwards on the populations already in possession, flooding over the Alps, and, under Brennus, sacking Rome, and by their union with the vanquished in Spain constituting the Celtiberi. We may therefore be tolerably certain that the Basques held France and Spain before the invasion of the Celts, and that the non-Aryan peoples were cut asunder, and certain parts of them left — Ligurians, Sikani, and in part Sardinians and Corsicans — as ethnological islands, marking, so to speak an ancient Basque non-Aryan continent which had been submerged by the Celtic populations advancing steadily westwards.

At the time of the Roman conquest of Gaul, the Belgæ were pressing on the Celts just as the latter pressed the Basques, the Seine and the Marne forming their southern boundary, and in their turn being pushed to the east by the advance of the Germans in the Rhine provinces. Thus we have the oldest population, or Basque, invaded by the Celts, the Celts by the Belgæ, and these again by the Germans; their relative positions stamping their relative antiquity in Europe.

The Population of Britain.

The Celtic and Belgic invasion of Gaul repeated itself, as might be expected, in Britain. Just as the Celts pushed back the Iberian population of Gaul as far south as Aquitania, and swept round it into Spain, so they crossed over the Channel and overran the greater portion of Britain, until the Silures, identified by Tacitus with the Iberians, were left only in those fastnesses that formed subsequently a bulwark for the Brit-Welsh against the English invaders. And just as the Belgæ pressed on the rear of the Celts as far as the Seine, so they followed them into Britain, and took possession of the "Pars Maritima," or southern counties. The unsettled condition of the country at the time of Cæsar's invasion was, probably, due to the struggle then going on between the Celts and Belgæ.

Basque Element in present British and French Populations.

The Basque non-Aryan blood is still to be traced in the dark-haired, black-eyed, small, oval-featured peoples in our own country in the region of the Silures, where the hills have afforded shelter to the Basque populations from the invaders. The small swarthy Welshman of Denbighshire is in every respect, except dress and language, identical with the Basque peasant of the Western Pyrenees, at Bagnères de Bigorre.

The small dark-haired people of Ireland, and especially those to the west of the Shannon, according to Dr. Thurnam and Professor Huxley, are also of Iberian derivation, and, singularly enough, there is a legendary connection between that island and Spain. The human remains from the chambered tombs as well as the river-beds prove that the non-Aryan population spread over the whole of Ireland as well as the whole of Britain. The main mass of the Irish population is undoubtedly Celtic, crossed with Danish, Norse, and English blood.

The Basque element in the population of France is at the present time centered in the old province of Aquitaine, in which the jet-black hair and eyes, and swarthy complexion, strike the eye of the traveller, now, as in the days of Strabo, and form a vivid contrast with the brown hair and grey eyes of the inhabitants of Celtica and Belgica. The map published by Dr. Broca ("Memoires d'Anthropologie," t. I., p. 330) shows at a glance the average complexion prevailing in each department, and the relative number of exemptions per 1,000 conscripts, on account of their not coming up to the standard of height (1·56 metre = 5 feet $1\frac{1}{2}$ inches), and it will be seen that the only swarthy people outside the boundary of Aquitaine, constitute five ethnological islands. Of these Brittany is by far the largest, probably because its fastnesses afforded a shelter to the Basques, who were being driven to the south-west. The department of the Meuse in the north, and those of Tarn and Arriège, in the south, are also sundered from the main body, while those of the Upper and Lower Alps present us with the descendants of the ancient Ligurian tribes.

The people with dark-brown hair, considered by Dr. Broca to be the result of the intermingling of a dark with a fair race, are scattered about through Aquitaine, and occur only in two departments in northern Celtica. The fair people, on the other hand, are massed in northern Celtica and Belgica.

The relation of complexion to stature may be gathered from the following table of exemptions per 1,000 for each department:—

Départements noirs	98·5 to 189
„ gris foncés	64 „ 97
„ gris clairs	48·8 „ 63·8
„ blancs	23 „ 48·5

From this table it is evident that the swarthy people are the smallest and the fair the tallest, the intermediate shades being the result of fusion between the two extremes.

The distribution, therefore, of the small swarthy Basque, and tall fair Celtic and Belgic races in France at the present time, corresponds essentially with that which we might have expected from the evidence of history.

When we consider the many invasions of France, and the oscillations to and fro of peoples, the persistence of the Basque population is very remarkable. It is not a little strange that the type should be so slightly altered by intermarriage with the conquering races.

Researches in Neolithic Caves and Tombs.

The evidence offered by an appeal to history and ethnology, as to the former northern extent of the Basque peoples, is confirmed by an examination of the human remains in the

Neolithic caves and tombs, scattered throughout the area under consideration. The discoveries in the caves of Gibraltar and of the Spanish Mainland prove that a small, long-headed race, with delicate features and orthognathic profile identical with the Basques who buried their dead in the modern cemetery of Guipuscoa ranged throughout the Peninsula, using with indifference caves and chambered tumuli for their tombs. And on the same grounds their former range through France, Britain, and Ireland, is demonstrated, and as far to the east as Belgium. They occupied the whole of this region in the Neolithic age, in which they were invaded and driven to the westward, and broken up into islands by the Celts, a fair, tall, broad-headed race. The Basques, therefore, have lived in Europe since the Neolithic age, history, ethnology, and researches in caves and tombs, offering independent and convergent testimony. At the present time the Basque blood asserts itself in the physique of certain isolated populations, and within the historic period is demonstrated to have been more strongly defined, and to have occupied larger areas, and lastly in the prehistoric period to have formed one continuous race from the Pillars of Hercules, as far north as Scotland, and as far to the east as Belgium.

PHYSICAL AND MATHEMATICAL SECTION.

February 3rd, 1874.

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

"On the Theory of the Tides," by DAVID WINSTANLEY, Esq.

According to that theory of the tides which appears to have met with something like general acceptance, the waters of the ocean are heaped up on opposite sides of the earth by those differences of lunar attraction which result from inequalities of distance, "the waters being pulled from the earth on the one side and the earth from the waters on the other."

In any of those expositions of this theory which have come under my notice I have failed to observe any mention of that resistance to the lunar attraction without the consideration of which it appears to me that the theory in question must be regarded as incomplete. One of the known results of the mutual gravitation of the earth and moon is found in the instance of the former body in an oscillation of its major portion on both sides of a given line, viz. the orbital path pursued by the centre of gravity of the binary system. The oscillation in question is certainly resisted by the tendency of the earth's particles to continue their motion in right lines, and the amount of this resistance clearly increases with the extent or violence of the oscillation, which attains its maximum at that portion of the earth's surface most remote from the moon, and sinks to zero at the common centre of gravity of the system. Without these differences of resistance to the forced oscillation caused by the lunar influence, it seems to me the differences of lunar attraction would fail to produce the effects ascribed thereto.

If this be so, there can be no doubt as to the desirability of making mention in any exposition of the tidal theory both of the resistance I have mentioned and its differences in extent, and the more especially as we know on the highest modern astronomical authority that "strange difficulties" are experienced by many of those who attempt from the expositions now in use to gain a knowledge of the *modus operandi* of the tides. The tendency to continued rectilinear movement to which I have alluded as a resistance to oscillation is however well known by the name of "centrifugal force," which term, or any of its modern substitutes, may, it seems to me, be conveniently employed in considering the theory of the tides.

If we discard for the time all considerations of how it has come to be so, and regard only the fact that the earth and moon together constitute a system which revolves upon its common centre of gravity, I am of opinion that we shall have unmixed with distracting matter all the facts that are necessary for theorising on the production of the lunar tides. It will be clear that the revolution in question will be accompanied by the development of centrifugal force, and the whole point to which attention is now directed is whether or no the said centrifugal force contributes in any material manner to the phenomena of the tides. It is needless to complicate the matter by any attempt to reduce the value of this centrifugal force to an expression of the motion or pressure of a number of tons or pounds. We know that at the earth's centre it is just sufficient to overcome that tendency to juncture which is the simple resultant of the gravitation subsisting between the earth and moon. Were it greater, the interval of separation would increase; whilst were it less, it would diminish. The constancy of this interval* proves the equality at the earth's centre at any rate of the attraction which perturbs and the centrifugal force which offers resistance to perturbation.

For convenience of expression we may apply the term "lunar unit" to indicate the quantity of either of the forces we have named. It will at once be evident that on the side of the earth nearest the moon there will be experienced the effect of more than a lunar unit of attraction and less than a lunar unit of resistance. Indeed the centrifugal force which at the further side of the earth is opposed to the attraction of the moon here augments the effects produced thereby, so that we have a tide-producing force equal to the sum of the forces in question. On the other side of the earth, however, we have less than a lunar unit of attraction, and more than a lunar unit—in fact more than two lunar

* The writer is not unaware of the ellipticity of the moon's orbit.

units of resistance; so that we have there a tide-producing force due to the resistance alone and equal in amount to the difference between that resistance and the amount of the lunar attraction.

A very little reflection will suffice to show that in whatever part of the earth's figure the common centre of gravity of the system may be found, the tide-producing energy resulting from the forces here named will be substantially the same on both sides of the earth. For instance, if we regard the figures representing the earth's diameter, the moon's distance, and the mass of both bodies, as being correctly given in the tenth edition of Herschel's "Outlines," we shall find the centre of gravity of the system to be 1249 miles below the surface of the earth. Here we have no centrifugal force at all, but at a distance beyond of 2713 miles (*i.e.* at the centre of the earth) we have one lunar unit and no more. Under the moon, where the attraction is 1·034 lunar units, the augmenting centrifugal force is ·46 of a lunar unit, and the tide-producing force equal to the amount of their sum, or 1·494 times the same standard of measure. On the further side of the earth, however, the centrifugal force equals 2·46 times the amount experienced at the centre, whilst the attraction of the moon at the same place is ·968 of that amount. The difference of these forces or 1·492 lunar units indicates the comparative amount of force available for raising the external terrestrial tide.

"Rainfall at Old Trafford, Manchester, in the year 1873," by G. V. Vernon, F.R.A.S., F.M.S.

The rainfall of 1873 was 5·950 inches below the average of the last 80 years, and no less than 21·877 inches below the great fall of 1872.

The total fall in 1873 was 29·815 inches, and fell upon 198 days; although the fall was below the average it fell upon an unusual number of days, and was 6 days above the average of the last twelve years.

January had a rainfall in excess of the average; February, March, April, and May had a fall below the average; June, July, and August were all in excess, and this joined to very little hot weather, excepting a short period in July, spoiled the prospects of the harvest; September had a fall below the average, October was above; November and December had a rainfall greatly below the average of the season, December especially, and excepting April was the driest month of the year.

The rainfall for the first quarter of the year was 1·628 inches below the average; for the second quarter, 1·914 inches below the average; for the third quarter, 0·937 inches above the average; whilst for the fourth quarter it was 3·445 inches below the average.

This distribution of the rainfall was unfortunate as the heaviest amounts fell at the time when the crops generally should have been ready for cutting: fortunately the weather suited the hay crop, which was generally a very good one, and there was plenty of after-grass.

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

Quarterly Periods.		1873.	Fall in Inches.	Average of 80 Years.	Differ-ence.	No. of Days Rain fell in 1873.	Quarterly Periods.		
1872.	1873.						80 Years.	1873.	Differ'nce
			ins.	ins.	ins.		ins.	ins.	ins.
56	48	January..	3·136	2·544	+0·592	21	7·220	5·592	—1·628
		February.	0·666	2·388	—1·722	11			
		March....	1·790	2·288	—0·498	16			
50	43	April......	0·516	2·045	—1·529	12	7·207	5·393	—1·914
		May	1·910	2·297	—0·387	17			
		June	2·967	2·865	+0·102	14			
59	64	July	4·649	3·572	+1·077	20	10·388	11·325	+0·937
		August ...	4·199	3·509	+0·690	27			
		Septemb'r	2·477	3·307	—0·830	17			
63	43	October...	4·441	3·899	+0·542	20	10·950	7·505	—3·445
		November	2·283	3·765	—1·482	13			
		December	0·781	3·286	—2·505	10			
228	198		29·815	35·765	—5·950	198	35·765	29·815	—5·950

Ordinary Meeting, February 24th, 1874.

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

Mr. A. Brothers, F.R.A.S., referring to a statement made by Mr. Dawkins, F.R.S., at the last meeting, that the sinking of a deep well recently at Sheerness had lowered the water in a well at Southend, read the following extract of a letter dated February 21st, 1874, which he had received from Mr. Joseph Beard, of Southend:—

"I have seen the agent of our water works and gleaned what particulars I could. Nothing has occured of recent date affecting our water at Southend; but about fifty years ago a well was sunk in the dockyard at Sheerness which by excessive pumping did drain all the deep wells at Southend. Our well at the water works was sunk fourteen years ago. It is 905 feet deep, dug to 385 feet, the rest a boring. The last 300 feet is through chalk, above that a layer of green sand. The water is particularly soft, and in washing you can't distinguish it from rain water. Nothing from Sheerness has disturbed this well since it was sunk, but the company had an accident at their beginning which may have given rise to what you have heard. They sank their first well 400 feet, and all at once broke into water that rose 200 feet. They tried to pump it out, but could not do so, and at last the bottom gave way and their pumping apparatus slipped down, where it remains unrecoverable at the present moment. They then started their labour again at a distance of 40 feet, but had great trouble with the quicksand through which they had to get to the chalk."

Mr. E. W. Binney, F.R.S., F.G.S., stated that of late years much had been said and written on the advantages of drainage in improving the sanitary condition of towns.

Now this, like many other good things, can be carried too far, especially in districts situate on a sandy soil and where the owners of crowded graveyards are permitted to bury corpses near to the public highway, a proceeding occasionally yet allowed by sanitary authorities. Some years since a main sewer was excavated on the Cheetham Hill Road. From the Workhouse to Temple it was made in strong brick clay or "till," but when it reached the latter place it came into a mass of quicksand and drained St. Luke's churchyard and took the water down to Manchester. The sewer was then continued in the sand all the way to the Bird in the Hand public house, where it stopped. Within the last month a main drain has been made from the turnpike road up Robin Hood Street so as to drain the water from part of St. Mark's churchyard. This drain runs into a new main sewer which joins the old one near the Bird in the Hand, and thus the drainage of part of St. Mark's churchyard is made to join that of St. Luke's, and both go down to Manchester. No doubt sand possesses a great filtering power, and will in some measure purify the water drained from churchyards; but water derived from such sources ought to be examined carefully before it is conveyed even into sewers connected with dwelling-houses. Probably our officers of health may have done so, and think that there is no harm in water flowing from graveyards, but as yet, so far as known to me, no information has been given to the public on this subject. The making of sewers in sandy soils near to burying grounds is a matter that deserves the attention of sanitary officers more than it has done up to this time.

Burial grounds cannot well be removed when once established, but their owners can be prevented from burying close to roads and streets having sewers under them, and caution can be used in making new burial grounds near to roads and sewers. When burial grounds adjoin churches, whatever the consequences, some people will build houses near them, preferring the contiguity of the church to any fear of the evils of a crowded cemetery.

"On the Effect of Acid on the Interior of Iron Wire," by Professor OSBORNE REYNOLDS, M.A.

It will be remembered that at a previous meeting of this Society Mr. Johnson exhibited some iron and steel wire in which he had observed some very singular effects produced by the action of sulphuric acid. In the first place the nature of the wire was changed in a marked manner, for although it was soft charcoal wire it had become short and brittle; the weight of the wire was increased; and what was the most remarkable effect of all was that when the wire was broken and the face of the fracture wetted with the mouth it frothed up as if the water acted as a powerful acid. These effects, however, all passed off if the wire were allowed to remain exposed to the air for some days, and if it were warmed before the fire they passed off in a few hours.

By Mr. Johnson's permission I took possession of one of these pieces of wire and subjected it to a farther examination, and from the result of that examination I was led to what appears to me to be a complete explanation of the phenomena.

I observed that when I broke a short piece from the end of the wire the two faces of the fracture behaved very differently — that on the long piece frothed when wetted and continued to do so for some seconds, while that on the short piece would hardly show any signs of froth at all. This seemed to imply that the gas which caused the froth came from a considerable depth below the surface of the wire, and was not generated on the freshly exposed face. This view was confirmed when on substituting oil for water I found the froth just the same.

These observations led me to conclude that the effect was due to hydrogen, and not to acid as Mr. Johnson appeared to think, having entered into combination with the iron during its immersion in the acid, which hydrogen gradually passed off when the iron was exposed.

It was obvious however that this conclusion was capable of being further tested. It was clearly possible to ascertain whether or not the gas was hydrogen; and whether hydrogen penetrated iron when under the action of acid. With a view to do this I made the following experiments.

First, however, I would mention that after 24 hours I examined what remained of the wire, when I found that all appearance of frothing had vanished and the wire had recovered its ductility, so much so that it would now bend backwards and forwards two or three times without breaking, whereas on the previous evening a single bend had sufficed to break it.

I then obtained a piece of wrought iron gas pipe 6 inches long and $\frac{5}{8}$ inch external diameter, and rather more than $\frac{1}{16}$ of an inch thick; I had this cleaned in a lathe both inside and outside; over one end I soldered a piece of copper so as to stop it, and the other I connected with a piece of glass tube by means of indiarubber tube. I then filled both the glass and iron tubes with olive oil and immersed the iron tube in diluted sulphuric acid which had been mixed for some time and was cold. Under this arrangement any hydrogen which came from the inside of the glass tube must have passed through the iron.

After the iron had been in the acid about 5 minutes small bubbles began to pass up the glass tube. These were caught at the top and were subsequently burnt and proved to be hydrogen. At first, however, they came off but very slowly, and it was several hours before I had collected enough to burn. With a view to increase the speed I changed the acid several times without much effect until I happened to use some acid which had only just been diluted and was warm; then the gas came off twenty or thirty times as fast as it had previously done. I then put a lamp under the bath and measured the rate at which the gas came off, and I found that when the acid was on the point of boiling as much hydrogen was given off in 5 seconds as had previously come off in 10 minutes, and the rate was maintained in both cases for several hours.

After having been in acid for some time the tube was taken out, well washed with cold water and soap so as to remove all trace of the acid; it was then plunged into a bath of hot water, upon which gas came off so rapidly from both the outside and inside of the tube as to give the appearance of the action of strong acid. This action lasted for some time, but gradually diminished. It could be stopped at any time by the substitution of cold water in place of the hot, and it was renewed again after several hours by again putting the tube in hot water. The volume of hydrogen which was thus given off by the tube after it had been taken out of hot acid was about equal to the volume of the iron.

At the time I made these experiments I was not aware that there had been any previous experiments on the subject; but I subsequently found, on referring to Watt's Dictionary of Chemistry, that Cailletet had in 1868 discovered that hydrogen would pass into an iron vessel immersed iu sulphuric acid. See *Comp. rend.* lxvi, 847.

The facts thus established appear to afford a complete explanation of the effects observed by Mr. Johnson.

In the first place, with regard to the temporary character of the effect, it appears that hydrogen leaves the iron slowly even at ordinary temperatures — so much so that after two or three days' exposure I found no hydrogen given off when the tube was immersed in hot water. With regard to the effect of warming the wire — at the temperature of boiling the hydrogen passed off 120 times as fast as at the temperature of 60°. Also when the saturated iron was plunged into warm water the gas passed off as if the iron had been plunged into strong acid; so that we can easily understand how the hydrogen would pass off from the wire quickly when warm, although it would take long to do so at the ordinary temperatures. With regard to the frothing of the wire when broken and wetted—this was not due, as at first sight it appeared to be, simply to the exposure of the interior of the wire, but was due to warmth caused in the wire by the act of breaking. This was proved by the fact that the froth appeared on the sides of the wire in the immediate neighbourhood of the fracture, when these were wetted, as well as the end; and by simply bending the wire it could be made to froth at the point where it was bent.

As to the effect on the nature and strength of the iron I cannot add anything to what Mr. Johnson has already observed. The question, however, appears to be one of very considerable importance, both philosophically and in connection with the use of iron in the construction of ships and boilers. If, as is probable, the saturation of iron with hydrogen takes place whenever oxidation goes on in water, then the iron of boilers and ships may at times be changed in character and rendered brittle in the same manner as Mr. Johnson's wire, and this, whether it can be prevented or not, is at least an important point to know, and would repay a further investigation of the subject.

Dr. Ransome, M.A., demonstrated the movements of the chest in respiration, showing the remarkable mobility of its several parts, and the consequent facility with which its cavity can be inflated.

Its motions are conditioned by the shape and mode of articulation and degree of movement of the bones composing it.

The motions of points on its anterior wall on either side of the breast bone may take place in an upward, forward, and lateral direction, and a stethometer has been constructed by means of which these movements may be measured during any one act of breathing, in three planes at right angles to one another.

From the records obtained with this instrument it has been observed that the ratio of the forward to the upward and outward movements varies very greatly, not only in different individuals but in the same person, and that by the constraining influence of the will it is possible to cause at one time the forward and at another the upward motion to predominate.

The upper ribs have sometimes more forward movement than the lower; in childhood its extent is very large, but with the advance of age the movement in this plane becomes comparatively very small. In disease the ratios of the motions in the three planes are much altered—especially in the early stages of consumption and in pleurisy after the effusion has been absorbed.

By means of an instrument for ascertaining the angles made by the plane of the rib-circuits with the vertical it may be demonstrated mathematically—

1. That the upward dimensions of the movements of the anterior ends of the ribs are sufficiently accounted for by the upward rise of the ribs, their chordlengths being taken as radii, their vertebral attachments as centres.

2. The outward indications are also probably to be accounted for by the radial rise of the costal cartilages, the sternal articulations being taken as centres.

3. But from the above-mentioned observations it is obvious that the forward thrust of the anterior ends of the ribs cannot be accounted for by their simple rise from a more to a less oblique position. From the records of the chest movements it appears that there is no constant relation to be

found between the amounts of forward and upward movements, and from the measurement of the angles made by the ribs with the spine it may be shown, that these angles are not such as to permit of the degree of forward motion recorded by the three-plane stethometer.

In some instances the discrepancy between the observed and the calculated forward movement amounted to 0·5in. for the fifth rib and 0·7in. for the third rib.

From these facts the conclusion has been drawn that in forced breathing the extreme effort of expiration causes a certain degree of inbending of the ribs.

This explanation was further shown to be correct by the use of a pair of callipers, especially devised for measuring the diameters of the rib-circuits in the two positions of extreme inspiration and forced expiration. In the case exhibited to the Society the difference between the chordlength of the right third rib in expiration and inspiration was proved to be about 0·4in.

Further confirmation of these views was found in the tracings of the stethograph, shown to the Society four weeks ago.

Diagrams of these tracings, prepared by the Rev. Brooke Herford, were exhibited to the meeting.

Mr. Carson desires to correct a statement which appears in a notice read at the meeting on January 13th last, "On a Crystalline Sublimed Cupric Chloride," in which, through a misunderstanding, he stated that sodium chloride was volatilized in Deacon's chlorine process when sodium sulphate is added to the copper salt. He since has learnt from Mr. Deacon that no such volatilization of sodium chloride has been observed.

Ordinary Meeting, March 10th, 1874.

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

The CHAIRMAN said that at a meeting of the Society on the 9th day of January, 1872, in presenting to the notice of the members specimens of fossil woods from the lower coal measures of Lancashire, he stated "that from some examples in his cabinet he was led to believe that Cotta's *Medullosa elegans* was merely the rachis of a fern or a plant allied to one." Now, Professor Renault, of Paris, to whom we owe so much for his researches in fossil botany, has lately read a memoir before the French Academy on the 26th January last, which has since been printed in the *Comptes Rendus*, that completely confirms this opinion.

The genus *Medullosa* was first given by Professor Cotta to some specimens of fossil plants found at Chemnitz. M. Brongniart changed the genus into *Myeloxylon*, and M. Renault has now altered it into *Myelopteris* and made two species, *M. radiata* and *M. Landriotti.*

The fossil is found in great abundance in the calcareous nodules of the Upper Brooksbottom coal in Lancashire, and varies in size from one tenth of an inch to an inch in diameter. In this district no leaves of ferns have been found attached to it, but in the strata adjoining the seam of coal where the nodules occur specimens of *Neuropteris*, with other ferns, have been met with. Professor Renault states that M. Grand' Eury refers the petioles of *Myelopteris* to *Neuropterides*, which comprehend the *Neuropteris*, the *Odontopteris*, &c. Side by side in the same nodules the most common plant found with *Myelopteris* is *Calamodendron commune* and the small cone which from similarity or structure has been supposed by me to be the fructification of that plant.

Professor Renault has been so fortunate as to have obtained a fine collection of specimens in a most perfect state of preservation from Autun, and they could scarcely have fallen into better hands. After describing them at length he comes to the following conclusion:

"D'après ce qui précède, il est donc à peu près certain que ces pétioles de *Myelopteris* sont des pétioles de Fougères, ayant eu le mode de croissance et le port actuel de nos *Angiopteris,* dont ils différaient pourtant à certains égards, et l'on peut les considérer comme ayant formé un genre d'une grande importance à l'epoque carbonifere, mais actuellement complétement éteint, que l'on doit ranger dans la famille de Marattiees."

"Further Observations and Experiments on the Influence of Acids on Iron and Steel," by WILLIAM H. JOHNSON, B.Sc.

At the last meeting of the Society Professor Reynolds in an interesting paper "On the Effect of Acid on the Interior of Iron Wire," appears to think that I did not attribute to hydrogen any portion of the remarkable change produced in iron and steel by immersion in acid. That immersion in acid is the primary cause no one, I think, will dispute; but that hydrogen plays an important part in producing these changes and is the cause of the bubbles, the following paragraph from a paper I read before the Society, March 4th, 1873, will prove:

"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."

The supposition that the absorption of hydrogen is the sole cause of the change in the breaking strain, diminution in toughness, etc., attendant on the immersion of iron in hydrochloric or sulphuric acids, and that there is no absorption of these acids into the interior of the iron, does not account for the following phenomena :—

1.—The gain in weight of a piece of iron by immersion in hydrochloric acid is less than by immersion in sulphuric acid, as is proved by experiments described in my first paper on this subject.

2.—Iron after immersion in hydrochloric acid sooner regains its original state than after immersion in sulphuric acid.

3.—If acid iron, *i.e.*, iron which has been immersed in hydrochloric or sulphuric acids, be steeped in an alkaline solution it sooner regains its original state than with immersion in water alone.

4.—Take two pieces of iron alike in size and quality, and immerse one in hydrochloric acid and the other in sulphuric acid for some hours, then wash them well in water and dry them gently, and leave them in a temperate room for some hours more. At the end of that time it will be invariably found—that the piece which was in hydrochloric acid is covered with a dark-brown red oxide of iron, while the piece which was in sulphuric acid will be only slightly rusted.

5.—Litmus paper when applied to the moistened fracture of acid iron is slightly reddened.

All the above phenomena have been observed so often and so carefully as to leave no doubt of their invariable recurrence if the conditions of experiment be only properly observed.

It seems to me the only satisfactory way of explaining all the phenomena is to suppose that when a piece of iron is immersed in acid two actions go on, viz.: An absorption of the nascent hydrogen into the interior of the iron, which hydrogen may subsequently be given off by gentle heat or immersion in a liquid, etc. Secondly, an absorption of the acid itself, possibly in a very concentrated form by the interstices between the fibres or crystals of the metal.

That it is possible for a liquid to pass into the interior of a piece of iron is I think proved by the sweating of the cylinders of hydraulic presses, and also the known diffusion of gases through iron. The structure of iron as revealed by the microscope and the changes it undergoes during manufacture, by which a spongy mass is by hammering and rolling squeezed together, all go to prove that there are numerous cavities in iron and steel.

It will however be said, the acid must act on the walls of the cavity and form a salt of iron with liberation of hydrogen. This may go on to a small extent, but in opposition to this view we may bring the experiments of Prof. Bequerel on solutions separated by a cracked tube (*Comptes Rendus*, LXXVI), where he shows that no precipitate is formed on placing a cracked tube filled with nitrate of lead in a solution of potassium sulphate within the crack, thus making it probable that chemical interchanges do not take place in very minute spaces.

By this theory we may easily explain the decrease in toughness after immersion in acid. For toughness implies a certain ease of mobility of the particles. When a piece of iron is bent the particles of one side are compressed, thus diminishing the minute cavities between the fibres, while those of the other side are stretched, and the minute cavities elongated. Now if we fill these cavities with a liquid this mobility of the particles is prevented, for the cavities cannot now be diminished in size and the compression of the one side cannot now take place, consequently the piece tears or breaks off just like a piece of frozen rope.

It will also explain the acid reaction of the moistened fracture, and furthur, as hydrochloric acid is much more volatile and of less specific gravity than sulphuric acid, it is only natural to expect that the effect of immersion in hydro-chloric acid will pass off more rapidly than of immersion in sulphuric. This experience fully confirms.

Influence of Immersion in Acid on the Tensile Strain and Elongation.

With a view of determining these interesting points a number of experiments were made in the following way, viz.: Small coils of iron wire were immersed in hydrochloric and sulphuric acids for different lengths of time, and then carefully tested for tensile strain in a very accurate machine, so constructed that the elongation of the wire while under strain could at any moment be ascertained. The weights could also be added quickly and without imparting any shock to the wires, points to which great importance should always be attached in experiments of this kind, as a slight shock or jar on the addition of a weight will often cause the rupture of a piece which otherwise would have stood a much higher strain. The length of the pieces tested was in all the experiments 10 inches between the dies of the machine, and their temperature at the time of experiment about 16° C.

After the ultimate elongation and breaking weight had been ascertained with this machine, the coils were placed on warm plates or in hot chambers for some hours and subsequently tested in the same way.

Experiments, the results of which are shown in tables A, B, C, were made in this way, and lead us to the following conclusions:

1st. That immersion in hydrochloric acid for 1 hour diminishes the

Tensile strain of annealed iron 297lbs. per sq. inch of section.
„ unannealed iron 2,389lbs. „ „

and diminishes the

Ultimate elongation of annealed iron 0·8 per cent.

„ unannealed iron 0·83 „

2nd. That immersion in hydrochloric acid for 6 hours diminishes the

Tensile strain of annealed mild steel 2,563lbs. per sq. in. of section.

and increases the

Ultimate elongation of annealed mild steel 4·7 per cent.

When first I discovered that a decrease of ultimate elongation under strain was the result of immersion of steel in acid, I thought there must be some experimental error, and accordingly carefully selected 3 coils, all of uniform temper, and made 3 tests from each coil, the results of which are given in detail in table C. The singular regularity of these tests must be said to remove all doubt as to the truth of the results of the first experiment.

It then occurred to me that possibly *prolonged* immersion in acid might so decrease the breaking strain that the wire would not recover its original strength. For this purpose I carefully tested the elongation and breaking strain of the wire before immersion in acid and again after heating for 5 days on a hot plate. The results of these experiments as given in tables D and E confirm this view, showing that there is

1st. A permanent decrease in breaking strain after prolonged immersion in

Sulphuric acid of 12,205lbs. per sq. inch of section.

Hydrochloric „ 31,275lbs. „ „

Both these results are doubtless too high as the surface of the wire was pitted by the acid, consequently its actual sectional area was less than that calculated from the diameter as measured.

2nd. A permanent increase in the elongation after prolonged immersion in

Sulphuric acid of 1·7 per cent.

Hydrochloric „ 2·17 „

Having examined the effect of acid on *annealed* steel it was next thought advisable to try the effect on *unannealed.* This was done with results as in table F, showing that the immediate effect of immersion in acid was to

Decrease the tensile strain of unannealed steel 4,045lbs. per square inch of section.

And increase the ultimate elongation of „ „ 1·14 per cent.

The change is thus similar to that which takes place in annealed steel as shown in table C. It is, however, interesting to observe that 12 hours at a temperature of 40°—100° C. not only restores but actually increases its original breaking strain and elongation, while a still more prolonged submersion of 7 days to the same temperature still further increases them. These last experiments also show that some considerable time is required to overcome the change produced by the acid.

In conclusion I may say that the numerical results arrived at, though based on experiments conducted with considerable care, must not be taken as more than approximations to the truth, for experimental errors and variations arising from the imperfect homogeneity of structure of all iron falsify the results and are only lost by multiplying experiments almost indefinitely.

Table A.

Effect of Hydrochloric Acid on Annealed Iron Wire.

Description.	Diametr inches.	Ultim'te Elongation.	Increase in ditto.	Breaking strain.	Breaking strain per sq. inch of section.	Increase in ditto.	No. of tests of wh. the mean is given
				lbs.	lbs.	lbs.	
Annealed Iron Wire immersed in acid 1 hour...	·150″	22·4°/ₒ		918·4	51,890		9
Same piece as above after being 48 hrs. on a hot plate of a temperature of 40°–200° C......	·150″	22·3°/ₒ	—0·1°/ₒ	922·8	52,140	250	9
Annealed Iron Wire immersed in acid 1 hour...	164″	18·6°/ₒ		1105·3	52,389		3
Same piece as above being 12 hrs. on a hot plate of a temperature of 40° –200° C.	·164″	20·3°/ₒ	+1·7°/ₒ	1112·6	52,733	344	3

Average decrease in B. strain after immersion in Acid = 297 lbs. per sq. in.

„ „ ultimate elongation „ „ = 0·8°/ₒ

Table B.

Effect of Hydrochloric Acid on Unannealed Iron Wire.

Description.	Diameter, inches.	Ultimate Elongation.	Increase in ditto.	Breaking strain.	Breaking strain per sq. inch of section.	Increase in ditto.	No. of tests of wh. the mean is given
Unanneal'd iron wire immersed in acid 1 hour..	·136″	2·0°/₀		lbs. 1193	lbs. 82.150		3
Same piece as above after being 12 hours in hot chamber of tem. 40°–200°C	·136″	2·83°/₀	0·83°/₀	1226·6	84,539	2,389	3

Dec. in B. strain after immersion in acid = 2,389 lbs. per sq. in. or abt. 3°/₀
„ elongation „ „ = 0·83°/₀

Table C.

Effect of Hydrochloric Acid on Annealed Mild Steel (about ·22°/₀ Carbon).

Number of piece.	Diameter, inches.	Elongation after immersion in Hydcl. for 6 hours.	Elongation after being heated 12 hrs. to a tem. of 100 C.	B. strain after immersion in Hycl. for 6 hrs.	B. strain after being heated 12 hours to a tem. of 100 C.
1	·135″	19°/₀	16°/₀	828 lbs.	840 lbs.
	·135″	19 „	17 „	824 „	836 „
	·135″	20 „	16 „	836 „	860 „
2	·135″	20 „	14 „	730 „	780 „
	·135″	20 „	13 „	730 „	754 „
	·135″	20 „	16 „	770 „	820 „
3	·135″	21 „	16 „	728 „	770 „
	·135″	20 „	17 „	742 „	800 „
	·135″	22 „	14 „	736 „	794 „
Average.	·135″	20·1 °/₀	15·4 °/₀	769·3 lbs. 53800 lbs. pr. sq. in.	806 lbs. 56363 lbs. pr. sq. in.

Decrease in B. strain after immersion = 2,563 lbs. or 4·56°/₀
Increase in elongation „ = 4·7°/₀

Table D.

Comparative Effect of Sulphuric and Hydrochloric Acids on Unannealed Charcoal Iron Wire.

Description.	Diameter inch.	Ultimate Elongation.	Decrease in ditto.	Breaking Strain.	Breaking Strain per sq. inch.	Decrease in ditto.	No. of tests of wh. result is a mean
				lbs.	lbs.	lbs.	
Charcoal Iron before immersion in acid...	·049″	2·3%	......	147·3	78,200	......	3
Ditto after 12 hours in hydrochloric acid and subsequent 5 hours in air at a temperature of 12°C.	·047″	1·17%	1·13%	122·7	71,930	6,270	3
Last piece after being on hot plate for 5 days at a temperature of 40—140°C...	·047″	3·5%	—1·2%	81·3	47,954	30,246	3
Charcoal Iron after 12 hours in sulphuric acid and subsequent 5 hours in air at a temperature of 12°C.	·045″	1·3%	1·0%	98·	61,800	16,400	3
Last piece after being on a hot plate for 5 days at a temperature of 40—140°C...	·045″	4·0%	—1·7%	104·6	65,995	12,205	3

All the above 15 tests were made from one coil.

Permanent decrease in B. strain after prolonged immersion in hydrochloric acid=30,246lbs. per sq. in.
" " " sulphuric " =12,205 " " "
" increase in ultimate elongation after prolonged immersion in hydrochloric acid=1·2 per cent.
" " " sulphuric " =1·7 "

Table E.

Effect of Hydrochloric Acid on Unannealed Charcoal Iron Wire.

Description.	Diameter. inch.	Ultimate Elongation.	Decrease in ditto.	Breaking Strain.	Breaking Strain per sq. inch.	Decrease in ditto.	No. of tests of wh. result is a mean
				lbs.	lbs.	lbs.	
Charcoal iron before immersion in acid...	·046″	1%	......	134	80,240	......	6
Ditto after immersion in acid for 5 hours...	·045″	1·83%	......	110·6	68,616	......	6
Last piece after 7 days on hot plate...	·045″	3·00%	—2·0%	94·6	59,700	30,540	6
Charcoal iron after immersion in hydrochloric acid for 5½ hrs., then washed and dried in air 6 hours and again immersed 5hrs. in acid	·045″	1%	......	86·	53,320	......	6
Same piece as last heated for 7 days on hot plate	·045″	5·3%	—4·3%	90·6	56,172	34,068	6

All the above 30 tests were made from one coil.

Average permanent decrease in B. strain after prolonged immersion=32,304lbs. per sq. in.

„ „ increase in elongation „ =3·15 per cent.

Table F.

Effect of Hydrochloric Acid on Unannealed Mild Steel.

Description.	Diameter. inch.	Ultimate Elongation.	Decrease in ditto.	Breaking Strain.	Breaking Strain per sq. inch.	Decrease in ditto.	No. of tests of wh. result is a mean
				lbs.	lbs.	lbs.	
Mild steel before immersion in acid......	·0905″	1·66%	......	656·3	102024	......	6
Ditto after immersion in dilute acid 5 hrs..	·0893″	2·8%	—1·14	613·3	97979	+4045	6
Last piece heated in hot room 12 hours to a temperature of 40°—120°C.	·0893″	2·16%	—·5%	671·6	107295	—5271	6
Last piece heated 7 days in same hot chamber	·0893″	3·42%	—1·76%	701·	111992	+9968	6

Decrease of Breaking strain on immersion=4,045lbs. per sq. in.

Increase of Elongation „ „ =1·14 per cent.

Each result is the mean of 6 tests on different coils.

Extension of the Influence of Acid beyond the part immersed in the liquid.

Hitherto all the experiments have been made on iron totally immersed in acid solutions. It appeared to me, however, that very possibly the changes produced in iron and steel by immersion in acid might not be confined to the part in contact with the liquid only, but might also spread beyond and produce effects similar in quality but less in degree. With this view the following experiments were made.

Pieces of carefully selected charcoal iron, mild steel (about ·22 carbon), and hardened and tempered steel wire (about ·65 carbon), each about 35 centimetres long and of same thickness, were partly immersed in 4 tubes 13·5 centimetres deep, filled respectively with dilute sulphuric, hydrochloric, and nitric acids, and a saturated solution of common salt in water. After 72 hours the pieces of wire were pulled out and examined, when considerable differences became apparent.

1st. The pieces immersed in salt solution were freed from a very slight coating of rust on them before immersion, where they were in contact with the liquid, but above the surface of the liquid the rust remained as before. No decided alteration in toughness was apparent, and no bubbles were given off from the moistened fracture of the wire either of the part in the liquid or that out.

2nd. The pieces in nitric acid were slightly eaten away on the surface, the hardened steel least and the mild steel most, and the surface was covered with a fine black dust in the case of the charcoal and hardened steel. The moistened fracture did not bubble, however, in any of the pieces either in the part in or out of the acid, and no alteration in toughness was apparent from that before immersion, except in that part of the hardened steel covered with acid, where a possible though undecided diminution seemed to exist.

3rd. The surface action of hydrochloric acid was rather more marked than with nitric acid, and seemed to develope fibrous markings, while the nitric acid only fretted thesurface.

The hardened steel was very little eaten away indeed, while the mild steel was half eaten away, the action being irregular and much more marked in some places than in others. The action on the charcoal was intermediate between the mild and hardened steel.

On moistening the fracture of the charcoal iron bubbles were given off in great abundance from the whole surface of the fracture. On trying the same experiment with the part adjacent, but not actually immersed in the acid, a few small bubbles were seen to rise from the surface of the fracture, even at a distance of 17c.m. from the surface of the liquid. These bubbles were found to increase in number and the toughness to decrease as you approached the surface of the acid. Moreover, when that part of the wire not immersed in the acid was broken, the bubbles arose almost exclusively from the centre of the fracture; while from the part immersed in the acid they arose from the whole surface, and took less time to attain their maximum.

No bubbles were visible to the unaided eye on the moistened fracture of the mild steel or on the hardened steel. That part of the mild steel immersed in the acid broke when bent like a pipe stem, 1 centimetre above the surface of the acid it was a little tougher, and at 2 centimetres distance it was as tough as before immersion, and would stand bending to and fro many times before breaking.

The hardened steel immersed in acid broke very short, but a little tougher than the mild steel; perhaps because it was not so much eaten away. At 1 centimetre above the surface of the acid it broke less short than in the acid, and the toughness seemed to increase with the distance from the acid until at 4 centimetres it broke as tough as before immersion.

4th. The effect of sulphuric acid was almost the same as that of hydrochloric, except that its surface action, particularly in the case of mild steel, was not so marked.

It thus appears that the fibrous nature of the charcoal iron allows the acid and absorbed hydrogen to pass up through the interior of the mass for some 17 c.m. above the surface of the acid, thus decreasing its toughness. The closeness of the grain of the steel and absence of fibre appear on the other hand to prevent this action. This difference in the behaviour of steel and iron is the more remarkable as the decrease of toughness consequent on immersion in acid is much greater with steel than with iron.

Further, it is well worthy of notice that nitric acid, which does not evolve hydrogen on contact with iron, does not appear to perceptibly diminish the toughness of iron or steel immersed in it, nor do bubbles arise from the moistened fracture of these metals after immersion in it, as they do with acids which give off hydrogen by their action on iron.

In view of the constantly increasing consumption of mild steel, and the proposal to introduce it in place of iron for ship building, the greater corrosive action of acids on mild steel than on iron has considerable interest. For it would seem to indicate that mild steel will more rapidly corrode than iron, and possibly this may render its use in shipbuilding to be attended with greater risk.

Mr. R. D. Darbishire, F.G.S., gave an interesting account of Dr. Schliemann's Excavations and Discoveries on the Site of Troy, and exhibited some selected Photographs from his collections.

"Results of certain Magnetic Observations made at Manchester during the year 1873," by Professor Balfour Stewart, LL.D., F.R.S.

These observations were made in a small wooden house, the property of The Owens College, erected in the garden of Professor Stewart, no iron whatever being used in its erection.

The latitude of the house is 53° 26′ N., and its longitude 2° 13′ W. The observations were chiefly made by Mr. F. Kingdon, assistant at the Physical Laboratory of Owens College, in the presence of students who were receiving instruction.

These conditions are not so favourable to minute accuracy as where the observations made are not combined with instruction to pupils: nevertheless it is believed that the following table will exhibit fairly accurate values of the magnetic dip and horizontal force for the year 1873.

The instruments used had been previously verified at the Kew Observatory. They consist of a Dip circle by Dover and a Unifilar by Elliott Bros. The dips are generally the mean results from two needles.

1873.	Dip.	Horizontal Magnetic intensity.
January	69° 19′·5	
February	69° 14′·6	
March	69° 17′·6	3·693
April	69° 18′·9	
May	69° 17′·7	3·686
June	69° 16′·1	3·684
July	69° 14′·6	3·704
August	69° 20′·2	3·704
September	69° 23′·8	3·715
October	69° 16′·8	3·661
November	69° 14′·4	
December	69° 18′·3	3·686

PHYSICAL AND MATHEMATICAL SECTION.

March 3rd, 1874.

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 1873," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The rainfall of 1873 was much below the average fall, and very different in result from that of the previous year. Whilst the rainfall of 1872 was about 36·7 per cent above the average fall, the fall of the past year was about 12 per cent below the average. It is remarkable how little this deficiency of rainfall was noticed by persons who had no means of ascertaining the fact. Frequently I was asked during the year if the rainfall was not greater on the whole year than usual. This doubtless arose from the fact that the summer months, the only enjoyable season of the year, had a considerable excess of rainfall. The excessive fall in August had a bad effect upon the potato crop in the neighbourhood of Eccles. The least amount of rain fell in the spring and autumn months, though the greatest deficiency was in December. The number of days on which rain fell during the year was in excess of the average; but this excess was between April and September; the number of days of rainfall in the remaining months was below the average.

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 13 years	Quarterly Periods: 1873.	1873.	Fall in inches	Average of 13 years.	Difference,	Quarterly Periods: Average of 13 years.	Quarterly Periods: 1873.
Days	Days						
51	45	January......	3·808	2·779	+1·029	7·481	7·047
		February ...	0·551	2·250	—1·699		
		March	2·688	2·452	+0·236		
46	52	April.........	0·699	2·078	—1·379	6·911	5·677
		May	2·052	2·085	—0·033		
		June	2·926	2·748	+0·178		
52	67	July	4·324	3·122	+1·202	10·296	10·783
		August	4·148	3·089	+1·059		
		September...	2·311	4·085	—1·774		
57	55	October......	4·587	4·271	+0·316	10·387	7·620
		November...	2·265	3·128	—0·863		
		December...	0·768	2·988	—2·220		
206	219		31·127	35·075	—3·948		

In the next table is given 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 10in. round receiver and the other a 5in. square receiver. The large receiver had an excess over the small one in most of the months of the year, the exceptions being January, July, August, and October. The total difference of the fall in the two gauges was very small, being even less than the difference in 1872. The total difference in 1872 was over four tenths of an inch, but in 1873 it was barely over three tenths of an inch. And an average fall in both gauges over a period of six years shows a difference of only about the same amount. Thus the two gauges, though of different characters as regards their receivers, are good checks upon each other.

1873.	Rainfall in inches in 10in. round receiver, 3ft. from ground.	Rainfall in inches in 5in. square receiver, 3ft. from ground.	Differences	From 1868 to 1873. Average of 6 years' rainfall in inches in 10in. round receiver, 3ft. from ground.	From 1868 to 1873. Average of 6 years' rainfall in inches in 5in. square receiver, 3ft. from ground.	Differences
January	3·808	3·823	— ·015	2·987	2·976	+ ·011
February	0·551	0·543	+ ·008	2·250	2·209	+ ·041
March	2·688	2·660	+ ·028	2·380	2·347	+ ·033
April	0·699	0·641	+ ·058	2·192	2·162	+ ·030
May	2·052	2·000	+ ·052	1·906	1·871	+ ·035
June	2·926	2·880	+ ·046	2·600	2·557	+ ·043
July	4·324	4·359	— ·035	2·902	2·890	+ ·012
August	4·148	4·176	— ·028	2·857	2·797	+ ·060
September ...	2·311	2·188	+ ·133	3·931	3·868	+ ·063
October.........	4·587	4·630	— ·043	5·124	5·097	+ ·027
November ...	2·265	2·207	+ ·058	2·828	2·851	— ·023
December......	0·768	0·713	+ ·055	3·308	3·291	+ ·017
	31·127	30·820	+ ·307	35·265	34·916	+ ·349

In the next table I give the results obtained from two exactly similar gauges placed at different heights from the ground and free from every interference. Each gauge has a 5in. square receiver, and the one is placed 3 feet and the other 34 feet above the ground. The total fall in the one 3 feet from the ground was 30·820 inches, and in the one 34 feet from the ground it was 25·401 inches, for the year 1873. The difference between the fall in the two gauges is 5·419 inches, or about 17 per cent less rain fell in the higher than in the lower gauge. In the following table I give the average fall in the same gauges for 6 years, and by comparing the results it will be found that the rate per cent of the difference of the rainfall in the two gauges is the same as it is for last year.

1873.	Rainfall in inches in 5in. square receiver, 3ft. from the ground. 1873.	Rainfall in inches in 5in. square receiver, 34ft. from the ground. 1873.	From 1868 to 1873. Average fall of rain in inches for 6 yrs. in 5in. square receiver, 3ft. from the ground.	From 1868 to 1873. Average fall of rain in inches for 6 yrs. in 5in. square receiver, 34ft. from the ground.
January	3·823	2·799	2·976	2·130
February	0·543	0·349	2·209	1·656
March	2·660	2·142	2·347	1·846
April	0·641	0·570	2·162	1·858
May	2·000	1·750	1·871	1·583
June	2·880	2·633	2·557	2·288
July	4·359	3·793	2·890	2·566
August	4·176	3·378	2·797	2·378
September ...	2·188	1·894	3·868	3·322
October.........	4·630	3·804	5·097	4·228
November ...	2·207	1·764	2·851	2·177
December......	0·713	0·525	3·291	2·760
	30·820	25·401	34·916	28·792

I thought it might be interesting if the ratios were given of the excesses of rainfall measured at 3 feet from the ground over the amount measured at 34 feet above the ground. These ratios appear in the following table. It is curious to observe that the ratios of the total amounts that fell in these gauges, both for 1873 and for an average of 6 years, are identical, the ratio of the difference between the fall in the upper gauge and that in the lower being 0·824. And even in the heavy rainfall of 1872 it will be found that the ratio of the deficiency between the fall in the two gauges was nearly similar, being 0·855. But when the monthly ratios are examined, either for the year 1873 or for the average rainfall in the gauges for 6 years, it will be seen that the greatest difference of fall happens in January and the least in June. The oscillations, too, of the differences between the fall of 1873 and the average fall of 6 years is very marked. On the theory that the excess of rainfall in the lower gauge is due to the particles of invisible vapour in the air between it and the higher gauge coalescing with the falling rain drops, the table seems to show that in the summer months, and particularly in June, there is relatively less of this vapour in the air below a height of 34 feet, and that there is relatively more of it in the winter months, and particularly in January. Hence the maximum of dry air on the ground is in June, and the minimum in January. This can only be due to a very rapid convection of warm air with its attendant vapour from the surface of the ground upward. A consideration of this law in its action upon public health cannot be too much regarded.

MONTHLY AND ANNUAL RATIOS OF THE EXCESS OF RAINFALL MEASURED AT 3 FEET FROM THE GROUND OVER THE AMOUNT MEASURED AT 34 FEET FROM THE GROUND.

	Ratios of such rainfall for 1873.	Ratios of such rainfall for an average of 6 years from 1868 to 1873.
January	·732	·715
February	·642	·749
March	·805	·786
April	·889	·859
May	·875	·846
June	·914	·894
July	·870	·887
August	·808	·850
September	·865	·858
October	·821	·829
November	·799	·763
December	·736	·838
Annual ratios	·813	·822

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, as usual, the fall during the night, and this happened in every month of the year excepting January, February, and December. The total excess of the day over the night fall for last year was 2·846 inches. In 1871 the excess was 4·136 inches, and in 1872 it was 1·891 inches.

1873.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Differences between Night and Day fall.
January ...	1·113	2·710	+1·597
February ...	0·249	0·294	+0·045
March	1·579	1·081	—0·498
April.........	0·502	0·139	—0·363
May	1·165	0·835	—0·330
June	2·194	0·686	—1·508
July	2·565	1·794	—0·771
August	2·424	1·752	—0·672
September..	1·218	0·970	—0·248
October......	2·423	2·207	—0·216
November...	1·153	1·054	—0·099
December...	0·248	0·465	+0·217
	16·833	13·987	—2·846

In the next table I present the average day and night rainfall for 6 years. An examination of this table continues to confirm the experience of previous years, that the day fall exceeds the night fall so far as the amount of the whole year is concerned. But curiously enough this table of 6 years' average gives an excess of night fall in the same months that were shown in a table of 5 years' average which I read at the Section last year, those months being January, February, August, September, November, and December. It is however remarkable how close the total day and night rainfalls are to each other, for though the 6 years' average shows a difference of double that of the 5 years', yet it is only about 4 per cent on the total fall.

Average of Six Years from 1868 to 1873.

1873.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Differences between Night and Day fall.
January	1·321	1·655	+0·334
February	0·919	1·288	+0·369
March	1·375	0·970	—0·405
April	1·279	0·883	—0·396
May	1·206	0·666	—0·540
June	1·447	1·110	—0·337
July	1·713	1·177	—0·536
August	1·350	1·447	+0·097
September.....	1·773	2·094	∓0·321
October	2·634	2·463	—0·171
November......	1·367	1·484	+0·117
December	1·448	1·843	+0·395
	17·832	17·080	—0·752

Ordinary Meeting, March 24th, 1874.

Rev. WILLIAM GASKELL, M.A., Vice-President, in the Chair.

"On some of the Perplexities which the Art and Architecture of the Present are preparing for the Historians and Antiquarians of the Future," by the Rev. BROOKE HERFORD.

One of the most interesting elements in historical and antiquarian studies is the consideration of the age of the various remains which have come down to us from the past. It continually occurs that points of great importance depend upon our being able to assign an approximate date to a document, an inscription, or some architectural feature of a building. It is thus that vague tradition is often eked out by corroborative fact, and questionable chronicles become susceptible of verification. Even in cases in which it is out of the question to assign actual dates, it is something to be able to be sure of the general period to which an object belongs; and even the *iron*, *bronze*, and *stone* periods afford distant landmarks by which we may grope our way back into an antiquity which may not always be so dim as it is at present.

Now all who have gone into the study of the dates or periods of ancient remains or monuments, must have been struck by one feature which is almost universally characteristic of them. I refer to the marvellous reliability of whatever indications they carry in themselves of style or stage of development. Once get back beyond a certain borderland of confusion and deception, and almost everything speaks for itself. You can tell—approximately, of course—*when* it was done, by the *how* it is done. The fact is that as the arts of life gradually developed men did everything simply according to the best idea they had attained. They worked by the clearest light of their own time. There was a sort of sturdy self-respect in their workmanship. They did not think that their own best forms were prosy and commonplace, and when they wanted something unusually beautiful run after the ruder forms of an earlier age. They had the thought of the beautiful, but they seem to have been happily innocent of that sentimental distortion of the sense of beauty which leads to the artificial manufacture of the picturesque. So you tread confidently and firmly as you trace back the ways of the ancient life. You may be walking among rude and imperfect things, but at any rate you are not walking among shams. The student of old manuscripts will tell you the age of a Greek codex by the uncial or cursive character in which it is written. If you find a tomb inscribed in the old, so-called "Lombardic" characters, you feel pretty sure that it is not later than the 13th or beginning of the 14th century, nor can it be much earlier, for previously the tombstones, coffin lids, had crosses, but not inscriptions on them. When you come across a rude circle of stones upon the moors above Ilkley, you may doubt if it was a hut circle, or a Druid Temple; but at any rate you are not haunted by a misgiving that it was only a freak of some early monk of Bolton Priory, with a taste for the picturesque. If you are pointed to an apparent date of 1174, like that which stands on the gable of an old house near Sowerby Bridge, you know, *at any rate*, that it cannot really be of 1174, because the Roman numerals were still universally in use, and the so-called Arabic figures hardly even known in England, till two centuries later. If you meet with an early undated specimen of printing and find some words in it in italics, you know that it cannot be earlier than 1501 when the great Venetian printer Aldus Manuzio first introduced the new slanting letters which he had had cut, in the series of works which have given the Aldine classics an imperishable name. Or, again, if you meet with an old parchment engrossed in the curious black-letter which is known as balloon writing, you can form a pretty fair conjecture that it dates back to the time of the Edwards.

But it is in Architecture that we come upon the most striking and important illustrations of the way in which each age, going a little beyond the age before, believed in its own improvements, and did simply the noblest thing it knew. I hesitate to speak of an art of which I know so little technically, but even the knowledge that an ordinary lover of old buildings attains suffices to illustrate my point, and also to show its value. You can trace our architecture, from the great Norman builders, who first taught the English how really to *build*, down to the exaggeration of the 16th and the debasement of the 17th centuries, by unbroken steps,—Norman, Transitional, Lancet, Geometrical, Curvilinear, Perpendicular,—every one of which opens up new studies of interest.

And even later still, though the architecture of Elizabeth's time is only reckoned of the poorest (and in ecclesiastical architecture there is nothing, for the church was too much weakened by the Reformation to do much building), yet in domestic architecture we have a type of house with many and sometimes fantastic gables, and massive mullioned and transomed windows, which at any rate *stands for itself*, and is an interesting study in many of our halls and manor houses.

Of course, all these cannot be divided by sharp and unerring lines of date. They cannot be divided, precisely because they are the outcome of true growth, and growth implies continuous and indivisible processes. But the order of growth is wonderfully observed. Sometimes even its minuter steps can be apportioned to their special dates with a curious exactness. I remember hearing Mr. Edmund

Sharpe, an architect and antiquarian who has studied the old Cistercian abbeys as no other living man has done, pointing out at Furness Abbey one special moulding—the plainest possible inverted volute—as really dating the capitals on which it appears to within about 20 years. It is seldom, of course, that any such exactness can be attained. The rate of progress was not equal everywhere, and in remote country places the old forms lingered after the great monastic builders had left them behind.

As to the practical value of all this in the study of history, one or two illustrations will suffice. Often it happens that much of the history of a church or an abbey is contained in the silent testimony of the successive stages of its architecture, or in the remains of older work embedded in more recent masonry.

For instance: you visit some little country church. Its general character is that of Henry VII.'s time—you know it by the Perpendicular windows and the spandrelled doorways. But look a little closer. In the chancel perhaps are a couple of simple Lancet windows, and above them are two or three quaint corbels, which never originally supported that late battlemented parapet; and underneath the comparatively recent porch is an old semicircular arch, and if you look carefully about the walls you will probably find here and there, built in, fragments of carved stone—perhaps pieces of old stone-coffin slabs with crosses visible upon them—which tell of a much earlier church; and sometimes you find fragments of old interlacing scroll work, such as that upon the Runic crosses of Ilkley, which carry back the interest still further, and tell of Saxon worshippers.

And now if you have gone with me in this appreciation of the self-respect which in old times men shewed for the best art of their own day, and of the value of the trustworthy indications of date, arising out of this orderly development, which everywhere present themselves, it will hardly need that I should enter very much into detail as to the perplexities which the art and architecture of the present are preparing for the students of future times. For every step in which I have been shewing these trustworthy characteristics of the past must have reminded you how different things are to-day. I tremble sometimes to think of the curses which may some day be heaped upon this self-complacent nineteenth century, with its great affectation of taste and art, by those who in some remote future may have the task of disinterring, and endeavouring to interpret our monuments! They will find inscriptions in every variety of character, Lombardic, old English, the antique Roman type of the end of the 17th century, and—but very sparingly—in the beautiful characters which our modern type founders have elaborated,—such type as never was in the world before, but of which modern Englishmen seem ashamed. They will find drinking fountains of Queen Victoria's reign inscribed in characters which would betoken an origin under the Plantagenets; and ridged tombstones of decent Manchester merchants hardly distinguishable from those of the old, spurred and belted border-knights who compounded for their sins by leaving estates to the monks on condition of burial within the cloisters. They will find books printed in carefully imitated types of the sixteenth century referring to matters which ordinary history would have led them to believe happened in the nineteenth.

But it is in matters of architecture that they will experience the most bewildering perplexity. Imagine, if it is possible, Manchester disinterred from the superincumbent mould of three thousand years by some enterprizing relative of Mr. Macaulay's celebrated New Zealander, who has read of "the Manchester School," and desires, like another Dr. Schlieman anent Troy, to prove to his incredulous countrymen, that Manchester was a real place, and its school a veritable seat of learning! Imagine the curious wonder with which the Cyclopean architecture of the shops underneath the Exchange would be compared with the fragments of the friezes and mouldings which might be found among the ruins. Imagine the perplexity with which the portico of the Infirmary would be compared with the facades of some of the Portland street warehouses, and all attempted by some systematic thinker to be co-ordinated with the Assize Courts and the New Town Hall. As for the endeavour to ascertain anything about our domestic architecture, that would be hopeless indeed. But there is one consoling thought. It is possible that with regard to this at any rate the perplexity may never occur; for it is few indeed of our modern houses that will stand into a second or third century!

Perhaps the most important feature in this matter is that what we are doing by this artistic and architectural confusion will not stop with the perplexity which it will cause concerning the remains of our own time. That might be suffered. Some may say, our buildings, at least our considerable public buildings, are all *dated*—so are our books, and our sculptured inscriptions. True. But the difference between those of our day *which are dated*, and those of six centuries ago, which are *not* dated, will grow less distinguishable every century, until at last they will all be touched with one equal aspect of antiquity, and the utter confusion which will then appear among those which *are* dated, can hardly fail to bring equal confusion, and at last discredit upon those which *are not*.

I do not think that this is a light matter, though I have pointed out in passing a certain ludicrous aspect which it undoubtedly possesses. I think it points to deeper defects in the mental and moral life of the age: to a craving for the excitement of the picturesque rather than an appreciation of the really beautiful; to a want of originality in the higher forms of art, that is poorly compensated by the skill with which we can lay Etruscan and Pompeian vases or mediæval cathedrals under contribution; and to a miserable depreciation of our own contemporary forms, even when, as in the case of our printed characters, they are really of surpassing clearness and excellence.

"A Few Observations on Coal," by E. W. BINNEY, V.P., F.R.S., F.G.S.

Of late years much has been written on the structure of coal and the various vegetable remains of which it is composed. Observers examining different coals under the microscope have, as might be expected, come to different conclusions. Some of them have found little else than the remains of spores and spore cases, others only scalariform and cellular tissues and a few spores, and a third class little trace of any structure whatever in the specimens they examined. Splint or hard coal would generally afford the results first named, soft caking or cherry coals the second, and cannel coals the third.

Soft coals yielding a large amount of charcoal enclosed in bright coal nearly always show plenty of structure in the "mother coal," as well as a few macrospores in the bright portions. Macrospores are nearly always found in abundance in splint and hard coals. In cannel coals they are sometimes found as well as the cellular and scalariform portions of plants.

For many years macrospores were known by the names of spore cases and spores. They could be easily observed by the naked eye in the black parts of the coal, and they were generally considered as sporangia, but Professor Adolphe Brongniart in 1868 described a cone (*Lepidostrobus Dabadianus*) having sporangia full of very minute spores, in fact microspores, in the upper portion, and sporangia full of macrospores, which had been so long known, in the lower part. These observations of Brongniart have been amply confirmed by specimens from the British coal fields.

Thirty years ago it was considered that the soft or cherry coals were chiefly composed of the remains of large plants such as *Sigillaria, Lepidodendron,* &c., while caking coals were formed of plants of a lesser size and much bark, for it had then been observed and since confirmed, that the outsides of *Sigillaria* and other fossil trees, sometimes reaching to two or three inches in thickness, were chiefly composed of bright soft coal showing little traces of structure.

In the great lawsuit which was tried at Edinburgh more than twenty years since as to the nature of Boghead coal, much evidence was given as to its structure, some witnesses finding in it scarcely anything else than the remains of vegetables, whilst others found only a stray portion of scalariform structure or a macrospore. Both Boghead and the other brown cannels of Scotland are now generally considered to afford but little evidence of vegetable tissues under the microscope, although numerous remains of *Sigillaria* and other common coal plants can be seen by the naked eye in them. Notwitstanding that they are so rich in volatile matter, and far exceeding other coals in their yield of paraffine and paraffine oils, they contain from 25 to 30 per cent of mineral matter in the form of ash. This circumstance has induced certain scientific men to class them as shales rather than coals, notwithstanding that they have the specific gravity of coals.

Some years since, when describing several fossil cones affording both kinds of spores, he expressed an opinion that the yellow matter seen in the vesicles of Boghead coal was nothing but microspores composed of paraffine or a similar hydrocarbon, and that they were driven off by heat in the form of a yellow vapour, leaving nothing behind but a spongy mass of earthy and carbonaceous matter. The evidence that caused him to come to this conclusion was that the microspores contained in the upper sporangia of *Lepidostrobus Harcourtii* had all the appearance of crude paraffine, and were of the same yellowish brown colour as powdered Boghead coal, and that the latter substance was composed nearly altogether of microspores and mineral matter. So far as his observations extended, microspores had not been observed in coal, although plenty of macrospores, which are generally $\frac{1}{20}$th to $\frac{1}{25}$th of an inch in diameter, and easily seen by the naked eye, had long been noticed.

He had some time since directed his friend Mr. J. W. Kirkby, of the Pirnie Coal Company, to examine the Fifeshire seams of coal in search of microspores, most of those beds yielding macrospores in great abundance, and that gentleman had lately furnished him with the specimens now exhibited, both splint and soft coals, but especially the former, affording the two kinds of spores. On burning the yellow coal composed of microspores, a most brilliant flame and a peculiar empyreumatic odour like that from burning Boghead coal were produced, whilst the splint coal full of macrospores only burnt and smelt like ordinary hard coal, thus clearly showing that these two kinds of spores differed very much in their inflammable properties and odours given off, and that such properties were certainly not due to the larger spores but most probably to the smaller ones.

The compressed lenticular bodies in the splint coal were formerly of oval and spherical forms with a triradiate ridge on one half, and although their exterior was composed of a brown coriaceous substance, their interior was full of white carbonate of lime or bisulphide of iron according to the nature of the matrix in which they were found; thus suggesting the idea of their having been filled with granules of starch when in a living state, which it is probable they would have been in case of their being germinating spores.

He and his late partners at Bathgate, when manufacturing paraffine oil there, had tried various means, by subjecting Boghead coal to the action of ethers and naphthas, to dissolve out paraffine from it, but they had never succeeded, so the yellow matter in the coal may probably be changed into paraffine by the heat employed in distillation. Dr. Schorleimer, F.R.S., who has been so kind as to examine the microspores found in the Muiredge splint coal, is of opinion that they are not composed of paraffine, but some other hydrocarbon. This may, therefore, change into paraffine by the application of heat in a similar way to what the yellow matter in Boghead coal does.

The macrospores are about 320 times the size of the microspores and constituted the germinating spores, while the microspores were the fertilizing agents, both having been contained in one cone.

Several specimens of fine bright soft coal, between 2 and 3 inches in thickness, taken from the outsides of *Sigillaria* and other fossil trees, were exhibited. In these there was no appearance of charcoal, spores, or vegetable structure, and in every respect they resembled the black shining parts of soft cherry and caking coals, which generally afford no distinct traces of vegetable structure. Hence from his observations he was led to conclude that soft or cherry coal was chiefly composed of the bark, cellular tissue, and vascular cylinders of coal plants with some macrospores and microspores.

That caking coal had much the same composition, except that it contained a greater proportion of bark in it.

That splint coal had a nearly similar composition, but with a great excess of macrospores.

That cannel coal, especially that yielding a brown streak, was formed of the remains of different portions of plants with a great excess of microspores, which had long been macerated in water.

These conclusions were arrived at merely as to the composition of the different kinds of coal. No doubt each seam would be materially affected by the nature of the roof, whether the latter was an open sandstone or a close and airtight black shale or blue bind, for the former would allow the free escape of gaseous matter, and the latter would prevent its escape. It is well known that the character of the roof has a deal to do with the quality of the coal under it.

Ordinary Meeting, April 7th, 1874.

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

Mr. J. Sidebotham and Mr. J. A. Bennion were appointed Auditors of the Treasurer's Accounts.

The CHAIRMAN exhibited to the meeting some portion of the cast iron roof from the Salford Station of the Lancashire and Yorkshire Railway, which after having been up for a period of four years was so much corroded and damaged that it had to be taken down. He attributed the effects to sulphuric acid and soot arising from the combustion of the coal used in the locomotives passing under it, aided by the action of steam and vibration. He referred to a paper by himself communicated to the Society and published Vol. II. (2nd series) of its Memoirs, on the effects of old coal pit water on cast iron, where similar results had been produced by sulphuric acid, carbonaceous matter, and water; also to a case alluded to by one of the most distinguished members of the Society, the late Dr. W. Henry, F.R.S., of the rotting of cast iron by the escape of steam from the junction of a pipe embedded in charcoal. Of course the rate of decomposition much depended on the quality of the iron; but as that metal was now so much employed in building and mining operations, he considered it desirable to bring before the public every instance that came to his knowledge where it had been damaged or decomposed.

"On the Action of nascent Hydrogen on Iron," by WILLIAM H. JOHNSON, B.Sc.

In a paper read before the Society last year, I showed that a piece of iron immersed in hydrochloric, sulphuric, or other acid which evolves hydrogen by its action on the metal, on breaking gives off bubbles of gas from the surface of the fracture. It subsequently occurred to me that these bubbles might be produced by subjecting the metal to the action of nascent hydrogen for some time, and without the aid of acid at all.

To test this I connected two pieces of iron wire ·07″ diam. respectively with the copper and zinc plates of a battery of 50 Daniell's cells and immersed them in a vessel of Manchester town's water at a distance of one inch apart. On closing the current, bubbles of hydrogen were given off from the wire connected with the zinc, but none from the wire connected with the copper, the oxygen liberated at the pole apparently forming oxide of iron which in 12 hours formed a thick smudge at the bottom of the vessel. After 24 hours the surface of the wire connected with the zinc was unchanged, but on moistening the fracture bubbles were given off abundantly just as if it had been immersed in acid. The other wire, on the contrary, though much oxidised and eaten away, did not give off bubbles when broken.

A variety of experiments were made in the same way with pieces of wire varying from 3 to 20 inches long and immersed from 5 to 24 hours, ½ inch to 4 inches apart in pure Manchester town's water, all with similar results· With this exception, that when the wire connected with the zinc was of steel, no bubbles were visible to the naked eye, just like steel after immersion in acid. Twenty-four hours in a warm room restored the iron to its original state, and no bubbles were seen on fracturing it.

The water in the last experiments was then replaced by an aqueous solution of caustic soda, when after two hours the moistened fracture of the wire connected with the zinc pole of the battery was found to bubble. Twenty two hours' longer immersion, the battery working all the time, caused the bubbles to be more abundant; the toughness of the wire was also diminished, and its surface was blackened. The wire at the positive pole was however unchanged either on the surface or in toughness.

Three pieces of wire, each 12 inches long, were immersed in hydrochloric acid about 1·2 sp. gr., one being connected with the positive pole, the other with the negative pole of the battery, and the third unconnected with the battery. At the expiration of half an hour the two last pieces were found to bubble on being fractured, and were also more brittle. The one however connected with the positive pole was not in the least affected. Thus showing that simple immersion in acid is not sufficient to produce in iron the remarkable changes before described, unless it be accompanied by evolution of hydrogen.

In conclusion, if the oxidation of the surface of iron be as a rule accompanied by the absorption of nascent hydrogen into the interior of the iron, then the diminution of strength and toughness consequent on this will affect iron ships, telegraph cables, and other structures in which iron is largely used and which are constantly immersed in water.

"Does the Earth receive any Heat directly from the Sun," by HENRY H. HOWORTH, Esq.

The term heat is one of unusual ambiguity. It has at least two meanings, which, if they do not exclude one another, are at least not commensurable. Their indefinite and careless use has created great confusion. The first meaning connotes the feeling heat, a purely subjective matter, whose investigation is a proper subject for metaphysical students, but with which we have nothing to do at present. The second meaning connotes objective heat, the force heat, that phenomenon of matter whose effects we can measure in certain definite ways. It is with this second meaning that we are concerned to-night. Having limited our subject somewhat, I wish to limit it further. The science of heat concerns itself with two main subjects, first, the transcendental problems which deal with the nature of heat, which endeavour to explain it as a form of motion, &c., &c. These conclusions may or may not be eventually confirmed by experience. At present they are powerful and ingenious theories which enables us to map out our knowledge to arrange and classify it, but I humbly crave permission to doubt whether any of them may be considered a final solution and *pro tanto*, I must have my hands free. This is not really of consequence in our discussion to-night, for I have no intention of entering into such a difficult and crooked controversy. The nature of heat is, in fact, outside our present subject. Eschewing the transcendental problems we shall very briefly consider only the direct effects of heat upon matter. So far then as our experience can take us heat is a peculiar condition or phenomenon of matter, which we can modify, increase, or lessen, which is universally present in greater or lesser intensity, and which we measure relatively by a comparison with a certain fixed standard. However produced, whether by chemical action, by percussion, by friction, &c., &c., there is one result which seems universal and which may be considered to be the correlative of heat. This result is universally present and modified in various ways it is probably the only result. So that if we exclude the feeling of heat we may define heat by using this result as an alternative term. The addition and abstraction of heat are correlative respectively with an increase and decrease in the bulk of the matter operated upon. If we add heat we increase the bulk of the substance operated upon; if we abstract heat we cause the portion of matter to contract and shrink, and the bulk is increased and diminished in certain definite proportions; and not only so, but the opposite holds good also, namely, that if we cause a portion of matter to shrink we must in doing so abstract heat from it, while if we increase its bulk in any way without adding more matter we must, willingly or unwillingly, add a corresponding portion of heat. I believe this is accepted as an axiom in physics by the best judges and accepted as universally true. Formerly some substances, such as bismuth, water between certain temperatures, &c., were quoted as exceptions to this rule, inasmuch as they expand in solidifying; but they are only apparent exceptions, for their aberrant behaviour has been shown, as I think beyond doubt, to be a phenomenon of crystallisation. Speaking of the most notorious case, that of water, Tyndal says: "The arrangement of the atoms of water when solid require more room than they need in the neighbouring liquid state. No doubt this is due to crystalline arrangement. The attracting poles of the molecules are so situated that when the crystallising comes into play, the molecules unite so as to leave larger interatomic spaces in the mass; we may suppose them to attach themselves by their corners, and in turning corner to corner to cause a recession of the atomic centres. At all events their centres retire from one another when solidification sets in. By cooling then this power of retreat and of consequent enlargement of volume is conferred."—Tyndall's *Heat as a Mode of Motion,* 107. These exceptions then I hold to be no exceptions at all. There is one substance, namely indiarubber, whose behaviour is eccentric and not explainable in this way, nor at present, so far as I know, explainable at all; but with this solitary exception, whose *raison d'être* I have no doubt will be shown to be as consistent with the general law as those of substances such as water, bismuth, &c., we may safely conclude from our experience of matter that it is a universal law that heat and expansion, cold and contraction are correlative terms.

Having laid down an abstract axiom, let us now apply it to a concrete example, namely, the earth.

The old notions about the stability of the earth when compared with the mobility of the sea have been long since exploded. We now know that there is no such thing as absolute terra firma. We know that the earth in a less degree is mobile like the water, rising here and sinking there in apparently restless pulsations, waves, swellings, and subsidings. This being so, it becomes an interesting problem to determine whether these risings and sinkings compensate one another; that is, whether a rising in one neighbourhood corresponds to a sinking elsewhere, or whether the whole periphery of the earth is undergoing enlargement or diminution, is stretching or shrinking. To decide this by direct experiment is not easy, for since water is our gauge the same relative effect will be produced either by the sinking of an ocean-bed or the rising of an adjacent continent. But we have a considerable amount of evidence notwithstanding which points, so far as I know, in one direction and one only. There is first the a priori evidence.

There are two sciences which deal with the inner constitution of the earth: astronomy, which deals with it as a part of the great macrocosm, the universe; and geology, which deals with its inner life as a universe of its own. The question of the alteration in the earth's bulk has naturally been discussed by both astronomers and geologists, and discussed too from very different points of view, but both are agreed in one conclusion, namely, that the earth is shrinking.

Since the days of La Place the Nebular hypothesis has been generally received by astronomers as the one which best meets observed facts. This hypothesis predicates the existence of gravitation everywhere, and shows how by its influence the various heavenly bodies have become condensed from nebular matter. It predicates that this force is still active everywhere, and that everywhere within our observation we have a condensation of matter in progress, matter condensing from a highly diffused condition to one of greater density. Thus each member of our system, it is argued, is gradually and surely nearing the sun and at the same time is shrinking, and the various planets are in fact in so many stages of evolution, and exhibit for us the various phases which the earth has passed through and will pass through before it is landed in the sun. And the most ingenious and successful of our analytical astronomers and physicists combined, Sir William Thomson, has compared our universe to an elaborately constructed clock which is inevitably and surely running itself down, until it has exhausted its various forces and until each component member has fallen into the common centre. This is elementary enough. I only quote it to show that the evidence of astronomy is, that the earth is contracting, and that its periphery is diminishing in area.

The conclusion of geology for our purpose is the same. It is argued by geologists that the earth was originally in an incandescent state, and that it has assumed its present shape after a gradual cooling, that is, a gradual contraction, which is still in progress. These are the words of Mr. Geikie, one of the most recent and competent authorities: "Among the geologists of the present day there is a growing conviction that upheaval and subsidence are concomitant phenomena, and that viewed broadly they both arise from the effects of the secular cooling and consequent contraction of the mass of the earth." On both grounds therefore, namely, those upon which astronomical and geological arguments are based, is it established that the earth is shrinking.

As the fire syringe discharges a certain amount of heat when sudden pressure is applied, which heat we can collect

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and measure, so we ought to be able to measure the amount of heat produced by the contracting of the earth's crust, although we cannot measure that contraction with a plummet. As we go down through the crust we ought to meet with evidence that the pressure of the strata has increased the temperature, and this we in fact do. In going down towards the centre of the earth we find an increase of temperature so wonderfully constant in all latitudes that we are constrained, if we accept the nebular hypothesis, to argue that this results, as it ought theoretically to result, from the pressure of the strata, *i. e.*, from the force of gravity. In some letters that I have recently written to "Nature," I have tried to show how the areas of upheaval and subsidence on the earth are distributed, my conclusion being that the areas of depression are distributed about the Equator, while the areas of upheaval have their foci at the Poles, that the earth is being strictured about the Equator, and thrust out in the direction of its shortest axis; in other words, is shrinking in the region where the temperature is the highest, the tropics, and is stretching out in the regions of cold, or the Polar regions.

Now let us see how far we have travelled. We have postulated that all shrinking matter is giving out heat. We have shown that the *a priori* evidence is conclusive that the earth is a shrinking mass. We have also adduced evidence which shows that the earth is in fact hot where it should be hot, and cold where it should be cold, in accordance with the law of contraction; and it seems to me to follow necessarily that the earth is giving out heat,—is in fact a furnace, a heat producing substance. This seems to be inevitable.

I need not stay to argue that both popularly and also among scientific men it is held as a cardinal doctrine that the earth receives a large quantity of its heat directly from the sun, and elaborate calculations have been made to show the terrific quantity of heat so received and the terrific energy which this represents. We are taught that of the sun's heat which beats on the earth one portion is reflected, another is absorbed, and that the latter is what we can alone recognize by our instruments, and whose energy is the subject of calculation. But the absorption of heat means, as we have argued, an increase of bulk, therefore whatever heat the earth absorbs must go to increase the earth's size. It is only on this condition that the earth can absorb heat at all, and it is only by the increase in bulk that we can in fact measure or gauge the heat.

But if the earth be shrinking it is clear that it must give out more heat than it absorbs, it must in fact produce enough heat to neutralize the expansion caused by the sun and some besides. The excess being measured by the amount of contraction that the earth is undergoing. But this means that any heat it receives from the sun is more than neutralized by its own heat, so that if the sun gave us no heat at all and the earth continued to contract, as it does now, it would not be affected in temperature, save perhaps in becoming even hotter, for if we receive heat from the sun which, when absorbed makes the earth expand, it is clear that a portion of the contracting force of the earth is spent and exhausted in neutralizing this expansion, which would be set free in the form of heat if this had not to be neutralized. But I confess that having brought the argument to this point I am constrained to go a step further, and to say that if the earth be independent of the sun for its heat, that if independently of the sun altogether it is throwing out an amount of heat equivalent to the amount of its contraction, that it is unnecessary and unphilosophical to postulate the sun as a source of heat, and that we are bound, paradoxical as it may seem, to conclude that the earth does not receive any heat directly from the sun. This conclusion seems inevitable, and if it be, we must face the various difficulties that suggest themselves at every turn and find a solution to them consistent with it.

I believe these difficulties are not hard to meet. It will be noticed that in the question I propounded at the head of this paper I use the word directly or immediately. When I say that the earth is itself the furnace which supplies the phenomena of heat which we study and feel I do not mean that it does so entirely *per se*, and without any assistance from without. I predicate all through that such heat as the earth possesses is induced in it by the force which is contracting it, by the gravitating force, and this force, so far as our evidence goes, is derived nearly altogether from the sun. My argument is that the heat of the earth is mediately, but not immediately due to the sun's influence, that the sun's influence causes the earth to contract, and that in contracting heat is squeezed out of it. That we derive no heat whatever *qua heat* from the sun, or to use a simile, the voltaic battery supplies the electric current which induces the magnetism in the iron but does not itself furnish the magnetism.

This at once removes a vast quantity of apparent difficulty, for wherever the sun beats there it exercises its influence of gravity, and the result is invariably an induction of heat, so that winter and summer, day and night, are dependent for their varying temperature on the sun, although in a different manner to that popularly supposed.

There is another and a more palpable difficulty that obtrudes itself at once upon one's notice in defending the position I am arguing for. It is said: surely, if we step out of the sunshine into the shade, or *vice versa*, and exclude the radiated heat from the ground we shall find direct evidence both from our feeling and from the usual effects of heat that the sun's rays are distinctly hot, that in beating on our hand, on a moist surface, &c. &c., symptoms of absorption of heat at once present themselves—the hand grows hot, the moisture evaporates, &c. And this is true in a larger way of the superficial layer of the ground, which feels the intermittent effects of day and night, summer and winter, by absorbing more or less heat. How then do we account for this, for the heat must be absorbed from somewhere, and the only practicable source is the direct sunshine. My answer to this is very short. If we ascend through the atmosphere we rapidly find the temperature becomes lower, that as we get nearer the verge of the atmosphere, and therefore nearer the point where there is no atmospheric absorption of the solar rays we find the temperature to lower so rapidly that we are justified in concluding that if we could get outside the atmosphere altogether we should find the scorching of the hand, the absorption of moisture, and the various effects we attribute to heat reduced to a minimum, and are justified in concluding that if the envelope of the atmosphere were removed, if the earth were to be, such as the moon is, without an atmosphere, that its surface temperature would be but slightly affected by the direct sunshine. The effects of the sun's attraction would be distributed throughout the mass of the earth, causing a general contraction and a general relative temperature with its focus at the centre, but there would be no surface layer of an aberrant and peculiar temperature, and few or none of the effects we now find in the direct sunshine. I argue, and I think I am justified in arguing, that these peculiar effects are due entirely to our having an atmosphere, to the fact of there being a medium between the sun and ourselves. If this be so, it is clearly most consistent with our contention, for the sun's contracting force acts not only on the earth's solid matter but on its gaseous envelope also, and in the latter case with much more powerful results. So that any surface exposed to the sunshine is exposed also to a column of air undergoing contraction or pressure on the part of the sun, and as this compressed air must give out its heat if it is in contact with any body at a less tension, it gives it out to my hand or to the moisture exposed to it, which in the one case feels hot and in the other experiences an effect of of heat, namely evaporation. It is the column of air that is giving out heat, each ray that pierces it squeezes it and squeezes out of it a certain amount of heat which we attribute directly to the sun's influence, while in fact it is only mediately due to it. This I think satisfactorily explains the great and elementary difficulties of my position. I am not aware of any other difficulty which cannot be as easily met, and as I cannot see my way from escaping from the main conclusion at which I have arrived, I have ventured, paradox though it be, to present it for your criticism. To sum up this conclusion in a phrase, I hold that the earth receives no heat directly from the sun, the sun only supplying the contractile force which induces terrestrial heat.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 31st, 1874.

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

Vice-Presidents.
E. W. BINNEY, F.R.S., F.G.S.
JOSEPH BAXENDELL, F.R.A.S.

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

Treasurer.
SAMUEL BROUGHTON.

"The Meteorological Theory of Cometary Phenomena," by DAVID WINSTANLEY, Esq.

On the 16th April, 1872, I had the honour of bringing under the notice of this Society the outline of a theory by which I showed, or endeavoured to show, that in the instance of bodies surrounded by abnormally voluminous atmospheres and subject to sudden and violent thermal changes, certain phenomena indistinguishable from the imposing aspects frequently presented by comets must of necessity ensue as the ordinary phenomena of their meteorology. So far as I have seen no criticisms have been advanced disputing the correctness of this deduction. If, therefore, I am able to satisfy you that any particular comet really was surrounded by an abnormally voluminous atmosphere, that it was subjected to sudden and violent thermal changes, and that it also exhibited the imposing aspects to which I have alluded, I think I am entitled to ask you to regard the phenomena in question as meteorological phenomena, and to regard the meteorological theory as affording a satisfactory explanation of the appearances presented at any rate by that particular body.

The analyses of meteorites falling upon the surface of the earth from the regions of external space have not, it appears, led to the discovery of any new chemical body*, whilst the spectroscope has shown that chemical substances, common enough upon the earth, exist in the solar atmosphere, in the atmospheres of his attendant planets, in the fixed stars themselves, and in the far distant nebulæ. In short, we have direct evidence that whatever quantitative differences there may be, qualitatively the community of matter extends throughout the fields of nature.

This fact at once disposes of those cometary theories which assume the existence of some new chemical body as a necessary agent in the production of cometic phenomena, and it also disposes of those objections to other theories founded upon the supposition that the ordinary operation of physical laws as observed upon the earth may be rendered inoperative in the case of comets by a non-community in their chemical nature.

We have, it is true, very little special evidence as to the chemical constitution of comets. What little we have has been furnished by comparatively insignificant members of the family, in unfavourable circumstances of luminous intensity, and with indications as yet unsatisfactorily interpreted.† Evidence has however been adduced that comets for the most part consist of matter moving between us and

* Guillemin's "Heavens," Eng. trans., page 175.

† See "Nature," Jan. 8th, 1874.

certain distant stars, matter which is picked up and appropriated by our system in its onward progress through the depths of space.* This in itself removes any ground for regarding comets as possible non-partakers in the universal community of matter. The spectroscope has shown however—and this is strictly to the point in the present inquiry—that of whatever ingredients comets may be composed they certainly are not as a class destitute of volatilizable ingredients.† The physical condition of all terrestrial substances—and in view of the community of matter the physical condition also of extra-terrestrial materials is merely a question of temperature and pressure. In short, given a sufficiently high temperature and a sufficiently low pressure and anything will vaporise.

The great periodic comet of our system, the body named after the illustrious Edmund Halley pursues an orbital path of such eccentricity that it is sixty times more remote from the sun in aphelion than in perihelion and is therefore subjected three thousand six hundred times more to the influence of the solar radiance in the latter position than in the former, a circumstance very suggestive of meteorological phenomena on a grand scale, when we consider that those terrestrial changes which to us seem so momentous are merely local circumstances resulting from local variations of presentation to the sun, from which, as a whole, the earth maintains an all but constant distance. The great comet of 1843 came from an aphelion point, at which the sun's apparent magnitude would only be the two hundredth part of one degree, and the influence of his light and heat one ten thousandth part of that which we enjoy. But this same body in its perihelous passage almost grazed the very surface of the sun approaching the photosphere within one-seventh of the solar radius,‡ and being there exposed to the terrific

* See "Nature," Jan. 22nd, 1874, p. 239.

† See "Roscoe's Spectrum Analysis," p. 290.

‡ Herschel's "Outlines," tenth edit., p. 400.

glare which the earth would feel were it illuminated by 47,000 suns instead of one. A far inferior degree of heat has been found to melt the most refractory rocks and dissipate materials of prodigious fixity, and there can be no doubt that this comet acquired "a temperature high enough to convert even carbon into vapour."*

The *a priori* evidence is complete. There can be no doubt the comet of 1843 possessed an atmosphere, and the only question now is one of its extent. I believe the actual depth of the terrestrial atmosphere is still unknown, though it is estimated as extending to a distance from the surface of more than seventy miles.† Were the mass of the condensed portion of the earth by any means diminished so as to lessen the coercive power exercised thereby over its gaseous parts, the volume of the latter would, according to Herschel, be increased in a proportion exceeding that in which the central mass had been diminished. The mass of comets is so small as to have furnished, so far as I can find, no data for its determination in any individual case. Whatever mass we may suppose the comet of 1843 to be possessed of consistent with this fact, it is clear its atmospheric limits must have been prodigiously remote from the solid matter of the nucleus. Indeed if we consider the recession resulting from increased distance from the central gravitating mass, no volume of atmosphere containable within the known extension of our system would seem too great to be attributed thereto.‡ The deduction from this is that the phenomena resulting to a body of extended atmosphere and subjected to violent thermal change are phenomena which

* Roscoe's "Spectrum Analysis," p. 352.

† Herschel's "Outlines," tenth edit., p. 711.

‡ "Newton, with the view of illustrating this point, calculated that if a globe of common atmospheric air one inch in diameter was expanded so as to have an equal degree of rarity with the air situated at an elevation above the earth's surface equal to the earth's semidiameter, it would fill the whole planetary regions as far as Saturn."—Grant's "History of Astronomy," p. 298.

should have been exhibited in an exceptional degree by the comet of which we speak. And all accounts agree that it presented "a stupendous spectacle." Its tail stretched to a hitherto unheard of distance, and so brilliant an object was this body that it could be seen by daylight and in the immediate neighbourhood of the sun.

It is however only on the gratuitous assumption that comets are composed of the most refractory materials in the universe that any close proximity to the sun is needed as an argument in favour of their having atmospheres. Admitting the community of matter and considering the rigorous cold they must experience in aphelion, it is but reasonable to suppose that as a rule the production of their atmospheres by the evaporation of their nucleii is commenced at distances from the sun even greater than the distance of the earth, and there is plenty of evidence to corroborate this view. The great comet of 1811 presented irrefragable evidences of the existence of a transparent atmosphere certainly several thousands of miles in extent,* and yet its nearest distance from the sun exceeded the distance of the earth! whilst during the last return to perihelion of Halley's comet in 1835 Sir John Herschel observed appearances which satisfied him that the nucleus of the comet was "powerfully excited and dilated into a vapourous state by the action of the sun's rays, escaping in streams and jets at those points of its surface which oppose the least resistance, and in all probability throwing that surface or the nucleus itself into irregular motions by its reaction in the act of so escaping."† Yet the perihelion distance of this comet was as much as 0·58 of the mean distance of the earth.

The close approximation to identity observed in the orbital

* See observations of Dr. William Herschel in Phil. Trans., vol. 102, pp. 134 to 136.

† Herschel's "Outlines," p. 382.

paths of certain comets and certain meteoric streams is a matter of too serious moment to be passed over without attention by a cometary theorist. The physical connection which it indicates is one to be accounted for. It may be, firstly, that meteors are the uncollected elements or particles from which comets are built up, or secondly, that meteor streams and comets are identical,* or thirdly, that by disintegration and dispersion the constituents of a comet have become a meteor stream.

So far as I have found no evidence has been adduced that comets aggregate material in their journeys round the sun whilst their supposed identity with meteor streams is merely an hypothesis built upon no observation and failing to explain with clearness any of their distinguishing phenomena. There is however evidence enough that sub-division is not uncommon amongst bodies of the cometic class.† Bi-partition is recorded in the Chinese annals four centuries before the birth of Christ, whilst of modern evidences of disintegration the separation of Biela's comet into two in 1846 places the phenomena of sub-division beyond the reach of doubt. The fact of Biela's total loss and the existence of a meteor stream along its former path seem to establish complete disintregation as a known result and a meteor stream as an observed effect. During the apparition of Donati's comet in 1858 an appearance was remarked in connection with the tail which Bond considered due to a quantity of scattered and abandoned gas.‡ But according to that theory which it is the object of this paper to advance the disintegration of cometic bodies is not a matter for surprise. For considering the enormous distances to which according to that theory some portion of the cometic matter is removed and the manner in which it is dispersed it is

* See "Nature," vol. 4, p. 268.

† See "Kirkwood on Disintegration." Nature, vol. 6, p. 148.

‡ "Annals Harvard College," vol. 3, p. 158—60.

quite inconceivable that it should ever be wholly re-absorbed. When its vapourous condition ends a flight of meteors must result whose densest part will lie along the comet's former path.

It was a favourite speculation of the elder Herschel* that in their perihelion passage comets lose some portion of their more elastic matter, so that what remains behind assumes a condition of greater consolidation, whilst other astronomers have believed that at each return a comet loses something of its former splendour.† Upon whatever evidence these beliefs may rest, the circumstances which they indicate are a necessary sequence if the theory here advanced be true.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 16th, 1874.

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

Mr. BAILEY brought under the notice of the Section a method which he had used for some years of exhibiting slides under microscopes, the audience being seated. This was described and explained, and it was proposed that the Council be requested to take the matter into consideration with a view to its being put into use at the meetings of the Section.

Mr. T. S. PEASE exhibited and explained a series of slides which he had brought for examination, showing the internal anatomy of the cockroach.

* "Observations at the Cape," p. 411.
† Smyth's "Cycle," vol. 1, p. 235.

March 16th, 1874.

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

Mr. HURST laid on the table some of the expensive Natural History works it had been decided to purchase with the fund given by the Manchester Natural History Society.

Mr. SIDEBOTHAM exhibited an old book cover of the date 1683, mined by a very rare beetle, *Hypothenemus eruditus*. This species was discovered more than 40 years ago by Professor Westwood and named by him, since which time no other specimen has been seen until this year, when it was met with by Mr. Janson. Mr. Sidebotham met with several specimens in this old book cover, one of which was living.

Mr. Sidebotham also mentioned that during the frost last week he noticed the steam from an engine at London-road Station speedily converted into snow, and on examination of the crystals with a pocket lens found them to consist of unusual forms of the most simple description, equilateral triangles, reels with triangular ends, &c., and a total absence of all wheel-like forms.

Annual Meeting, April 21st, 1874.

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 Treasurer's Account for the past year shows that the general balance has increased from £407 1s. 4d. on the 31st of March, 1873, to £435 13s. 1d. on the 31st of March last. It must however be remarked that this increase is due to the fact that no expenditure has been incurred during the year for binding books, in consequence of the Hon. Librarian having been unable to give his attention to the business of the Library as heretofore.

The number of ordinary members on the roll of the Society on the 1st of April, 1873, was 169, and eleven new members have since been elected; the losses are, deaths, 4; resignations, 3; and defaulters, 2. The number on the roll on the 1st of April instant was therefore 171. The deceased members are Thomas Carrick, Peter Higson, Thomas Turner, and Frederick Crace-Calvert.

Mr. Thomas Carrick was born at Rockcliffe House, five miles from Carlisle, on the 12th of April, 1816. He was the eldest son and third child of David Carrick, Jun., banker, Carlisle, and Sarah his wife, *née* Brockbank, of Ulverstone. When three years old his life was nearly lost by fire; his clothes were ignited in nursery play, and he was severely burned. For many succeeding years he was a suffering and delicate boy, naturally shy and diffident, quiet and fond of books, and not joining much in the active sports of boys of his own age. His father died in 1821. In 1825 and 1826 he occasionally attended a mixed school in the village of Stanwix, Carlisle, taught by a master in reading, writing, and arithmetic. In December, 1826, he was placed at a boarding school of the Society of Friends, at Wigton, Cumberland, and remained there till October, 1829. After an interval at home he was two years at the late Samuel Marshall's boarding school at Kendal. Mr. Marshall was interested in his pupil, and sent specimens of his writing and drawing to William Johnson and Sons, then land surveyors in Manchester. These specimens, with Samuel Marshall's recommendation, procured him a favourable entrance as an apprentice into Wm. Johnson and Sons' office, in 1832, and to their honour they received him without premium because his mother was a widow. At the close of his apprenticeship he took offices in Brown Street on his own account as land surveyor, and was much engaged on railways by C. F. Cheffins, of London. His home was with the late John B. Brockbank's family, Salford, and at J. B. B.'s death the partnership of Carrick and Brockbank commenced. He was a most dutiful son to his widowed mother up to her death in 1844, and at all times a considerate and loving brother.

Mr. Carrick became a member of the Literary and Philosophical Society in January, 1857; he was elected treasurer on the 6th of October, 1868, and continued to fill this office till the time of his death, May 27th, 1873. He contributed the following papers to the Society:

Feb. 22, 1859.—"On the grouping of unexplained Cosmical Phenomena."

Oct. 13, 1859.—"On Orbit Inclination."

Dec. 8, 1859.—"On the Sun's Orbit plane."

Feb. 2, 1860.—"On the Moon's Orbit plane."

Oct. 11, 1860.—"On the Atomic Constitution of Water and Ice."

Apr. 30, 1863.—"On the wave of High Water: a new Theory of Tides."

In this paper he certainly accounted for many tidal phenomena, and brought together many curious facts gleaned from a wide circle of reading. His views of cosmical phenomena were original, and he frequently took part in discussions from his own peculiar point of view.

Mr. Thomas Turner, F.R.C.S., was born at Truro, the 23rd August, 1793. His father, Mr. Edmund Turner, was a banker in that town. His mother was descended from an old Cornish family named Ferris. Mr. Thomas Turner was the youngest of three sons. The eldest, Edmund, succeeded his father as a banker, and became a member of Parliament, representing his native town in the Liberal interest for 20 years. The second son, Charles, a lieutenant in the East India service, was killed by a sunstroke in India. There were also two sisters older than Mr. T. Turner, both of whom are dead. Educated at Truro Grammar School, he left for Bristol, in 1811, to be apprenticed to Mr. Duck, who was surgeon to St. Peter's Hospital in that town. Mr. Turner left Bristol for London in 1815, and entered as student under Sir Astley Cooper at the United Hospitals of Guy's and St. Thomas's. He passed at the Royal College of Surgeons and Apothecaries' Hall in 1816. After this he studied in Paris, and became acquainted with most of the eminent men who flourished in the Parisian schools at the time. He left Paris in 1817, and it was through the influence of relatives then residing in Manchester that he was appointed resident house surgeon to the Manchester Workhouse. He remained there until the autumn of 1821, when he commenced practice in Piccadilly, opposite the Royal Infirmary.

Mr. Turner was appointed secretary to the Manchester Natural History Society in 1821, and continued for very many years to take an active interest in its progress. The Royal School of Medicine and Surgery, heretofore in Pine Street, and which Mr. Turner has lived to see united to that noble academical establishment, the Owens College, owes its first complete organisation to Mr. Turner. In the autumn of 1822 he commenced his first course of lectures. They were given at the rooms of the Literary and Philosophical Society, in George Street, on the anatomy, physiology, and pathology of the human body. These lectures Mr. Turner regularly continued till October, 1824, when he established the Medical School, with the object of enabling local practitioners to study their profession completely without the necessity of going to London. Dr. Dalton became the lecturer on chemistry at Mr. Turner's Institution.

In 1831 Mr. Turner was elected surgeon to the Manchester Royal Infirmary, the duties of which office he zealously performed for 25 years, when he resigned, and was appointed honorary consulting surgeon. This position he held to the last. In 1843 he was appointed honorary professor of physiology to the Royal Institution, when he annually delivered a course of lectures—the last course being delivered 12 months ago. Mr. Turner has also been honorary surgeon to the Manchester Deaf and Dumb School from its foundation till now. Of the Infant Deaf and Dumb School he was the chief promoter, and he laid the foundation stone of the building at Old Trafford in 1859. He also originated the Manchester and Salford Sanitary Association in 1851, and was always one of its most earnest supporters. His loss will be very widely felt, from the systematic beneficence which he practised as a medical man.

Mr. Turner was made Fellow of the Royal College of Surgeons of England in 1843, and elected to the Council of the College in 1866. He resigned his seat there last July. In addition to those already enumerated, Mr. Turner held the positions of Fellow of the Linnæan Society, and honorary member of the Harveian Society, London. He was the author of "Outlines of Medico-Chirurgical Science;"

"Observations on Aneurism and Hæmorrhage," "Provincial Medical Education," "Treatise on Dislocations of the Astragalus and Injuries of the Foot," "Retrospect of Anatomy and Physiology," and other publications.

Mr. Turner married, in 1826, Anna Mary, daughter of Mr. James Clarke, of Needham, near Newport, Isle of Wight. One of Mr. Turner's sons is a clergyman at Stockport; another a lieutenant in the army. One daughter, married, is resident in Shropshire. The youngest daughter was her father's devoted nurse at home. His illness lasted many weeks, during which there was no hope of his recovery. He was in his 81st year.

Dr. Frederick Crace-Calvert, F.R.S., was born near London on the 14th of November, 1819.

In the year 1835, when 16 years of age, he left London and went to France, where he commenced the study of chemistry under the celebrated chemist Gerardin, at Rouen, and continued with him for two years. At the expiration of this time he went to Paris, and carried on his studies at the Jardin des Plantes, the Sorbonne, Collége de France, and École de Médecine, his attention being principally given to the Natural Sciences.

About the age of 21 he was appointed to manage the well known works of Messrs. Robiquet & Pelletici, where the manufacture of pure chemicals and pharmaceutical products is carried on. This position, however, he soon vacated on being offered that of "Démonstrateur de Chimie Appliquée," under the eminent chemist Chevreul, and here he remained from 1841 till 1846, when he left France. From the former date his career as a chemist began and continued with untiring energy during the succeeding 32 years.

He published his first paper, "Sur l'extraction de quinine et cinchonine," in September, 1841.

In 1843, in conjunction with M. Ferrand, he elaborated an interesting paper on the analysis of gases enclosed in some organs of plants, the gases being taken from the same plants at different times of the day and year to demonstrate the action of the sun's rays. This paper is entitled "Memoir sur la Végétation," and may be found in Volume V. of "The Comptes Rendus." In the following year the diseases of beer engaged his attention, and some interesting facts were embodied by him in a paper read to the Société de Pharmacie, "Sur la fermentation visqueuse de la bière."

From 1843 till the time of his leaving France he was engaged in a research on some compounds of lead which first brought him into note. One of the papers consequent on this may be found in the "Comptes Rendus" of 1843, entitled "Procédé au moyen duquel on obtient un protoxyde de plomb cristallisé et ayant la couleur du minium."

In 1844, he wrote "On the presence of indigo in the Orchidaceous plants;" in 1846, "On the preparation of Calomel on the large scale;" and, in the same year, a compilation of facts relating to the properties of animal black.

On returning to England at the latter end of 1846 he was first appointed to the chair of the honorary professorship of chemistry at the Royal Institution, and afterwards to that of lecturer on Chemistry at the School of Medicine in Pine Street, Manchester.

In 1847 he published a paper "On Bleaching Powders," and in 1848 one "On the Bleaching of Cotton and Flax."

About this time Dr. Calvert gave a long series of lectures on his favourite subject at the Athenæum, Manchester, "The Application of Chemistry to Manufactures." These were recorded in the daily papers.

During the following years many other subjects engaged his attention, but we may notice the following publications as some of the results of his labours.

In 1849, "Process for the Preparation of Chlorates, particularly the Chlorate of Potash."

In 1851 "On the Oxides and Nitrates of Lead."

In 1854 "A case of Poisoning by the Sulphate of the Protoxide of Iron."

In 1855 "On the Adulteration of Tobacco."—"On the Action of Organic Acids on Cotton and Flax Fibres."—"On the Actions of Gallic and Tannic Acids in Dyeing and Tanning."

In 1856 "On the Solubility of Sulphate of Baryta in different Acids."—"On the Purification of Polluted Streams."

About this time he commenced an enquiry, in conjunction with Mr. Richard Johnson, on the physical and chemical properties of different alloys. The publications resulting from these investigations were:

In 1858 "On the Hardness of Metals and Alloys."—"On the Conductibility of Metals and Alloys."—"On the Chemical Changes which Pig Iron undergoes in its Conversion into Wrought Iron."

In 1861 a series of papers "On the Expansion of Metals and Alloys."

In 1862 "On the Composition of a Carbonaceous Substance existing in Grey Cast Iron."—"On the Employment of Galvanised Iron for Armour Plated Ships."—On the Conductibility of Heat by Amalgams."

In 1863 "On the Preservation of Iron Plated and other Ships."

The interest he took in the preservation of ships from the action of sea water never ceased; many unrecorded experiments were carried on by him at intervals on this subject till the last days of his life.

In 1865 Mr. Richard Johnson and he published "On the Action of Sea Water on certain Metals and Alloys," and in 1866 "On the Action of Acids on Metals and Alloys."

In 1870, two papers appeared by Dr. Calvert, one "On the Composition of Iron Rust," the other "On the Oxidation of Iron," and a third on the same subject in 1871.

In 1858 we find a publication of his "On a New Method of preparing Hydrochloric Acid."

In 1859 "On the Analyses of Wheaten Flours."—"Influence of Science on the Arts of Calico Printing."—"On Starches: the purposes to which they are applied, and improvements in their Manufacture."

During this year, his attention was called by the late Dr. Ransome to hospital gangrene, and in seeking its cause he was led to investigate the compounds produced during putrefaction.

Two papers, descriptive of his results, appeared in 1860, the first "On Products of Putrefaction." The second "On New Volatile Alkaloids given off during Putrefaction."

He continued, during the following two or three years, with these researches, and had collected about an ounce of a precipitate, produced by combining the gaseous products of putrefaction with platinum, by passing the gases emitted by the putrefying meat through bichloride of platinum, by means of aspirators, during many months. This accumulation of precipitate was unfortunately destroyed before its examination could be completed, through the carelessness of one of his assistants, which caused him much regret ever afterwards.

In 1861 he wrote "On Improvements in the Manufacture of Coloring Matters," and "On the Chemical Composition of Steel." A report followed "On the Action of Water supplied by the Manchester Corporation on Lead of different kinds," in connection with the Manchester Sanitary Association.

In 1862 he gave a series of lectures to the Society of Arts, "On the Improvements and Progress in Dyeing and Calico Printing since 1851;" in 1864 "On Chemistry as applied to the Arts;" in 1866 "On Discoveries in Agricultural Chemistry," and "On Discoveries in the Chemistry of Rocks and Minerals."

These were the beginning of the Cantor lectures, which are now continued every year by different lecturers.

In this same year we find another paper by him, "On Wood for Shipbuilding."

In 1863 he patented and worked at his process for the separation of sulphur from coke, by use of common salt, for the purpose of the manufacture of iron of superior quality.

The following is a list of some of his further publications:

In 1865 "On the Action of Silicate and Carbonate of Soda upon Cotton Fibres."—"On the Crystallized Hydrate of Phenic Alcohol."

In 1866 "On the Hydraulicity of Magnesian Limestone." "On the Preparation of Acetylene."

About this time he interested himself with the properties of phenic or carbolic acid, and being satisfied of its valuable disinfecting properties, built works for its manufacture, and to him belongs the honour of having first brought it in a pure state into commerce.

In 1867 he wrote papers "On Oxidation by means of Charcoal."—"On the Presence of Soluble Phosphates in Cotton Fibres, Wheat, and other Seeds," and five articles "On the Synthesis of Organic Substances."

In 1867 "Carbolic or Phenic Acid and its Properties" (three articles).

In 1869 "Presence of Soluble Phosphates in Seeds."—"Preparation of Nitrogen."

In 1870 "Testing Petroleum."

In 1872 "Sulphur in Coal and Coke;" and papers on "Protoplasmic Life;" "Vitality of Disease Germs," &c. Part of the latter series remains yet unpublished.

In concluding the list of Dr. Crace-Calvert's various researches we may mention that, besides the above, many others were made by him, but their unfinished state does not justify publication. Among these may be mentioned one on "Light," which cost him much labour, and one "On the Action of different Gases on each other under Enormous Pressures."

He was a Fellow of the Royal Society, of the Chemical Society, and of many other societies both at home and abroad.

Dr. Calvert showed remarkable devotion to the science he studied, and his knowledge of its literature was such as very few have attained, and such also as could only be obtained by a most unusual amount of reading, accompanied with strong interest, and in all probability much pleasure. He showed this knowledge more in the departments referring to industry, and, as might be expected, he intended to give his experience to the public in a more convenient form than his lectures presented. One of these works, that relating to "Colours other than Aniline," was nearly completed—if not entirely so—before his last illness, and is expected to be published shortly. Whilst rather exhausted with this work, added to the attention required for the manufactures in which he was engaged, he was chosen as one of the jury of the Vienna Exhibition. The summer of 1873 was sultry and unpleasant, and other causes may have operated to make it unhealthy; but whatever the reason or combination of reasons may have been (and we cannot doubt that work and anxiety contributed), the result was that Dr. Calvert returned in a very enfeebled state, and a few days after his arrival in Manchester was seized with a fatal illness, which terminated on the 24th of October of the same year.

He was a firmly built man, of middle height, and apparently of unusual strength. His hair was dark, and he seemed to be younger in constitution than his years indicated. His manner was animated, and he had great pleasure in communicating information. It is not attempted in this notice to say to what extent his writings have contributed to the advance of science or the knowledge of manufactures, but in the latter department it is certain that his influence was widely felt, and his friendly disposition enabled him to become a frequent medium of communication between scientific men in England and in France, in both of which countries he felt equally at home. With these combined characteristics the position he made for himself was peculiar, and of its importance we may judge partly by the fact that, although one to which many would be glad to attain, it is not yet filled up.

In accordance with the resolution passed at the last annual meeting the Council has entered into an arrangement with the Manchester Geological Society to afford accommodation for its meetings and room for its library, &c., the Geological Society to pay an annual rent of £25, and also a further sum of £5 per annum for the services of the Keeper of the Rooms.

Steps have been taken for procuring the Incorporation of the Society under the provisions of the "Companies Acts," but a question having arisen whether it may not be necessary to alter the rules of the Society, it has been thought desirable to obtain counsel's opinion on this point before proceeding further in the matter.

An application having been received from the "Scientific Students' Association" for permission to hold its meetings in the rooms of the Society, a resolution will be submitted this evening for the approval of the members, authorising the Council to negotiate the terms and conditions of an arrangement with the Association.

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the present session:—

October 7th, 1873.—"Atmospheric Refraction and the last rays of the Setting Sun," by David Winstanley, Esq.

October 13th, 1873.—"On Specimens of Carex punctata," by Charles Bailey, Esq.

October 14th, 1873.—"Mean Monthly Barometric Readings at Old Trafford, Manchester, from 1849 to 1872," by G. V. Vernon, F.R.A.S., F.M.S.

"Results of Meteorological Observations taken at Langdale, Dimbula, Ceylon, during the years 1868—72," by Edward Heelis, Esq. Communicated by Joseph Baxendell, F.R.A.S.

October 21*st*, 1873.—"On the Relative Work spent in Friction in giving Rotation to Shot from Guns rifled with an Increasing, and a Uniform Twist," by Professor Osborne Reynolds, M.A.

November 4*th*, 1873.—"On the Bursting of Trees and Objects struck by Lightning," by Professor Osborne Reynolds, M.A.

"On some Roman and Celtic Antiquities," by the Rev. W. N. Molesworth, M.A.

"On Vitrified Forts," by Dr. R. Angus Smith, F.R.S., V.P.

November 10*th*, 1873.—"Remarks upon the British locality for Lobelia urens," by J. C. Melville, M.A., F.L.S.

"On Lymexylon Navale," by Joseph Sidebotham, F.R.A.S.

November 18*th*, 1873.—"On the Bursting of Trees and Objects struck by Lightning," by Professor Osborne Reynolds, M.A.

"On the Colour of Nankin Cotton," by Edward Schunck, Ph.D., F.R.S., V.P.

"An Improved Method for preparing Marsh Gas," by C. Schorlemmer, F.R.S.

December 2*nd*, 1873.—"On a Remarkable Thunderstorm at Brockham Green, Surrey, on the 7th November, 1873.

December 8*th*, 1873.—"On a Collection of Shells from the Worden Gravel Pit, near the Leyland Station, near Chorley," by R. D. Darbishire, F.G.S.

December 16*th*, 1873.—"Method of Construction of a New Barometer," by J. P. Joule, D.C.L., LL.D., F.R.S., &c., President.

"On the Explosion of Water," by C. Piazza Smyth, F.R.S., Astronomer Royal of Scotland.

"On the Destruction of Sound by Fog and the Inertness of a Heterogeneous Fluid," by Professor Osborne Reynolds, M.A.

"The Chemical Constitution of Bleaching Powder," by C. Schorlemmer, F.R.S.

December 30th, 1873.—"On the Rapid Growth of the Bar at the Entrance into the Queen's Channel, Liverpool," by E. W. Binney, F.R.S., F.G.S., V.P.

January 13*th*, 1874.—"On Further Improvements in a

Mercurial Air Exhauster," 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.

"On Crystalline Sublimed Cupric Chloride," by S. Carson (Student in the Laboratory of the Owens College). Communicated by Professor H. E. Roscoe, F.R.S.

"Memorandum on Brown-stapled Cotton," by Major R. Trevor Clarke. Communicated by Dr. E. Schunck, F.R.S., V.P.

"On the Graphical Representation of the Movements of the Chest Wall in Respiration," by Arthur Ransome, M.D., M.A.

January 19th, 1874.—"On the Similarity of Certain Crystallised Substances to Vegetable Forms," by Joseph Sidebotham, F.R.A.S.

January 27th, 1874.—"On a Source of Error in Mercurial Thermometers," by Thomas M. Morgan, Student in the Laboratory of Owens College. Communicated by Professor H. E. Roscoe, F.R.S.

"Notes on Fossil Lithothamnia (so-called Nulliporæ)," by Arthur Wm. Waters, F.G.S.

February 3*rd*, 1874.—"On the Theory of the Tides," by David Winstanley, Esq.

"Rainfall at Old Trafford, Manchester, in the year 1873," by G. V. Vernon, F.R.A.S., F.M.S.

February 10*th*, 1874.—"The Northern Range of the Basques," by W. Boyd Dawkins, M.A., F.R.S., F.S.A.

February 24*th*, 1874.—"On the Drainage of Churchyards and Burial Grounds," by E. W. Binney, F.R.S., F.G.S., V.P.

"On the Effect of Acid on the Interior of Iron Wire," by Professor Osborne Reynolds, M.A.

"On the Movements of the Chest in Respiration," by Dr. Ransome, M.A.

March 3*rd*, 1874.—"Results of Rain-gauge Observations at Eccles, near Manchester, during the year 1873," by Thomas Mackereth, F.R.A.S., F.M.S.

March 10*th*, 1874.—"On Professor Renault's Memoir on Specimens of Fossil Plants of the Genus Myelopteris," by E. W. Binney, F.R.S., F.G.S., V.P.

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

"On Dr. Schliemann's Excavations and Discoveries on the site of Troy," by R. D. Darbishire, F.G.S.

"Results of Certain Magnetic Observations made at Manchester during the year 1873," by Professor Balfour Stewart, LL.D., F.R.S.

March 16th, 1874.—"On Some Specimens of Hypothenemus Eruditus," by Joseph Sidebotham, F.R.A.S.

March 24th, 1874.—"On Some of the Perplexities which the Art and Architecture of the Present are preparing for the Historians and Antiquarians of the Future," by the Rev. Brooke Herford.

"A Few Observations on Coal," by E. W. Binney, F.R.S., F.G.S., V.P.

March 31st, 1874.—"The Meteorological Theory of Cometary Phenomena," by David Winstanley, Esq.

April 7th, 1874.—"On the Corrosion of some portions of the Cast Iron Roof of the Salford Station of the Lancashire and Yorkshire Railway," by E. W. Binney, F.R.S., F.G.S., V.P.

"On the Action of Nascent Hydrogen on Iron," by William H. Johnson, B.Sc.

"Does the Earth receive any Heat directly from the Sun?" by Henry H. Howorth, Esq.

April 13th, 1874.—"On the Introduction of Planorbis dilatatus from America into Lancashire," by Thomas Rogers, Esq.

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

Although the system of electing sectional Associates has not yet met with the success which its promoters anticipated, the Council consider it desirable to recommend that it be continued during the ensuing year.

The Librarian reports that having been unable to give much personal attention to the working of the library, the Council found it necessary to secure some paid help to bring up many arrears, and prepare the parcels of Memoirs and

Proceedings now being distributed to the honorary and corresponding members, and to the various learned bodies with whom the Society exchanges it transactions. These parcels are now on the point of being dispatched to their various addresses. No progress has been made in the binding of the books, and the number needing to be bound is now so large as to demand early attention, prompt and regular binding being of great importance in a public library. Considerable shelf space will be required to provide for the accumulation of works which have no fixed place in the library. The number of Societies holding relations with the Society continues as was reported last year.

SAML. BROUGHTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

Dr. FROM MARCH 31ST, 1873, TO MARCH 31ST, 1874. **Cr.**

		£ s. d.	£ s. d.	£ s. d.
1873. April 1.	—To Balance from last year			505 16 4
1874. Mar. 31.	—To Members' Contributions:—			
	169 on Roll April 1st, 1873, at 42s.	354 18 0		
	Less Compounders 4, Deceased 4, In Arrear 8 } 16	32 12 0	321 6 0	
	Arrears—5 at 42s.		10 10 0	
	9 Members elected in 1873, April—Dec.	18 18 0		
	2 ,, ,, 1874, Dec.—Mar.	2 2 0		
		21 0 0		
	Less Arrears	1 1 0	19 19 0	
	11 Admission Fees	23 2 0		
	Less Arrears	2 2 0	21 0 0	
	4 Associates Microscopic Section	2 0 0		
	2 ,, ,, ,, Arrears	1 0 0	3 0 0	
	To *Sale of Publications*:			
	Proceedings	0 12 10		
	Memoirs	0 13 3	1 6 1	
	To *Sundry Income*:			
	Sectional Contributions:			
	Microscopic Section	2 2 0		
	Physical Section	2 2 0	4 4 0	
	To Bankers' Interest		15 12 9	
	Total Income for Year			396 17 10
	To Interest on £1,500 from November 20th, 1872, to December 25th, 1873, Natural History Society, less Tax			64 16 10
				£967 11 0

	£ s. d.
Compound Fund	98 15 0
Natural History Fund	64 16 10
General Balance	435 13 1
	£599 4 11

		£ s. d.	£ s. d.
1874. Mar. 31.	—By *Charges on Property*:		
	Chief Rent	12 14 8½	
	Fire Insurance	7 0 0	
	Inhabited House Duty	2 2 6	
	Property Tax	6 7 6	28 4 8½
	By *House Expenditure*:		
	Water, Gas, Coals, Candles	21 6 11	
	Cleaning and Petty Expenses	3 0 11	
	Tea and Coffee at Meetings	16 12 7	41 0 5
	By *Administration Charges*:		
	Wages of Keeper of Rooms	57 4 0	
	Attendance on Sections	4 4 0	
	Postages and Parcels	19 10 1	
	Stationery, Printing, Circulars, Stamps	12 15 8½	93 13 9½
	By *Publishing*:		
	Memoirs, Printing and Engraving	42 10 0	
	Proceedings, Printing	54 8 3	
	Editor of Memoirs and Proceedings	50 0 0	146 18 3
	By *Library*:		
	Periodicals	25 2 11	
	Subscription to Ray Society, 2 years	2 2 0	
	,, Palæontographical Society	1 1 0	
	Assistance to Librarian	27 0 0	55 5 11
	Total Annual Disbursements		365 3 1
	By Steel Plate of Dr. Dalton		3 3 0
	Total Disbursements		368 6 1
	By Balance in Heywood's Bank	597 11 9	
	,, in Treasurer's hands	1 13 2	599 4 11
			£967 11 0

1874, *April 8th*,
SAML. BROUGHTON, TREASURER.

Audited and found correct, April 14th, 1874,
JOSEPH SIDEBOTHAM,
JOHN A. BENNION.

On the motion of Mr. F. NICHOLSON, seconded by Mr. J. A. BENNION, it was resolved unanimously:—"That the Report just read be adopted and printed for circulation among the members of the Society."

On the motion of the Rev. JOSEPH FREESTON, seconded by Mr. D. WINSTANLEY, it was resolved unanimously:—"That the system of electing Sectional Associates be continued during the ensuing session."

On the motion of Mr. C. BAILEY, seconded by Mr. J. BAXENDELL, it was resolved unanimously:—"That the application of the Scientific Students' Association for permission to hold its meetings in the Society's buildings in consideration of a payment to be fixed be acceded to, and the Council be authorised to negotiate the terms and conditions of such arrangement."

On the motion of Mr. E. W. BINNEY, seconded by Dr. SCHUNCK, it was resolved unanimously:—That the thanks of the Society be given to Mr. Bailey for his valuable services as Honorary Librarian."

The following gentlemen were then elected Officers of the Society and Members of Council for the ensuing year:—

President.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

Vice-Presidents.

JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
REV. WILLIAM GASKELL, M.A.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A.

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Treasurer.

SAMUEL BROUGHTON.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Of the Council.

ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
ALFRED BROTHERS, F.R.A.S.
REV. BROOKE HERFORD.
CHARLES BAILEY.

The following communication from Dr. JOULE, F.R.S., was read by Mr. BAXENDELL:—

"Will you permit me, in reference to Mr. Howorth's communication to the last meeting, to refer to my papers in the Phil. Trans., 1859, p. 91 and p. 133, in which experiments are described proving what had previously been shown by Sir W. Thomson to be corollaries to the dynamical theory of heat, viz.:—That the heat evolved by substances on being compressed is never exactly equivalent to the force of compression; is generally very different therefrom; while in the case of water taken between the limits 32° and 39° Fahrenheit, cold, not heat, is the result of compression. I cannot therefore, admit the axiom on which Mr. Howorth builds his ingenious theory."

PHYSICAL AND MATHEMATICAL SECTION.

April 28th, 1874.

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

"On the Ratios and Frequency of Rainfall, deduced from Observations made at Eccles," by THOMAS MACKERETH, F.R.A.S., F.M.S.

A few weeks ago Thomas Glazebrook Rylands, Esq., of Thelwall, near Warrington, was kind enough to draw my attention to the amount of rainfall at that place for the years 1872 and 1873. The amount which fell there in 1872 was 47·52 inches, and in 1873 it was 24·53 inches. These amounts Mr. Rylands considers extremes of rainfall for that district, and he was led to make several comparisons between them. The results of these comparisons show curiously enough that "the amount of excess in rainfall increases with the amount of rain, and therefore that the increase is due mainly to the maximum falls."

In putting the falls of the two years mentioned into ratio they stand thus, 1·940:1. But the number of days of rainfall in the two years stand thus, 1.235 : 1. Mr. Rylands then introduces into comparison the ratios that exist between other elements; but as those results only apply to his own register, I will not now mention them. I will merely state, having mentioned the rainfall for the two years at that place, that the number of days on which rain fell there in 1872 was 233, whilst in 1873 the number of days was 189. But if the number of days on which rain falls in a wet year is to bear any comparison with those of a dry year the result must be very different, for the ratio of the amounts of rain and the number of days I have given stand thus, 24·53:47·52::189::366, so that if the number of rainy days at Thelwall in 1872 had any proportion with the rainfall it would have rained there on every day in the year instead of on only 233 days.

Seeing that some information might be derived from a similar consideration of all the years of rainfall I had registered at Eccles, I have made the requisite comparisons and beg now to submit the results to the section.

In the following table I present the rainfall at Eccles for the last thirteen years, in the order of the amount of fall in inches, together with the number of days in each year on which rain fell.

RAINFALL AT ECCLES.

Year.	Amount of Fall in inches.	Number of Days of Rainfall.
1865	27·809	177
1870	30·404	178
1864	30·874	180
1873	31·127	219
1868	32·922	208
1871	33·161	192
1861	33·674	202
1867	35·515	211
1869	35·701	210
1863	36·216	221
1862	37·664	225
1866	43·076	232
1872	48·416	264
Mean	35·119	209

Now, if the extremes of rainfall and number of days on which rain fell, as shown in this table, be placed in comparison, their ratios will stand thus:

27·809:48·416::177:308.

Now the numbers of days on which rain fell, in 1872 was only 264; whereas, if this number had been in propor-to the rainfall, it should have been 308.

From the foregoing table I have deduced by means of the average amount of rainfall, and the average number of days on which rain fell, the ratios of the rainfall of each of the last 13 years, and placed each ratio opposite the ratio of the number of days of rainfall of each year. They are as follows, column (*b*) representing the ratios of rain-fall, and (*c*) the ratios of the number of days on which rain fell.

Year.	*(b.)*	*(c.)*
1865	·794	·847
1870	·868	·851
1864	·882	·861
1873	·889	1·047
1868	·940	·955
1871	·947	·918
1861	·962	·966
Mean	·897	·920
1867	1·014	1·009
1869	1·020	1·004
1863	1·034	1·057
1862	1·076	1·076
1866	1.230	1·110
1872	1·383	1·263
Mean	1·126	1·086

From the above table it will be seen that the ratios of the first Means are inverse to those of the second Means; and that when the rainfall was below the average there was a relative *increase* of the number of wet days; but when it was above the average there was a relative *decrease* in the number of wet days. So that in wet years, as a rule, more rain falls at a time than in dry years; and we have more rainy days in proportion to the fall in dry years than we have in wet ones.

"The Cause of Solar Heat," by David Winstanley, Esq.

When a body possessing the visible energy of mechanical translation is arrested in its course, that energy, according to the laws of conservation, is not destroyed, but becomes apparent in another form, generally in the form of heat. Should a body receive two equal impulses in diametrically opposite directions at the same time, mechanical translation as a result thereof is impossible, and the energy thus expended, it appears to be agreed, will in the main assume the form of heat. Should a body however receive two equal impulses in directions inclined to each other mechanical translation will ensue, but the value of the energy of visible motion thus communicated to the body in question is not equal to the sum of the impulses, being, as is well known, justly represented by the diagonal of a parallelogram the length of whose sides is proportioned to the value of the primal impulses. What then in this case becomes of the residue of energy applied and not converted into visible motion? I apprehend it assumes the form of heat. If so in the instance of a body receiving equal impulses in directions inclined to each other at an angle of 120 degrees, the resulting visible motion will just account for the energy of one of these impulses, and the resulting heat for the energy of the other. If then a body having a certain amount of visible energy observable as rectilinear movement have a series of impulses imparted at an angle of 120 degrees in each instance to the line of motion and equal in value to the visible energy of the body at the time of application, the body in question will gain nothing in visible motion through the application of these impulses, but will simply alter the direction of its movement at each application and acquire at the same time an amount of heat of which the impulse is the mechanical equivalent. Adopting this view, it matters not at what angle the impulse may be given, for so long as the figure representing its amount yields with that representing the visible motion of our body a parallelogram whose resultant is equal to the line of visible motion, it follows that the energy of mechanical translation remains unaltered and the energy of the impulse becomes wholly transformed into heat. This condition of things however is what obtains in the instance of a body in planetary revolution in a circular orbit. It is continually receiving impulses through the instrumentality of gravitation at or nearly at right angles to its line of flight, and its energy of visible motion does not increase, whence I infer that the mechanical energy by which its rectilinear movement is destroyed is converted into an equivalent of heat which elevates the temperature of the planet so far as its conditions of radiation will permit. The aggregate value of the impulses required to make a body describe a circle and return to its primal position and direction without loss or gain of visible energy I have endeavoured to determine graphically by means of the mechanical parallelogram. The result at which I arrive is that the aggregate in question exceeds the momentum of the body to be deviated in the same proportion as the circumference of a circle exceeds its radius, and this irrespective of the circle's size. I have not attempted to calculate the temperature to which the matter of the earth would be elevated by a sudden stoppage in its orbital path, for it appears to me that satisfactory data for doing this do not exist. The number of thermal units to which such a stoppage would give rise if multiplied by 6·28 indicate the amount of heat which upon the present hypothesis the earth annually receives through the instrumentality of solar gravitation. This amount will, I apprehend, amply account for a certain initial temperature of the terrestrial matter, and for that excess of internal heat which appears to have been pretty well made out. It will be evident that any heat which in virtue of this hypothesis may be supposed to fall to the lot of our earth will in some calculable proportion fall to the lot of the other planets also. That proportion I apprehend to be directly as the planet's mass multiplied into its velocity in orbit and divided by its periodic time. Of course this amount of heat will be distributed over the entire matter of the planet, the temperature of which will depend on the capacity for heat of the matter composing it and on its facilities for radiation. These latter will clearly diminish with the comparative diminution of superficies enjoyed by the larger planets and with the extension of their atmospheres. In the case of bodies moving in elliptic orbits the amount of energy which according to this hypothesis will be transformed to heat in a single revolution will be practically as before, *i.e.*, 6·28 times the planet's visible energy of motion at its mean distance from the sun. There will, however, be this difference between the two cases. In the instance of a body moving in a circular orbit the accession of heat will be a constant quantity in equal increments of time and in all parts of the orbit, whereas in the case of a body moving in an ellipse the accession will be least in approaching the perihelion and greatest in receding from it, inasmuch as the solar gravitation produces an increase of mechanical translation in the former instance and a diminution in the latter. Assuming the theory to be correct this circumstance is one which will greatly mitigate the extremes of heat to which, apart from such a consideration, we should expect cometic bodies to be subject. Indeed, in spite of the enormous distances to which they recede from the sun, the voluminous nature of their atmospheres by which radiation must be diminished, may, in conjunction with this view of the case, afford an explanation of the unexpectedly high temperature which, in the case of these bodies, the spectroscope certainly seems to indicate.

The application of the views herein advanced to an explanation of the cause of solar heat is briefly this:—As the mutual and ever-acting gravitation of the sun and his attendant planets produces in the instance of the central luminary no more acceleration of visible motion than in the instance of the planets themselves, the gravitation must result in the exhibition of some other form of energy, which in the one case as in the other will, I apprehend, be the form of heat. Carrying, however, this idea to the extreme, we should be led to infer that the force of gravitation, which in some circumstances is certainly capable of transference into the form of mechanical translation, and thence into the form of heat, is in others just as capable of transformation in the opposite order. Indeed, if heat is to be regarded as the individual movement of a multitude of particles whose simultaneous movement is observable as mechanical translation, it is but reasonable to suppose that a force acting upon all, but from the circumstances of the case incapable of producing a simultaneous and concordant movement, will produce an individual and in a sense discordant motion of those same particles. Looking at the matter in this way, any agglomeration of particles whatever must have some initial heat as a result of that gravitation which is unable to evince itself as mechanical translation. And as in our experience gravity is unceasing, this heat so far as we can see should be never ending during the continuance of those conditions which prevent the movement of translation. And a never ending supply of heat must cause an elevation of temperature which will only cease when a point is reached at which the rapidity of radiation equals the rapidity of supply. The more numerous the particles composing the agglomerated body the less become the opportunities of radiation and the more elevated the temperature, which latter attains its maximum in our own system in the instance of the stupendous body which maintains the superficial mundane heat by irradiation from its fires.

It will be seen that the theory here projected places itself in antagonism with the doctrine that "a stone high up" has anything which can be justly termed "the energy of position." But I have already occupied sufficient time without staying now to combat further the doctrine I have named.

To those who have been accustomed to regard gravitation as a property which enables bodies to act where they are not, the present considerations will present difficulties not encountered by others who accept the beautiful, and to my thinking more rational, hypothesis that the simple mechanical movement of infinitesimal particles is the immediate cause of that grand effect the law of universal gravitation.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

April 13th, 1874.

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

Mr. PLANT, F.G.S., exhibited some bones of the extinct Auroch (*Bison priscus*), which had been taken from a deep fissure in the limestone above Castleton, where nearly the whole of the skeleton had been found, together with the bones of the reindeer.

Mr. THOS. ROGERS read a paper "On the Introduction of Planorbis dilatatus" (Gould), a North American fresh water mollusk, which he discovered (June, 1869) adhering to the stones immediately below the surface of the water in the Bolton canal at Pendleton, and in close proximity to the blowing room refuse discharge, and warm water discharge from the engines of Messrs. Armitage's cotton mill. He also afterwards found the same species under similar conditions in the canal adjoining the mills of Messrs. Rylands, at Gorton. After examining all the circumstances under which the mollusk was found (the details of which he placed before the members of the section), he was led to believe that its introduction into this country was by means of American cotton, which had been used for such like war purposes as barricades for steamboats or river defences by the soldiers in the civil war during the presidency of Abraham Lincoln, and which had been accidentally submerged in water and redried with the fry or spawn masses of the Planorbis attached to its fibres previous to its exportation to England, and this ultimately finding its way through the cotton refuse into the canals adjoining the aforementioned mills.

He also remarked the abundance of the beautiful fresh water Zoophyte Plumatella repens, which is found in both habitats of the Planorbis, and on the dead branches of which it seems to find its favourite food. Mr. Rogers said that since the year 1869 (when the mollusk was found in small quantity) it had increased its area of distribution, and multiplied so much as likely to become one of the commonest of our local shells.

Annual Meeting, May 4th, 1874.

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

The following report of the Council for the year ending May 4th, was read and passed:

The following papers and subjects have been brought before the meetings during the session:—

November 10*th*, 1873.—"On Lobelia urens," by J. C. Melvill, M.A., F.L.S., &c.

"On the Occurrence of Lymexylon navale in Dunham Park," by Joseph Sidebotham, F.R.A.S.

December 8*th*, 1873.—"On a Collection of Shells from the Drift," by R. D. Darbishire, B.A., &c.

"On Moths and Butterflies captured at Sea 300 miles from the coast of Brazil," by James Linton.

"On an old Microscope made by Benjamin Martin, a celebrated Mathematician of the last century," by J. B. Dancer, F.R.A.S. The microscope was exhibited by Mr. Plant.

January 19*th*, 1874.—"On the Similarity of certain Crystallised Substances to Vegetable Forms," by Joseph Sidebotham, F.R.A.S. Illustrated with drawings, and specimens under the microscope.

February 16*th*, 1874.—"On a Method of Exhibiting Slides under the Microscope to a number of persons seated at a meeting," by Charles Bailey.

"On the Anatomy of the Cockroach," by J. S. Peace.

March 16*th*, 1874.—"On Hypothenemus eruditus," by Joseph Sidebotham, F.R.A.S.

April 13*th*, 1874.—"On the Introduction into Lancashire from America of Planorbis dilatatus," by T. Rogers.

"On a Bone Cave at Castleton," by J. Plant, F.G.S.

Besides these papers various matters of interest have been brought before the members at the meetings, notices of which appear in the Proceedings.

The Council have to report the donation, by Mr. Rideout, of the valuable old microscope which was exhibited to the members in February, 1873, and a figure and description of which appear in the Proceedings of that date.

The Council have had notice of the payment by the Owens College Trustees of the first interest on the £1500 given to this Section by the Manchester Natural History Society, and as soon as this money has been received by our Treasurer, it is purposed to purchase some valuable Natural History works for the library which are much wanted for reference.

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 £43 19s. 9d.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION OF THE LITERARY AND PHILOSOPHICAL SOCIETY, IN ACCOUNT WITH H. A. HURST, TREASURER.

	£	s.	d.
1873.			
To Expenses connected with the Exhibition of the Spence Collection	3	8	6
1874.			
To Parent Society for use of Rooms	2	2	0
,, Charles Simms & Co., Printing Circulars to 18th March, 1874	4	1	6
,, W. Roscoe, Teas and Postages	4	10	2
,, J. E. Cornish, Microscopical Journal to January, 1874	0	16	0
,, Envelopes	0	16	0
,, Check Book	0	2	6
,, Balance at the Manchester and Salford Bank, St. Ann's Street	43	19	9
	£59	16	5

	£	s.	d.
1873.			
By Balance at Messrs. Heywood's Bank	37	13	0
1874.			
By Associates' Subscriptions	3	10	0
,, Members' Subscriptions	17	10	0
,, Interest allowed by Messrs. Heywood	1	3	5
	£59	16	5

Examined and found correct,
(Signed) JOSEPH SIDEBOTHAM,
JOHN BARROW.

(Signed) H. A. HURST, Treasurer.

Mr. SIDEBOTTOM exhibited a cut flower of *Primula Japonica* the result of Hybridisation.

The election of officers for Session 1874–5 was then proceeded with, and the following gentlemen were appointed:

President.
R. D. DARBISHIRE, B.A., &c.

Vice-Presidents.
JOSEPH BAXENDELL, F.R.A.S., &c.
W. BOYD DAWKINS, F.R.S., &c.
W. C. WILLIAMSON, F.R.S., &c.

Treasurer.
HENRY ALEXANDER HURST.

Secretaries.
SPENCER H. BICKHAM, JUNR.
JOSEPH SIDEBOTHAM, F.R.A.S.

Of the Council.
CHARLES BAILEY.
JOHN BARROW.
ALFRED BROTHERS, F.R.A.S.
THOMAS COWARD.
J. COSMO MELVILL, M.A., F.L.S.
THOS. H. NEVILL.
ROBERT B. SMART, M.R.C.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, HRNRY 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, HRNRY, 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.

HARDY, JOHN.
HUNT, JOHN.
LABREY, B. B.
LINTON, JAMES.
MEYER, ADOLPH.
PEACE, THOS. S.
PERCIVAL, JAMES.
PLANT, JOHN, F.G.S.
ROGERS, THOMAS.
RUSPINI, F. O.
STIRRUP, MARK.
TATHAM, JOHN F. W.
WATERHOUSE, J. CREWDSON.