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NOTE.

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

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PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 4th, 1870.

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

"On Convertent Functions," by Sir JAMES COCKLE, F.R.S., Corresponding Member of the Society.

It was only after some rather intricate and laborious calculations that the possibility, which ought to have presented itself to my mind earlier, of illusory results stealing in and interrupting the processes, occurred to me. And even then I did not at first realize the full extent of such results, or sufficiently explain and illustrate the means which I suggested for escaping them. Perhaps those means may be in some measure inapplicable or impracticable, and the importance and interest which, as I conceive, attach to the subject induce me to enter upon it more elaborately. To do it full justice would require opportunities that I cannot promise myself at present, but I propose, in this necessarily short paper, to show how to obtain convertent functions in certain marked cases. The results here indicated seem to show (1) a correlation between the theory of coresolvents and that of convertent functions, (2) the possibility of arriving at an organized theory of conjugate definite integrals, and (3) the possibility of expressing the Boolian integrals by indefinite integrals. Availing ourselves of a word suggested by Mr. De Morgan, let us call a rational and entire function of v, wherein the coefficients may be any functions whatever of another variable x, a "quotic." Also let us call a rational fraction, whereof the denominator is the m-th power of an irreducible quotic of the the n-th degree, and the numerator a quotic of a degree not exceeding mn—1, a proper fraction of the n-th class. Then the integral, with respect to v, of a proper fraction of the n-th class in general satisfies a linear differential equation of the (n—1)th order, wherein the independent variable is x. But there is an advantageous modification of this theorem. Let θ be a quotic of the n-th degree, and ϑ a quotic of the (2n—1)th degree, and, conforming to the notation of my last preceding paper (*Supra* vol. IX., pp. 86, 87), intituled in the same way as the present supplement to it, let us put

$$\phi(x, v) = \frac{\vartheta}{1+\theta^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Also, in (5) of the preceding paper, let us take the summation on the sinister from $a=0$ to $a=2n-2$ and that on the dexter from $b=0$ to $b=2n\,(2n-2)-1$ and, further, let us put

$$f_b = \frac{G_b v^b}{(1+\theta^2)^{2n-2}} \quad . \quad . \quad . \quad . \quad (11).$$

Moreover let us add a term h, defined by the relation

$$h = \frac{d}{dv}\left\{ H_1 \log(1+\theta^2) + H_2 \tan^{-1}\theta \right\} \quad . \quad . \quad (12),$$

wherein the symbols H, like F and G in (5) and (6), are functions of x only. Then (5) will, after these substitutions are made, be the convertent equation of (10), but it will be observed that in the present paper h is so constructed as to be integrable by means of logarithmic and trigonometrical functions, while in the preceding paper it was supposed to be unintegrable, save by series. Thus $2n$—2 will be the order of the convertent equation, and the possibility of the reduction of order depends upon the circumstance that

$$\frac{1}{(1+\theta^2)^m}\frac{d\theta}{dv}$$

is always integrable in terms of algebraical and trigonometrical functions. When m, which here represents $2n-1$, is greater than unity, the case of m may be made to depend upon that of $m-1$, and so on.

Again, since

$$\frac{\vartheta}{1+\theta^2}=\Theta+\frac{\zeta_1}{1+i\theta}+\frac{\zeta_2}{1-i\theta} \quad . \quad . \quad . \quad (13)$$

where i is an unreal square root of unity we see that the conversion of the integral of (10) may be made to depend upon that of the integrals of two other proper fractions, whereof the denominators are of the n-th degree only. For Θ is supposed to be rational, and consequently immediately integrable. It seems therefore that in many cases, and perhaps universally, the conversion of the integral of a proper fraction may be made to depend upon the solution of a linear differential equation of the first order. For if one and only one particular integral of a certain linear differential equation is a linear function of one and only one particular integral of a second linear differential equation, and also of one and only one particular integral of a third linear differential equation, then each of the three particular integrals may be assigned by means of a linear differential equation of the first order. This is shown as follows. From the first equation eliminate its dependent variable by means of the given linear relation, and call the result the fourth equation. Then eliminate the dependent variable between the second and fourth equations, and the result will be a linear differential equation which will in general have one, and only one, particular integral in common with the third equation. Hence this one integral can be found by means of a linear differential equation of the first order.

(See Boole, Diff. Eq., 2nd ed., pp. 206-7). In like manner, by eliminating a dependent variable between the third and fourth equations, we shall obtain a result which, combined with the second equation, will give us a linear differential equation of the first order for determining the particular integral of the second equation. Use the particular integrals thus obtained in the formation of an integral of the first equation, and substitute the result therein, giving where necessary proper values to the arbitrary constants. Thus an integral of each of the three equations will be found; in the case of the Boolian integrals, the process admits of a simpler application, which is not however in all cases so simple as in that of the cubic. I shall illustrate this application.

Consider the Boolian cubic in y. Transform it into another cubic in z, wherein $z=y^m-1$, m being within Boole's limits, and having no relation to the m hereinbefore mentioned. Then the differential resolvent of the cubic in z is a linear biordinal which may be written thus:—

$$\frac{d^2z}{dx^2}+Z_1\frac{dz}{dx}+Z_2z=\phi(z)=Z \quad . \quad . \quad . \quad (14),$$

and Boole has shown that one of the three values of z satisfies an equation of the form

$$z-\int(\alpha-\beta)dv,$$

the integration being within the limits zero and infinity and α and β being, each of them, proper fractions of the third class. But the integrals of such fractions satisfy linear differential equations of the second order. Hence, putting

$$\int\alpha dv=a, \quad \int\beta dv=b,$$

we may write

$$\frac{d^2a}{dx^2}+A_1\frac{da}{dx}+A_2a=\chi(a)=A \quad . \quad . \quad . \quad (15)$$

$$\frac{d^2b}{dx^2}+B_1\frac{db}{dx}+B_2b=\psi(b)=B \quad . \quad . \quad . \quad (16)$$

Now every value of z satisfies (14). Hence

$$\phi(a-b)=\phi(a)-\phi(b)=Z \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Consequently, combining (15), (16) and (17), we have

$$\begin{aligned}&\phi(a)-\phi(b)-\chi(a)+\psi(b)\\ &=(Z_1-A^1)\frac{da}{dx}-(Z_1-B_1)\frac{db}{dx}\\ &+(Z_2-A_2)a-(Z_2-B_2)b\\ &=Z+A+B \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)\end{aligned}$$

But the linear differential equation (18) can be integrated in the form

$$a+X_1b=X_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where X_1 and X_2 are functions of x. By means of (19), a and b may respectively be eliminated from (15) and (16), giving results which may be represented by (20) and (21) respectively. Then (20) and (16) will have a common integral which will give an available value of b, and a like value of a can be deduced by combining (21) and (15). And, a and b being so determined, we have next to substitute the resulting a—b for z in (14) if any constant remains arbitrary. If not, the required integral is obtained as soon as a and b are known.

I would add that, in certain cases, some of our expressions may become infinite at the limits, but this circumstance will not necessarily render the results illusory or inapplicable. In dealing with the Boolian integral for quadratics by the method of conversion I ascertained, and communicated the calculations and results to Mr. Harley some time ago, that infinite values occur in the conversion but do not affect the final results.

Brisbane, Queensland, Australia,
August 5th, 1870.

Mr. Boyd Dawkins, F.R.S., gave a short account of the work done in the Victoria Cave, near Settle, since the last notice brought before the Society. The two layers containing traces of man, which were separated at the entrance by a talus of fallen stones, seven feet thick, that gradually coalesced as the excavation passed into the cave, and at last became so confused together as not to be easily distinguished at a few feet from the entrance. The remains of a gigantic bear which had been eaten, probably may be assigned to the lower horizon, which furnished flint flakes, and a bone harpoon in form resembling that used by the natives of Nootka Sound; the upper or Romano-Celtic stratum, continued to supply evidence of the comparatively late date of its accumulation in barbarous imitations of coins of Tetricus (A.D. 267-273.) A portion of the ivory handle of a Roman sword and a coin of Trajan have also been found, along with large quantities of the bones of animals that had been used as food. Several spurs of cocks proved that the inhabitants ate the domestic fowl, which was probably imported into this country either directly or indirectly by the Romans. The most striking object however is a beautiful sigmoid fibula made of bronze, and ornamented with a beautiful pattern in red, yellow, green, and blue enamel. It is an admirable example of the art of enamelling ("Britannicum opus"?) which the Celtic inhabitants of Britain probably taught their Roman conquerors.

Ordinary Meeting, October 18th, 1870.

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

Professor Balfour Stewart, F.R.S., exhibited a series of sun-spot curves projected from results obtained by himself and Mr. De la Rue, from observations of Sehwabe, Carrington, and the Kew series of photographs of the sun. These extend over a term of about 40 years, and exhibit a principal and seeondary maximum and minimum in each solar spot period of 11 years, thus corresponding with the light eurves of R Sagittæ observed by Mr. Baxendell, and β Lyræ by Prof. Argelander. Henee it may possibly be that notwithstanding the darkening of the sun's surface during the maximum spot period, the total light and heat emitted by the sun at this period is really greater than at the times of minimum spot frequency.

Mr. Lockyer, F.R.S., gave an aeeount of his reeent speetroseopie investigations of the solar atmosphere, and pointed out that the eonclusions arrived at by De la Rue, Stewart, and Loewy eonfirmed the views to whieh he himself had been led by speetroseopie observations of the sun during the last two or three years. These tended to show that the absorbing atmosphere, termed the ehromosphere, which he had proved to exist round the sun's body, had gradually diminished in thiekness sinee the last solar spot minimum in 1867.

Mr. Boyd Dawkins, F.R.S., gave a short account of the examination of Offa's Dyke made in the autumn by Col. Lane Fox and himself. The portion examined extended from Cherbury in the south to the abrupt range of limestone hills to the north of Llanamyneeh. At Nanteribba Hall, near Forden, the dyke passes nearly due north between the road to Montgomery and the abrupt boss of volcanic trap which looks at a short distance like a ruined castle, and which has been encircled by a very broad and deep moat. There can be no doubt but that this was a point of observation, and as it is but some 20 yards on the English side of the dyke, it was most probably one of the positions permanently occupied by the English followers of the Mercian king. From this point the dyke gradually swerves to the east from the road between Montgomery and Buttington and makes directly over the low slopes of the hills, in some places being nearly ploughed down, and in others, and especially in the small valleys, being of considerable height and resembling a railway embankment, until it reaches the higher ground of Fron. Thence it runs through Pentre and gradually approaches the road, and finally dies away in the alluvium of the Severn, nearly a quarter of a mile to the south of Buttington Church. The commanding camp to the south of this portion of the line is Caer Digol, or the Beacon Ring, on the top of the Long Mountain. The morass, which in Offa's time must have extended between the Main Ditch and the Severn, prevented the necessity of any bank being made between Buttington and the Cefn. Where, however, the open country demands a defence to the north of Cefn, an embankment makes straight for the greenstone ridge of the Garreg, and is very plainly seen close to the farm-house of that name, near the Trewern Gate. Here we lost our clue, and it is very likely that the steep ridges of Moel y Golfa, and the marvellously strong camps of the Breiddan and Middleton Hills, formed a sufficiently strong barrier without any dyke being raised. We picked it up again, however, on the western or Welsh side of the Severn, from which it runs, as shown in the ordnance map, due north to the Four Crosses, where it joins the Oswestry road, and where it is cut across by the new railway. Thence it makes straight for the fortified hill of Llanamynech, its line coinciding with the high road. On reaching the summit of Llanamynech it takes the western or Welsh side of the two large camps, and passes down into the valley to the south of Whitehaven, which was the limit of our expedition. The results of our examination are the direct proof that the dyke was made for military purposes and that it took the line which was best adapted for repelling the incursions of the Welsh. Throughout the district which was examined the embankment faces Wales, and was therefore made to defend the country within it from the Welsh. Dr. Wright's view, therefore, that it was a mere geographical boundary to prevent the Welsh from stealing the cattle of the Mercians cannot be maintained, although it may perhaps receive some confirmation from the nursery-legend of Taffy. The camps in the neighbourhood of the dyke are probably older than Offa's time. The bronze spears found in Llanamynech imply that the camp is not later than the Bronze Age, while the Roman coins in that of the Breiddan point to its occupation by the Romans.

"On the Action of Sulphuric Acid on Diallyl," by WILLIAM ROBERT JEKYLL, Dalton Chemical Scholar in Owens College. Communicated by Professor ROSCOE, F.R.S.

Diallyl was first prepared by Berthelot and Luca in 1856. They found that it dissolves in concentrated sulphuric acid with the evolution of much heat, and that after some hours an oil separates, which appears to be modified hydrocarbon.

In 1866 Schorlemmer published a paper on a new series of hydrocarbons derived from coal tar, having the formula $(C_nH_{2n-2})2$ (Proc. Roy. Soc. xv. 132). He there says, "As these hydrocarbons were obtained by the action of sulphuric acid on coal tar oils boiling below 120° and as they differ by C_2H_4, it appears to me almost certain that they are polymers of the hydrocarbons of the acetylene series C_nH_{2n-2} formed in the same way as diamylene is formed by treating amylene with sulphuric acid. In order to test this theory I have made some experiments with the two isomers C_6H_{10}, viz., diallyl and hexoylene. By acting with sulphuric acid on these compounds, I obtained, besides large quantities of tarry matter, polymeric modifications boiling above 200°, having a smell similar to the hydrocarbons described above, giving also similar nitro-compounds; but the quantities which I got were not large enough for a more exact examination." With a view to throwing light upon this point, at the request of Mr. Schorlemmer I undertook to investigate the action of sulphuric acid on diallyl. The diallyl used was obtained by the action of sodium upon allyl iodide and boiled at 59°. Since concentrated sulphuric acid acts with great violence upon diallyl, the latter was diluted with about an equal bulk of pure paraffins boiling at from 55° to 60°. To this mixture sulphuric acid was gradually added in small quantities, the bottle being frequently shaken. At the end of the reaction the contents of the bottle were found to be arranged in two layers, of which the upper one consisted of unaltered paraffins, and the whole of the diallyl having been taken up by the acid. The heavier and acid portion was diluted with water, when a dark coloured oil lighter than water separated out, and the whole was distilled from a large flask. The distillate consisted of a light oil, which came over below 100°, mixed with a little water. After a second solution in sulphuric acid and a repetition of the foregoing processes, in order to remove all traces of the paraffins, the oil was dried over calcium chloride and heated for some hours over potassium. The oil was thus obtained pure and boiled constantly at 93°. Analysis showed that its composition is expressed by the formula $C_6H_{12}O$.

This substance is readily soluble in concentrated sulphuric and fuming hydriodic acids, and slightly so in water. It is unacted upon by either caustic potash or potassium, and possesses a strong ethereal odour like that of peppermint. In presence of potassium bichromate and sulphuric acid, it yields a blue colour, similar to that produced by perchromic acid and common ether.

This compound has already been obtained by Wurtz (Ann. Chim. Phys. (4) III. 174), by treating di-iodhydrate of diallyl with silver oxide. He calls it diallyl monohydrate, but says further on that this body comports itself as an oxide or anhydride (ether), corresponding to dihydrate of diallyl $\left.\begin{matrix} C_6H_{12} \\ H_2 \end{matrix}\right\} O_2$ standing to the latter in the same relation as hexylene oxide to hexylene glycol, and might be called therefore *hexylene-pseudoxide*. As I have shown that the body is not acted upon by potassium, this view of Wurtz's is correct—it cannot be a hydrate, and I therefore propose to adopt Wurtz's second name, and to call it pseudoxide of hexylene.

To throw some light on its constitution it was oxidized by heating it in sealed tubes with a solution of potassium bichromate and sulphuric acid. On opening the tubes carbonic acid was evolved. Their contents were distilled, and the distillate neutralized with sodium carbonate. The neutral sodium salt was heated in a retort with sulphuric acid, by which means a distillate was obtained, which furnished a silver salt. The following analysis shows the salt to be silver acetate.

Found.	Calculated for silver acetate.
64·41 °/ₒ silver.	64·67 °/ₒ silver.

The mother liquor likewise furnished silver acetate.

Repeated experiments showed that nascent hydrogen evolved from sodium amalgam is without action upon hexylene pseudoxide.

Hydriodic acid acts upon the pseudoxide even in the cold. A few grams of the substance were heated at 100° with an excess of fuming hydriodic acid in sealed tubes for about four hours. A red heavy liquid formed at the bottom of the tubes, the contents of which were distilled from a retort in presence of a little phosphorus. The iodide in the distillate was separated from the water, dried over calcium chloride, and distilled under a partial vacuum. On distillation, much decomposition ensued, with the formation of hydriodic acid, a little free iodine, and with the separation of tarry matter. After a second distillation in vacuo, and drying over caustic potash, a liquid was obtained, boiling under the ordinary pressure of the atmosphere, at 165° to 167°, which is the boiling point of the β hexylic iodide of Wanklyn, (Chem. Soc. Journal, 21.)

Several iodine determinations, made by means of an alcoholic solution of nitrate of silver, further shows the substance to be hexyl iodide.

Found.		Calculated for $C_6H_{13}I$.
(1)	(2)	
59·43	59·68 °/ₒ iodine.	59·90 °/ₒ iodine.

In order to convert hexyl iodide derived from hexylene pseudoxide into hexyl hydride Schorlemmer's method was employed. The oil obtained by this means contained but little olefines, and after purification boiled constantly at 68° to 69°. The following results of analysis show that it consisted of hexyl hydride.

	Found		Calculated for
	(*a*)	(*b*)	C_6H_{14}
C	83·49	83·35	83·72
H	16·30	16·42	16·28
	99·79	99·77	100·00

A portion of this hexyl iodide was oxidised by heating it in a flask attached to an upward condenser with a solution of bichromate of potash and sulphuric acid. During the operation much carbonic acid was liberated. The condensed acid was rendered neutral by sodium carbonate. From the sodium salt thus formed the acid was fractionated from a retort by adding successively five drops of sulphuric acid. From the first four distillates silver salts were obtained which furnished the following results on analysis.

	Found.	Calculated for silver acetate.
(1)	59·36 per cent. silver.	64·67 per cent. silver.
(2)	66·63 " "	" " "
(3)	64·13 " "	" " "
(4)	64·66 " "	" " "

The non-agreement of No. 1 with the calculated results was owing to the fact that the salt was very impure and uncrystalline, nor could I succeed in purifying it by recrystallization. From distillates No. 2 and 4 similar results were obtained from salts, which crystallized from the mother liquors. A second series of experiments, in which a weaker oxidizing solution was employed, also yielded carbonic and acetic acids as the products of oxidation. It is of interest that the hexyl iodide, which was obtained from hexylene pseudoxide, and the boiling point of which resembled that of Wanklyn's β hexylic iodide, yielded carbonic and acetic acids as oxidation products, while the β iodide yields on the other hand butyric acid in addition.

The diluted sulphuric acid, which had been used for acting upon diallyl, was neutralized with barium carbonate, filtered and evaporated to dryness, but no organic sulpho-acid had been formed.

Polymers of Diallyl. After distilling off the hexylene pseudoxide from the diluted acid, a layer of hydrocarbons remained on the top of the liquid, from which they were separated by means of a stop-funnel. The hydrocarbons were dried over calcium chloride, and found to boil at between 200° and 300°. After several distillations over metallic sodium, they were separated into three portions, boiling at from 205°—215°, 240°—245°, 275°—285°. These hydrocarbons invariably left a slight residue on distillation. Analysis showed that they have an empirical formula of C_6H_{10}.

(1) Boiling point 205°—215°.

	Found (a)	Found (b)	Calculated for C_6H_{10}
C	87·31	87·38	87·8
H.........	12·52	12·31	12·2
	99·53	99·69	100·0

(2) Boiling point 240°—245°.

	Found (a)	Found (b)	Calculated for C_6H_{10}
C	87·30	87·30	87·8
H.........	12·42	12·31	12·2
	99·72	99·61	100·0

(3) Boiling point 275°—285°.

	Found	Calculated for C_6H_{10}
C	86·96	87·8
H.........	12·81	12·2
	99·77	100·0

No. 3 attacked sodium slightly, although it had been distilled over it several times, therefore it is probable that its non-agreement with the calculated result was owing to admixture with an oxygen compound. From the above analysis and boiling points, it is probable that at least two polymers of diallyl are formed by the action of sulphuric acid upon it. I had not however a sufficient quantity of the hydrocarbons to obtain satisfactory vapour density determinations, which would at once have settled the point. It is nevertheless probable that No. 1 consists of two molecules of diallyl condensed into one, and that it has the formula $C_{12}H_{20}$; for Schorlemmer, by the action of sulphuric acid on hydrocarbons boiling below 120° from cannel oil, obtained one which boiled at 210°, and the vapour density of which showed that its formula was $C_{12}H_{20}$.

In conclusion, I have much pleasure in tendering my thanks to Dr. Roscoe and Mr. Schorlemmer, for their kindness and attention to me throughout the whole course of this research.

Ordinary Meeting, November 1st, 1870.

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

Mr. William H. Johnson, B.Sc.; Mr. Walter Morris, and Professor Balfour Stewart, LL.D., F.R.S., were elected Ordinary Members of the Society.

Dr. JOULE, F.R.S., exhibited a series of curves obtained by Dr. Stewart, F.R.S., from the self-recording instruments at the Kew Observatory, showing a large amount of disturbance of the magnetic declination and horizontal force during the progress of the Aurora of the 25th October. He also showed a curve of the changes which took place in the magnetic dip as observed by himself at Broughton. The most remarkable variation occurred during the interval from 6h. 15m. to 6h. 23m. G.M.T., when the dip increased from 69° 8′ to 70° 30′.

"On the Aurora Borealis of October 25th, 1870," by T. T. WILKINSON, F.R.A.S.

On the afternoon of Tuesday the 25th instant the wind blew pretty strongly from about W.S.W. at Burnley. The barometer suddenly rose from 28·5 to 29·1 at my residence, which is situated about 692 feet above the level of the sea. The atmosphere was cloudy most of the day, but soon after noon the clouds assumed a very decided *cumulo-stratus* form, and the crests of the huge masses were deeply tinged with red. About three o'clock the western portion of the sky became mostly free from clouds, with the exception of what appeared to be a dense mass of dark brown vapour in the low horizon. Immediately above this the sky was of a deep sea-green colour, which gradually faded into a whitish blue at about 30 degrees above the horizon.

Occasionally this portion of the sky became of a deeper green, and then the crests of the clouds in the S. and E. acquired a deeper rosy red tint; and hence it may be inferred that all the electrical conditions for an auroral display were present for most of the afternoon, but the light of the sun was too powerful for it to become visible. Between four and five o'clock there were several vivid flashes of lightning, accompanied by heavy thunder and occasional showers of hail. By six o'clock the thunder clouds had mostly cleared away, and the auroral display became most magnificent.

In the zenith there appeared to be a splendid corona of large diameter. The centre at times became intensely black, whilst its edges seemed to be draped with festoons of bright rose-coloured rays. At times these seemed to become wreaths of vapour, which soon shot out on all sides in red streaks stretching towards the horizon. The intervening spaces were frequently filled with stripes of bright green; the edges were tinged with red, shading off into yellow and gray, like the gores of a variegated balloon. Occasionally the whole visible atmosphere assumed this variegated appearance, which anon changed into masses of bright rose-coloured vapour. As the coronæ disappeared the aurora assumed the form of a bright semicircular bank of white cloud, from which the usual pointed rays shot up towards the zenith from S.W. to N.E. About seven o'clock rain began to fall, and the sky was mostly overcast for several hours. At ten o'clock the atmosphere was again clear, and the auroral display was at times nearly as magnificent as before. Coronæ were formed every few minutes, and on their dispersion the streamers shot up on all sides from the semicircular bank in the north. By eleven o'clock the atmosphere was again clouded; rain began to fall, and no further observations could be made. Certainly nothing approaching to this magnificent display has occurred here for very many years.

Mr. BAXENDELL stated that his observations of the Aurora of the 25th ult. were directed principally to the determination of the position of the centre of the corona, or point to which the beams of the Aurora appeared to converge, by reference to the stars in its immediate neighbourhood. The mean of his results gave at 6h. 35m. G.M.T. azimuth, S. 19° 52′ E; altitude, 66° 9′. From this position it would appear that the direction of the lines of magnetic force in the region of the auroral beams deviated sensibly from parallelism with the line of dip at the surface of the earth.

Mr. BOYD DAWKINS, F.R.S., exhibited a number of casts in plaster of Paris of various objects of natural history, and explained the process by which any one can make them for himself. The material of the mould is artists' modelling wax, which is a composition akin to that which is used by dentists. And as it becomes soft and plastic by the application of heat, though in a cold state it is perfectly rigid, it may be applied to the most delicate object without injury. As it takes the most minute markings and striations of the original to which it is applied, the microscopic structure of the surface of the original is faithfully reproduced in the cast. The method is briefly this. 1. Cover the object to be cast with a thin powder of steatite or French chalk, which prevents the adhesion of the wax. 2. After the wax has become soft either from immersion in warm water or from exposure to the direct heat of the fire, apply it to the original, being careful to press it into the little cavities. Then carefully cut off the edges of the wax all round, if the under cutting of the object necessitates the mould being in two or more pieces, and let the wax cool with the object in it, until it be sufficiently hard to bear the repetition of the operation on the uncovered portion of the object. The steatite prevents the one piece of the mould sticking to the other. The original ought to be taken out of the mould before the latter becomes perfectly cold and rigid, as in that case it is very difficult to extract. 3. Then pour in plaster of Paris after after having wetted the moulds to prevent bubbles of air lurking in the small interstices, and if the mould be in two pieces, it is generally convenient to fill them with plaster separately before putting them together, 4. Then dry the plaster casts either wholly or partially. 5. Paint the casts in water colours, which *must* be fainter than those of the original, because the next process adds to their intensity. The delicate shades of colour in the original will be marked in the cast by the different quantity of the same colour which is taken up by the different textures of the cast. 6. After drying the cast steep it in hard paraffin. The ordinary paraffin candles, which can be obtained from any grocer, will serve the purpose. 7. Cool, and polish the cast by hand with steatite. The result of this process is far better than that obtained by any other. The whole operation is very simple, and promises to afford a means of comparison of natural history specimens in different countries, which has long been felt to be a scientific need. Casts of type specimens may be multiplied to any extent at a small cost of time and money, and are as good as the original for purposes of comparison, and almost as hard as any fossil. Mr. Boyd Dawkins has employed it for copying flint implements, fossils, and bones and teeth, which can scarcely be distinguished from the originals.

Mr. BOYD DAWKINS then explained the extraordinary hoax which had been practised on the *Times* by a sweep of St. Asaph. The paragraph to which he referred gave a most vivid picture of the capture of a huge reptile by a "Mr. Hughes," in the Cefn Caves, which were recently visited by the British Association. The reptile in question died a natural death in a menagerie at St. Asaph, and passed into the hands of Hughes, a sweep of that place, by purchase, and not as the meed of valour. And he exhibited it to the visitors at Rhyl as having been killed in the caves of Cefn, after advertising himself in the *Times*, and thereby exciting a great deal of lucrative curiosity. The whole story as related in the *Times* is a mendacious and impudent hoax, which has been copied into many of the local papers and widely distributed. Its insertion in an organ of public opinion like the *Times* implies an amount of ignorance of natural history which is not creditable to English civilisation in the nineteenth century.

"Notes on Glacier Moraines in Cumberland and Westmorland," by William Brockbank, F.G.S.

The author referred to the proceedings of the Geological Society of London for 1840–1, which contain notices of the evidences of glaciers having existed in Great Britain, by Professor Agassiz, Dr. Buckland, and others, and which point out (1) "Moraine-like Masses of Drift," which occur near the junction of the Eamont and Lowther with the Eden, near Penrith; (2) The "large and lofty insulated piles of gravel in the valley of the Kent near Kendal, and the smaller moraines and their detritus, which nearly fill the valley from thence to Morecambe Bay"; (3) "Similar mounds near Shap," and (4) the "Gravel mounds near Milnthorpe and thence to Lancaster."

Of these the author considered the Kentmere Group, near Kendal, as most nearly fulfilling the conditions required in true glacier moraines, and that in the other cases it admitted of doubt whether they were really due entirely to glacial action.

The districts more particularly the subjects of the author's notes are (1) The valleys of Eskdale and the Duddon (which were not visited by Dr. Buckland, but in which he supposed moraines to exist, from the appearances of the valleys as delineated in Fryer's map of Cumberland); (2) The valleys eastwards from Bowfell, and (3) The district of Shap Fells.

The highest mountain in the Lake District is Scawfell Pike, 3,210 feet, and separated from it only by a narrow valley is Bowfell, 2,960 feet. These two noble hills form the central nucleus, from which radiate the valleys of Wastdale, Borrowdale, Langdale, Eskdale, and the vale of Duddon, and in this district the author found the evidences of glacial action in a very marked degree. The conformation of Bowfell is exactly suited for a great gathering ground for snow, which would accumulate on its summits, and flow over its huge shoulders as glaciers, into the vales below. Its three summits are piled up masses of unworn rocks, whilst its flanks are everywhere scored and polished by glacial action; the porphyry and greenstone of which they are composed retaining the markings very clearly, so that in many places you are able almost to trace the course of the glaciers.

The finest series of moraines occur at the head of the great Langdale valley, at the point where the paths diverge by Rossett Gill to Wastdale, and over the Stake Pass to Borrowdale. The valleys formed by five brooks, off the shoulders of the Langdale Pikes and Bowfell here converge, and the glaciers at this point would be abundantly fed with ice from the lofty mountains, and a wide area of gathering ground.

The moraines here stretch across the valley in a very perfect series of rounded knolls of huge boulders and debris, rising some 40 or 50 feet above the stream, and forming at least three irregular lines; as if the glacier had gradually receded up the valley, at distant intervals of time. The boulders are of the porphyries and greenstones of the surrounding mountains, intermixed with clay soil, deeply tinged with red Hæmatite iron ore, which occurs abundantly in a vein at the summit of Bowfell, and in Rossett Gill. There are many "perched blocks" on each side the valley above the moraines, and which may possibly mark out the track of the glaciers; and the rock surfaces, especially in Rossett Gill, are much scored and polished. Altogether the Langdale moraines afford an almost perfect example of glacier debris, and they are, in all probability, the remains of the last glaciers which existed in England, and after the valleys around Bowfell had assumed almost their present forms. Doubtless at some earlier period, the Langdale glaciers extended far down the valley, even to Windermere Lake, as the mammillated rocks, perched blocks, and groovings, which can be clearly seen at many prominent points, abundantly prove; and they appear to have gradually receded, probably as our climate became warmer; until at length they had dwindled down to the comparatively small size indicated by the moraines now existing in almost perfect condition, at the heads of the valleys, immediately under Bowfell.

Proceeding through Rossett Gill on the path to Wastdale we find in Angle Tarn an interesting example of glacial action; the basin evidently having been scooped out by a glacier, which descended from the top of Bowfell, poured over the precipitous rocks which now overlook the Tarn, and deposited the debris of moraines which now forms the embankment across the valley at its foot. A little further on is a similar example, at the head of Long Strathdale, where another Tarn has evidently existed, pent in by a glacier moraine. The waters have here broken through the embankment and escaped, leaving a swampy marsh behind. The rocks which are thus left bare by the departed waters, cover a considerable area, and are much grooved and polished by ice.

There are similar moraines at the Borrowdale side of the Stake Pass, as also at the foot of the Wrynose Gap at the head of Little Langdale; and doubtless they will be found in all the valleys to the eastwards of Bowfell and Scawfell.

Westwards from Bowfell are the vales of the Esk and Duddon. Eskdale proceeds directly to the sea at Ravenglass, having in its short course of 12 miles a fall of nearly 3000 feet, and that chiefly in the first 6 miles of the valley.

The estuary at Ravenglass has a very remarkable appearance, from the numbers of large boulders of granite, greenstone, porphyry, and clay slate, which lie scattered along the beach; resembling, on a small scale, the shores now frequented by drift ice in the harbours of Newfoundland. A nearer examination at once introduces a Lancashire geologist to the family of boulders, occurring so plentifully in our drift clays, and which, in all probability, have their origin in the Eskdale valley. Proceeding up Eskdale the granite district is soon reached. Standing on the bridge above Muncaster Castle, about two miles from the sea, you are in the centre of an amphitheatre of granite mountains, comprising Muncaster Fells, Harter Fells, and Birker Moor Fells. The valley at this point, and for several miles inland, forms a wide and almost level 'strath,' being filled up with diluvium, and it bears every appearance of having been an arm of the sea; which would in such case have washed the bases of the granite hills from which the boulders came. This is that part of Eskdale in which Dr. Buckland, judging by Fryer's map, supposed moraines to be in existence, but it is quite evident that in the present aspect of the valley, none are to be found.

The panorama of mountains which form the head of Eskdale is by far the finest in the Lake District; comprising as it does the whole of the Scawfell, Bowfell, and Coniston range. The flanks of Bowfell on this side are glaciated in a remarkable manner; and in the whole of the upper part of the valley there are evidences of the action of ice at almost every turn. Seen from above the whole valley has an ice-worn "hummocky" aspect.

The author did not find any moraines in the upper portions of either the Esk or Duddon Valleys; and in this respect there is a marked difference between the east and west sides of Bowfell. This is probably to be accounted for by the fact that to the eastwards the vales of Langdale and Borrowdale lie high, and the ice would soon be checked in its flow; so that we now find the terminal moraines at the heads of the valleys. To the westward the valleys have a continuously rapid fall for five or six miles, throughout which course they have a "hummocky" aspect, and below this they are comparatively wide and levelled; so that in all probability the glaciers which formerly existed had their terminations in arms of the sea.

This is exactly the sort of glacial condition which would best explain the requirements of a drift theory, by which the travelled boulders found in Lancashire shall have been carried thither by ice;—and a careful study of the Eskdale valley, after first ascertaining the existence of undoubted glacial evidences at its head, confirms the writer in the opinion that it is from this district, and by glacial agency, that they were transported. It is only needful to suppose a state of things to have existed in England analagous to that which now obtains in similar latitudes, on the ice-bound coasts of Labrador and Newfoundland, where floe-ice prevails for many months of the year over an area of from 200 to 300 miles in width, whilst the land is covered with glaciers during the same period. The glaciers there carry the boulders down to the shore during the summer, and they are picked up by the floe-ice the following winter, and borne away at the breaking up of the ice, as warm weather returns, and floated seawards, as the winds and waves may direct. To complete the picture, we have to realise the fact, that the Bowfell range of mountains was at this period an insulated group, washed by a frozen sea, and covered with perpetual snow; and much of Lancashire and the Midland Counties submerged. There is every reason to believe that such was the condition of our country during the boulder drift period.

The Duddon Valley, in its upper portion, which centres in Bowfell, is, in its glacial aspect, similar in every way to Eskdale. Below Seathwaite it would however receive a very important affluent from the Coniston range of mountains, which would there unite its glacier with that of the Duddon Valley;—and at this point we find, as might be expected, very fine examples of glacial action. The rocks are mammillated in large groups. Perched blocks, high up the hill sides, testify to the great thickness of the glacier, and are very conspicuous in the landscape. They frequently occur in groups, and seem to lie as if in one main current, forming in places fine *lateral moraines.* The estuary of the Duddon below Broughton-in-Furness opens out into a wide bay, and supposing, as in the Eskdale valley, that the sea formerly reached much further inland, and was subject to the action of floe or drift ice, it would then receive the moranic debris, as before described. The granite district of Harter Fell would furnish its contribution of boulders, to be mixed with the porphyries and greenstones of Bowfell and the slates of Coniston in the terminal moraines.

Dr. Buckland held the opinion that the granite boulders of the Shap district had been carried southwards by glaciers, and deposited by their agency over the midland counties; but the writer was not able to find the same evidences of glacial action in the Shap fells as those which exist in West Cumberland. The granite district of Shap is limited to an area of some 800 to 1,000 acres; comprising Wasdale Pike, about 2 miles above the Shap Wells House. Wasdale Pike is not an isolated peak, but is an outlier of the mountain range at the head of Troutbeck, of which Tarn Crag forms the central summit. The valley below the pike and thence over the whole of Shap Fells has a most remarkable aspect, from the large numbers of immense rounded masses of granite which are everywhere scattered. These widely dispersed boulders cannot altogether be accounted for by the agency of glaciers. The summit of Wasdale Pike is only 1,853 feet above the sea, and the valley at its base some 500 feet lower, and the junction of Wasdale Pike with the Lune at Tebay is but 700 feet above the sea; so that it scarcely appears probable that any glacier could be continuous to such a point as would admit of its transporting its moraines to the sea, where they could be carried away and dispersed by drift ice.

Another explanation must be sought, and it will probably be found in the very fact of the occurrence of this solitary peak of granite. The whole district abounds in intrusive veins of whinstone, and other plutonic rocks, crowned by the granite peak of Wasdale Pike, which has evidently been forced up through the slates by some volcanic force, and this might produce the tremendous effects which scattered these blocks far and wide.

The denudations here have been on a tremendous scale, and possibly carried the granite boulders far down the Lune valley, and formed the moraine-like mounds referred to by Dr. Buckland, near Milnthorpe and Lancaster, at which points possibly drift ice became the modifying and transporting agent.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 10th, 1870.

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

Mr. JOSEPH SIDEBOTHAM read the following paper—"On the Variations of Abraxas Grossulariata."

The variations in animals and plants are of great interest, and each contribution to the store of facts accumulated relative to these variations, their causes and limits, is of value in determining the identity, and limits of species, in whatever way we interpret the word *species.* Abraxas grossulariata is probably one of the most variable insects we possess in this country, in colour and markings, and it would be quite pardonable in any one not well acquainted with it, were he to split it up into four or five species; but although it varies in colour and markings in such a great degree, all these varieties are joined together by gradual steps, and yet no step is found to join it to the next species on our list, Abraxas ulmata.

The larvæ of this species will feed upon the leaves of most trees and shrubs, and is therefore easily experimented upon, as to whether the changes in food influence the colour or markings. So far as my own experiments, and I believe those of others are concerned, no difference whatever can be detected from the varieties of food, except in size. That long-continued changes of food through many generations might have a perceptible effect is however more than probable.

The type form of this moth is too well known to require description. I will therefore exhibit a drawer of specimens, having the type form in the centre, the various forms radiating from it in steps, in one line ending in white, another in black, another in which the white ground runs gradually into brown, and various other marked varieties.

We may divide these into the following seven groups:—

1st variation. White, or the spots very few and distant: this leads up to the type form.

2nd. Spots joined together, forming curves and lines.

3rd. A variety of intermediate spots and patches.

4th. The spots at the border becoming lines, and running towards the base of wings.

5th. Spots confluent, forming solid black patches over nearly the whole of wings.

6th. The spots having the type form, but the white ground tinged with a smoky brown or drab colour, sometimes suffusing the whole of the wings.

7th. Spots of the type form, but the ground of wings bright yellow.

From various experiments with many thousands of larvæ of this species, I have come to the conclusion that these variations are in a great measure hereditary, that one brood of eggs will produce moths of forms in a great measure identical, if the parents be of the ordinary type; if the eggs be the produce of moths of extreme colouring, varying much from the type, then, although the bulk of moths will be marked dark or light, as the parents, there will be others of the ordinary type, and also some of the very opposite character of marking, precisely as in many florists' flowers, the seed from those varying most from the original form are known to produce the most marked and opposite varieties.

These experiments can only produce approximate results, unless a great number of years could be devoted to them, and in this and many others of our most variable species, it is almost impossible to rear them in confinement beyond the second generation.

PHYSICAL AND MATHEMATICAL SECTION.

October 11th, 1870.

Joseph Baxendell, F.R.A.S., in the Chair.

Mr. G. V. Vernon, F.R.A.S., stated that being at Keswick on August 15th, 1870, he observed, at about 8.45 p.m., a shooting star fall from near the zenith, and apparently explode at an elevation of about 30° above the horizon, leaving a very peculiar appearance behind it.

After the explosion there appeared an elliptic ring of illuminated vapour whose axis was nearly parallel to the horizon, having a bright appearance at its western extremity almost resembling the nucleus of a comet. The ring of vapour became gradually fainter, but was plainly visible for 20 minutes. The major axis of the ring which was parallel to the horizon was about twice the length of the minor axis.

The night was beautifully fine, and many stars were visible, but the reflection of light in the west following sunset made the brightness of the ring not so great as it otherwise no doubt would have been.

Any one who had not seen the fall of the meteor would certainly have taken the subsequent appearance for a comet.

Mr. Dickinson communicated the following "Observation of the Occultation of Saturn, September 30, 1870," by Mr. James Fellows.

The opportunity for the observance of this phenomenon was unfavourable. The moon and planet being very low, and the latter much obscured by the dense atmosphere. The disappearance was not seen by me, but having arranged my telescope—one of 4 feet focal length, and $2\frac{7}{8}$ aperture, and using a power of 50, I observed the reappearances as under noted, viz.:—

Reappearance............ 7h. 12m. 45s. G.M.T.
Last contact .... 7h. 13m. 20s. „

My watch having been carefully checked by the Town Hall clock at about 5 p.m., no appreciable error could exist. Place of observation, Ashton-upon-Mersey.

Ordinary Meeting, November 15th, 1870.

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

John Durham Bird, M.D.; Mr. John A. Bennion; Henry Deacon, F.C.S.; Mr. Joseph Carter Bell, and Thomas Steadman Aldis, M.A., were elected Ordinary Members of the Society.

Mr. W. B. Johnson, C.E., brought before the notice of the meeting the extraordinary advance that had been made within the last 20 years in the capabilities of machines for cutting and paring heavy articles of machinery, and said this was particularly noticeable in the treatment of heavy forgings. At no very remote date it was the universal practice to pare down heavy forgings to something near the finished dimensions in the smithy by hand labour only; this mode of procedure was not only expensive, but rude and imperfect in its results. The introduction of tools to supersede the use of the hand chisel and file in the workshop has been developed to a remarkable degree. Machines are now made of such enormous strength, and the cutting tools so carefully devised, that the old system of paring down in the smithy has been set aside; this competition between the tools of the workshop and the hand work of the smithy has resulted in establishing the system of tool paring in preference to smith-work to an almost universal extent. Twenty years ago there might be seen in engineering establishments a large smiths' fire, in which was placed a part of some heavy forging, and when the part under operation was heated to a blood-red heat the superfluous parts were cut off by means of a set, upon which the successive blows of four and sometimes six strikers were delivered with surprising precision and regularity, and by these repeated heatings and dressings the forging would be eventually brought down to its finished dimensions, except by so much as was necessary for the final finishing or getting up. These operations in the smithy, as before observed, have now ceased, the forgings are taken at once and placed in a machine, which by heavy and continuous cutting soon pares down the forgings, and then finishes them without changing them to another machine or process. Tool paring is not only economical of labour, but the result as to accuracy is more satisfactory. Mr. Johnson then showed to the meeting some specimens of steel and iron parings sent to him by Messrs. Smith and Coventry, machinists, Salford, and further remarked that these parings demonstrated very clearly the capabilities of the machines and cutting tools of the present day. One specimen from a Bessemer steel shaft, the result of taking a cut $\frac{5}{8}$ths of an inch deep by $\frac{3}{8}$ths of an inch traverse, was particularly interesting on account of the form and size in which they, the parings, left the cutting tools. The cutting tools used in obtaining the specimens exhibited to the meeting were of a peculiar construction, and possessed some marked advantages over those in ordinary use.

The PRESIDENT said that Mr. Brockbank, in a communication made to the Society at its last meeting, stated "that the estuary at Ravenglass has a very remarkable appearance, from the numbers of large boulders of granite, green slate, porphyry, and clay slate which lie scattered along the beach." Had he (the President) been present, he should have called the attention of the members to two singular accumulations of boulder stones on the sea beach below Seascales station and west of Drigg station. At the first-named place the upper permian sandstone of St. Bees bounds the sea there at high-water mark, and is covered with blown sand in which are mingled some large boulders. Along the shore, about 400 yards apart, are two singular banks of stones running down to low-water mark parallel to each other in a westerly direction for above 200 yards. The sands between and outside these banks are quite free from blocks of stone except some common shingle. At the last-named place, on the Drigg beach, is seen another accumulation of boulders known by the name of Barnscar. It is nearly a mile long, so far as it is exposed seaward, and may be longer, running from north-east to south-west, and about 300 yards in breadth. The blocks are for the most part rounded and consist of green slates, porphyries, greenstones, and granites, ranging in size from a few feet to 33 feet in circumference and reaching up to 8 and 10 feet high. There were three stones lying in a straight line considerably larger than the rest. The first or most southerly was a greenstone 7ft. 6in. in height and 33ft. in circumference, of an irregular oval shape. The second was also a greenstone about the the same size as the last, but more square in form. The third was a Wastdale granite not so square in shape as the last, and measured 7ft. high and 33ft. in circumference. In measuring the heights only that portion of the stones exposed was taken; a considerable part may have been covered up. At both Seascales and Drigg no cliffs of till are at present seen on the beach there composed of drift sea sand, but these banks of stones appeared to him to indicate the former existence of a deposit of till from which the smaller stones and clay had been removed by the action of the sea in a similar manner to that which had taken place between the Pennystone and the present cliff to the north of Blackpool, alluded to in his drift paper, page 130 in vol. x, second series, of the Society's Memoirs. Both deposits probably owe their origin to the action of ice, and are the remains of lateral and terminal moraines. His observations were made nine years since, and the banks may have altered somewhat in that time; but as, to his knowledge, they have never been noticed by any writer in treating of the Cumberland drifts, he thought it worth while to allude to them.

"On the Temperature Equilibrium of an Enclosure containing a Body in Visible Motion," by BALFOUR STEWART, LL.D., F.R.S.

It has been established that in an enclosure containing bodies which are all at the same temperature, and at rest, the same amount of heat enters any surface forming part of the walls of the enclosure as leaves it in the same time, so that the body, of which this is the surface, neither gains nor loses heat. It is also known that if we take not the outer surface of such a body, but any plane passing through its substance; say for instance one parallel to its outer surface, then, as much heat passes across this plane going into the body, as passes across it going out of the body in the opposite direction; and further, this equilibrium of heat is known to hold seperately for every one of the individual rays of which the whole heterogeneous radiation is composed.

The effect of the motion of a body in altering the wavelength of the radiated light is also well known. In consequence of this, if a cosmical mass, such as a star or nebula should be formed of incandescent hydrogen, and be at the same time rapidly approaching the earth, the light which strikes the earth will not be the double line D, but a line more refrangible than it, and therefore this light will be able to pass through a mass of ignited sodium vapour at the earth's surface without suffering absorption, while, however, the light emanating from the sodium vapour will still be the double line D.

In such a case even if the star and the terrestrial sodium vapour should both be of the same temperature, yet the light radiated by the latter will not be the same in quality as that absorbed. This instance would appear to show that the equilibrium which holds in an enclosure of uniform temperature when all the substances are at rest does not hold when some of these are in visible motion, and that if in that enclosure there be a body moving towards or from the surface of the enclosure, the heat which enters the surface from the moving body will not be the same as that which the surface gives out.

Suppose for instance that the walls of the enclosure are made of glass, and that the temperature of the whole enclosure including that of the moving body is 0°C., then, were the whole at rest, the heat which strikes the glass surface will all be absorbed at a very short distance below the surface, and in like manner the heat radiated by the glass will all emanate from a short distance below the surface. But let us now suppose, to take an extreme case, that the moving body is approaching one of the glass surfaces so rapidly that the heat which it emits has been so much increased in refrangibility as to enter the boundary of the visible spectrum.

Then while the heat radiated by the glass will still continue to proceed from a very short distance beneath the surface, the heat absorbed by the glass from the moving body will be able to penetrate to a very considerable depth beneath the surface of the glass.

The outer layer of glass will thus lose, while the inner layer will gain, heat.

Now it is possible to conceive an enclosure with a fixed diaphragm, and containing a revolving body, so arranged that the heat which leaves it in the direction of a certain part of the enclosing surface, shall always be given out by that part of the revolving body, which is moving towards the surface; while on the other hand, the heat given out by the revolving body to another surface, shall be given out when the revolving body is moving from that surface.

There will thus be a want of temperature equilibrium among the various layers, those near the surface being somewhat different in temperature from those beneath. But when we have a temperature difference of this kind have we not acquired the power of converting heat into work? It would thus appear at first sight that the mere presence of a moving body, has given us the power of obtaining work from an enclosure, all of whose particles were originally at the same temperature. This appears however to be opposed to the theory of the dissipation of energy, and in consequence we are induced to think there must be some error in the assumption.

Now does not the unwarranted part of the hypothesis consist in our supposing that the revolving system can continue to revolve without losing part of its visible motion?

When two moving bodies approach or recede from each other is it not possible that each loses a small part of its visible energy while at the same time there is a surface disturbance produced in both?

It might be said that believing in a medium pervading all space we were prepared for a stoppage of motion of this nature, and that there is therefore nothing gained by the supposition which has been made; but it might be replied that by looking at the problem in the above light we appear to connect this stoppage of motion with other facts, besides being made aware of a source of surface disturbance when cosmical bodies approach or recede from each other.

Postscript added 19th November.—If we imagine a stoppage of the motion of cosmical bodies of the nature above described, then if the two approaching bodies be exactly equal and similar, either extremity of the medium between them will be similarly affected by the motion derived from the approaching bodies; but if these bodies are unequal, the two extremities of the medium will be dissimilarly affected.

MICROSCOPICAL AND NATURAL HISTORY SECTION,

November 7th, 1870.

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

Mr. BAXENDELL reported that, in accordance with a wish expressed by the Council of this Section, the Parent Society had agreed to an alteration of the rules respecting the admission of Associates to the Section; in future the subscription for Associates, with the present privileges, will be 10s. per annum, and to those paying a subscription of 20s. per annum, the right of taking books out of the Library will be accorded.

Mr. SPENCER BICKHAM, Jun., called attention to the very serious inconvenience that was experienced by all having occasion to distribute Natural History specimens, owing to the recent prohibition of the authorities to allow such parcels to be transmitted by "sample post," and it was agreed that a Memorial be forwarded to the Postmaster General.

"The Hawthorns of the Manchester Flora," by Mr CHARLES BAILEY.

Amongst the aggregate species of plants whose distribution in subordinate species is imperfectly known, stands the common Hawthorn, *Cratægus Oxyacantha* L., met with throughout Britain. In recent years (beginning with Jacquin, in 1775), this plant has been separated into several segregate species, three or more of which are known to occur in this country, viz., *C. monogyna* Jacq., *C. kyrtostyla* Fing., and *C. oxyacanthoides* Thuill. Of these three species, subspecies, or varieties, according as they are so held, Mr. H. C. Watson, in his recently completed "Compendium of the

Cybele Britannica" (Part III. p. 510), reports one, the *C. oxyacanthoides* of Thuillier, as being ascertained to grow in the Thames, Humber, Tyne, and East Highlands provinces. It will doubtless be found in other provinces, and amongst the rest the Mersey province, in which the only species hitherto recorded is the common *C. monogyna* of Jacquin.

From the specimens now exhibited it will be seen that the three segregate species just referred to occur within the limits of the Manchester Flora, and as some confusion seems to have arisen in their nomenclature, it will be desirable to give, briefly, the characters by which they may be separated from each other. Similar confusion exists amongst continental authors; thus, Boreau, in his Flore du Cent. (T. 2, p. 234), makes the *C. oxyacanthoides* of Thuill. a synonym of *C. monogyna* Jacq., while Koch, in his Synopsis (p. 303), and Grenier and Godron, in their Flore de France (T. 1, p. 567), refer it to the *C. Oxyacantha* L., although they recognise Jacquin's plant.

C. monogyna Jacq.	*C. kyrtostyla* Fing.	*C. oxyacanthoides* Thuill.
Peduncles; *glabrous.*	Ped.: *pubescent.*	Ped.: glabrous.
Divisions of calyx: *glabrous,* or with a few scattered hairs; lanceolate acuminate; reflexed and closely applied to the fruit.	Calyx div.: *pubescent;* oblong acuminate; patent-reflexed.	Calyx div.: glabrous; triangular acuminate; spreading, but recurved at the extremity.
Style: *one;* slightly bent.	Styles: 1 to 2; erect, or slightly bent.	Styles: *two to three;* often diverging.
Fruits: subglobose; *with one stone.*	Fruits: oblong; *one to two* stones?	Fruits: large; oval; *two to three stones.*
Leaves of barren shoots; deeply divided into 3 to 5 lobes, which are somewhat acute.	Leaves: with three to five acute lobes; base with sides generally convex.	Leaves: usually *trilobate*; lobes obtuse; base cuniform with concave sides.
Nerves of leaves: *divergent.*	Nerves: *divergent.*	Nerves: *convergent.*

The prevailing form in this district is the *C. monogyna* Jacq.; it is that of which all our quickset hedges are made, and is said to flower a fortnight later than the third sub-species.

The second form, the *C. kyrtostyla* of Fingerhuth, I collected on the 25th May, 1867, in Botany Bay Wood (Mersey Province, County No. 59 of Watson) on the path from Barton Moss to Worsley, where it forms several handsome trees. It attracted my attention at once by the large proportion of its flowers which possessed two styles, and by the comparative large size of the corymbs; its fruit I have not been able to examine, as the ground in which it occurs is preserved by the Earl of Ellesmere, and is accessible only by a written order.

The addition of the third sub-species, *C. oxyacanthoides* Thuill., to our flora, is the most noteworthy, and is due to the keen sight of Mr. John Hardy, who detected a single bush of it on the 27th August last, at Marple (Trent Province, County No. 57), on the right hand side of the high road from the railway station, a little past the uppermost lock of the canal. The leaves of this plant are of considerable size, being about twice as large as those of a plant in my herbarium from Hampstead, collected by Dr. J. Boswell Syme, and excepting that the leaves are glabrous, the Marple plant appears to agree with the variety β *majus*, Hobkirk. The fruits on the specimens exhibited are small and urceolate in form, but they were not mature at the time they were gathered. For some seasons back I have unsuccessfully sought for it in this neighbourhood, and at present it is not ascertained to occur in the Mersey province, though it will doubtless be discovered on a more careful search. The most obvious character for determining this sub-species in the absence of the flower or fruit, is the arrangement of nerves in the leaves, which are arcuate, with the extremities turned towards the midrib; in the two first-named forms the nerves are arcuate in the opposite direction, *i.e.* they are turned outwards.

It is not a little remarkable that there is one peculiarity in the venation of the hawthorns which is invariably overlooked by the draughtsman and engraver, viz., the direction of the secondary nerves, which proceed from the midrib to the base of each sinus; such an arrangement is very rare, being found only in some other species of *Cratægus,* as *C. Azarolus,* &c., in species of *Fagus,* and in a few other plants.

Mr. JOSEPH SIDEBOTHAM exhibited a series of specimens of *Limobius dissimilis,* from Llandudno, on which the markings were very distinct and perfect; he discovered the species in considerable numbers beneath the flowers of *Geranium sanguineum.*

Mr. SPENCER H. BICKHAM, Jun., reported the occurrence of *Myosurus minimus,* L., in plenty at Vale Royal, near Northwich, which species he believed had never previously been noticed in the neighbourhood. Mr. Bickham then exhibited a series of specimens of *Polygonum minus,* Huds, collected at Mere and the surrounding district; he stated that he had searched for *Polygonum mite,* Schrank, but without success, and believed with Mr. Hunt, that luxuriant specimens of *P. minus* had been mistaken for it: on the other hand he called attention to the fact that in 1859 Mr. John Hardy, to whom Mr. Bailey had previously alluded, distributed specimens of *P. mite* from Mere, through the Thirsk Exchange Club, and on this authority Mr. J. G. Baker, the Curator, remarked in the report, "new to the Mersey Province."

It seems doubtful also whether *Alopecurus fulvus,* reported from the same locality, has not been erroneously recorded, peculiar states of *A. geniculatus* having been mistaken for it. As, however, it was found in considerable quantity at Oakmere, in 1868, it appears probable that it may occur elsewhere in Cheshire.

Ordinary Meeting, November 29th, 1870.

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

Sir Eustace Fitzmaurice Piers, Bart., and Edward John Syson, M.D., Medical Officer of Health for Salford, were elected Ordinary Members of the Society.

Mr. R. D. Darbishire, F.G.S., exhibited a series of palæolithic instruments from the valley of the Little Ouse, and explained (after Mr. J. W. Flower, Q. J. Geolog. Soc. xxv. 449) the general features of the district and the deposit of the beds and the implements.

Mr. W. Boyd Dawkins, F.R.S., indicated the age of these deposits as related to the period of the existence of Elephas primigenius in the district of the south east of England and the adjoining portions of the bed of the German ocean and the north west portions of France.

"The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena," by Professor Osborne Reynolds, M.A.

Although the tails of comets are usually assumed to be material appendages which accompany these bodies in their flight through the heavens—and the appearance they present certainly warrants such an assumption—yet this is not the only way in which these tails may be accounted for. They may be simply an effect produced by the comet on the material through which it is passing; an effect analogous to that which we sometimes see produced by a very small insect on the surface of still water. We see a dark spot, and on looking closer we find a small fly or moth flapping its wings and creating a disturbance which was visible before the insect which produces it.

There is nothing else that we can conceive their tails to be so that they must be one or other of these two things; either

(1) Material appendages of the nucleus, whether the material be limited to the illuminated tail or surround the comet on all sides.

(2) Matter which exists independently of the comet, and on which the comet exerts such a physical influence as to render it visible.

Respecting the composition of these bodies, Sir John Herschel says:—"There is beyond question some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the ætherial or imponderable elements) may ere long enable us to penetrate this mystery, and to declare, whether it is matter in the ordinary acceptation of the term that is projected from their heads with such extravagant velocities, and if not impelled at least directed in its course by reference to the sun as a point of avoidance. In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration than in that of the enormous sweep which it makes round the sun in perihelio, in the manner of a straight and rigid rod, in defiance of the law of gravitation, nay, even of the received laws of motion, extending (as we have seen in the comets of 1680 and 1843) from near the sun's surface to the earth's orbit, yet whirled round unbroken: in the latter case through an angle of 180° in little more than two hours. It seems utterly incredible that in such a case it is one and the same material object which is thus brandished. If there could be conceived such a thing as a *negative shadow*, a momentary impression made upon the luminiferous æther behind the comet, this would represent in some degree the conception such a phenomenon irresistibly calls up. But this is not all. Even such an extraordinary excitement of the æther, conceive it as we will, will afford no account of the projection of lateral streamers, of the effusion of light from the nucleus of the comet towards the sun; and its subsequent rejection of the irregular and capricious mode in which that effusion has been seen to take place, none of the clear indications of alternate evaporation and condensation going on in the immense regions of space occupied by the tail and coma—none, in short, of innumerable other facts which link themselves with almost equally irresistible cogency to our ordinary notions of matter and force."

There can be no doubt that if these tails are matter moving with the comet, this matter must be endowed with properties such as we not only have no experience of, but of which we can form no conception. This alone would seem a sufficient reason for rejecting the first hypothesis. Moreover, on the second hypothesis there is no difficulty in the immense velocity with which these tails are projected from the head or whirled round when the comet is in perihelio. For to take the "negative shadow" as an illustration, here we should have a velocity of projection equal to that of light, and the only effect of the whirling would be a slight lagging in the extremity of the tail, causing curvature similar to that which actually exists. And whatever the action may be, if its velocity of emission or transmission be sufficiently great, this effect will be the same; but whether this hypothesis is to be rejected because involving assumptions beyond conception or contrary to experience, must depend on the answers to the following question—Do we know, or can we conceive any physical state into which any substance which can be conceived to occupy the space traversed by comets could possibly be brought so as to make it present the appearance exhibited by comets?

Now, I think the answer must be in the affirmative, and that we may leave out the terms conceive and conceivable. For electricity is a well known state, and gases are well known substances; and when electricity under certain conditions, as in Dr. Geissler's tubes, is made to traverse exceedingly rare gas, the appearance produced is similar to that of the comets' tails; the rarer this gas is, the more susceptible is it of such a state, and so far as we know there is no limit to the extent of gas that may be so illuminated. Hence we may suppose the exciting cause to be electricity, and the material on which it acts and which fills space to have the same properties as those possessed by gas. What is more, we can conceive the sun to be in such a condition as to produce that influence on this electricity which should cause the tail to occupy the direction it does. For such an electric discharge will be powerfully repelled by any body charged with similar electricity in its neighbourhood.

The electricity would be discharged by the comets on account of some influence which the sun may have on them, such an influence being well within the limits of our conception.

The appearances of the comet in detail, such as the emission of jets of light towards the sun and the form of the illuminated envelope are all such as would necessarily accompany such an electrical discharge.

In fact, if the possibility of such a discharge is admitted, I believe it will explain all the phenomena of comets. As to the possibility, or even the probability of such a discharge, I think it may be established on very good grounds.

The tails of comets may or may not be one with their heads; but whichever is the case, it is certain that the difference in the appearance of comets and of planets indicates some essential difference either in the materials of which these bodies are respectively composed, or else in the conditions under which their materials exist. Now from the motion of comets we know that their heads follow the same laws of motion and gravitation as all other matter, and therefore we have good evidence, so far as it goes, that comets and planets are similarly constituted as regards materials. And since the appearance of a comet changes very much as it passes round the sun, any assumptions with regard to the material of comets in order to account for their difference from planets would not account for the variety of appearance the same comet presents at different times. On the other hand the conditions of comets and planets must necessarily be very different, from the extreme difference in the shapes of the orbits they describe. Each planet remains nearly at a constant distance from the sun (whatever that distance may be), so that the heat or any physical effect the sun may have upon it will also be constant; on the comets its action must change rapidly from time to time, particularly when the comet is in certain parts of its orbit. Hence we may say that the temperature and general physical condition of planets is nearly constant, and that of comets for the most continually varying.

There is, too, a very remarkable connection between the appearance of the comet and the rate at which the sun's action on it changes. Herschel says:—"Sometimes they first make their appearance as faint and slow moving objects, with little or no tail, but by degrees accelerate, enlarge, and throw out from them this appendage, which increases in length and brightness till (as always happens in such cases) they approach the sun and are lost in his beams. After a time they again emerge on the other side, receding from the sun with a velocity at first rapid, but gradually decaying. It is, for the most part, after thus passing the sun that they shine forth in all their splendour, and their tails acquire their greatest length and development; thus indicating plainly the sun's rays as the exciting cause of that extraordinary emanation. As they continue to recede from the sun their motion diminishes and their tail dies away, or is absorbed into the head, which itself grows continually feebler, and is at length altogether lost sight of."

Here, although unconsciously, Herschel has connected the increase of brightness with the increase of speed with which comets approach the sun, and the diminution in brightness with the diminution of the velocity with which they leave the sun. And although from Herschel's remark just quoted it might be inferred that proximity to the sun is the cause of the increase of brightness, this is proved not to be the case, for (as in the case of Halley's comet) when near its perihelion the tail always dies away, and the comet shrinks. Thus when the comet is nearest to the sun there is no development of tail, which shows clearly that it is not the intensity of the sun's rays but the change in their intensity that is the exciting cause of these extraordinary appearances. So that there is no reason to suppose that a planet composed of the same material as a comet, no matter how close to the sun, would show a vestige of tail or other cometic appearance.

It is then to this change in position that we must attribute those peculiar appearances which belong to comets.

Now, is not electricity the very effect which would naturally result from such a state of change and variation in condition?

A. De la Rive remarks, "Electricity is one of the most frequent forms which the forces of nature assume in their transformations." It certainly often accompanies a change in temperature. There is every indication that it is so in our atmosphere, for the times when its intensity is a maximum are just after sunrise and just after sunset, both winter and summer.

From these reasons it seems to me not only possible but probable that these strange visitors to our system are clothed in electrical garments with which the regular inhabitants are unacquainted.

The electricity must after all depend on the composition of the comet, for known substances do not all show the same electrical properties. Hence by assuming comets to be composed of various materials, we have a source to attribute the different appearances presented by the different individuals. To the same source we may attribute the irregularity in the direction of their tails and the lateral streamers they occasionally send out.

Secondly, I think this electrical hypothesis is supported by the to me seeming analogy between comets, the corona, and the aurora; an analogy which suggests that they must all be due to the same cause. They may be all described as streams of light or streamers, having their starting point more or less undefined, and traversing spaces of such extent and with such velocities as entirely to preclude the possibility of their being material in any sense of that word with which we are acquainted.

The aurora has long been considered as an electric phenomenon, and recently the same effect has been produced by the discharge of electricity of very great intensity through a very rare gas, there being no limit to the space which it will thus traverse, This being so, why should not the tails of comets and the corona also be electric phenomena? Their appearance and behaviour correspond exactly with those of the aurora, and there is surely nothing very difficult in imagining the sun which is the source of so much heat being also the source of some electricity. Neither will there appear anything wonderful in the electricity of comets when we consider that of the earth. We must not look on our inability to explain the cause of such an electric discharge as fatal to its existence, for we cannot any more explain the existence of the electricity which causes the aurora. If we cannot explain from whence these electricities come, we can at least show that the conditions which are most favourable to the development of the aurora exist in much greater force on the comets than they do on the earth. The greatest development of the aurora borealis takes place at the equinoxes. There is a cessation in summer, and another in winter. Now, the equinoxes are the times when the action of the sun on our northern hemisphere is changing most rapidly. Hence the condition favourable for the aurora is change in the action of the sun. The same thing is pointed out by the diurnal variation in the electricity of the atmosphere. Now, as has been already shown, the change in temperature on the comets is incomparably greater than it is on the earth, and its variation corresponds with the variation in the splendour of the comet.

Ångström has also shown that the light from the aurora, the corona, and the zodiacal light, are all of the same character, or all give the same bright lines when viewed through the spectroscope, and that these lines correspond to the light from no known substance. This indicates that whatever this light may be, the incandescent material is the same in all cases; or may we not assume that it is the medium which fills space that is illuminated by the electric discharges? This would be supported by the fact that the light from the heads of two small comets indicated carbon, whereas that from the tails only gave a faint continuous spectrum. For an electric discharge would first illuminate the atmosphere of the comet, or even carry some of the solid material off in a state of vapour, and then pass off to the surrounding medium. Thus while the spectrum from the head would be that of cometary matter, the tail would be due to the incandescent ether.

I would here suggest that gas, when rendered incandescent by electricity, may reflect light—it will certainly cast a shadow from the electric light—and if this be the case, part of the light from comets' tails may after all be reflected sunlight.

At any rate, it is certain that the appearance of streamers, the rapidity of change and emission, the perfect transparency and the wave-like fluctuations which belong to these phenomena, are all exhibited by the electric brush; in fact, the electric brush will explain all these appearances which have defied all attempts at explanation on a material hypothesis.

I have only to add that the main assumption involved in the electric theory is, that space is occupied by matter having similar electrical properties to those of gas; and I would ask, is it not more rational to make such an assumption than it is to attribute unknown and inconceivable properties to cometary matter?

Theories even, if founded only on rational speculation, often, I believe, prove very useful, insomuch as they afford observers a definite purpose in their observations—something to look for, something to establish or to refute; and I publish these speculations of mine at this particular moment in the hope that they may perchance serve such a purpose.

"On Iso-di-naphthyl," by WATSON SMITH, F.C.S. Communicated by Professor ROSCOE, F.R.S.

About the commencement of the month of March, 1870, when endeavouring, on the suggestion of Mr. John Barrow, in whose laboratory I was then engaged, to obtain anthracene by the action of a red heat upon naphthalin, the vapour of this body being passed through a red hot tube: I found that instead of the anticipated result occurring, according to the equation $7C_{10}H_8 = 5C_{14}H_{10} + 6H$, a body was obtained which had a *melting point* and also a *boiling point* pretty nearly agreeing with those of anthracene, but almost all its other properties were dissimilar to those characterising that body.

This substance I found to fuse at from 200° to 204° C., its boiling point lying over that of mercury considerably, and also over that of anthracene as nearly as I could judge.

It is difficultly soluble in alcohol and ether, more soluble in carbon tetrachloride and benzole, freely soluble, even in the cold, in carbonic disulphide and oil of turpentine.

From all the above solutions except that of the turpentine it crystallises in beautiful silky rhomboidal plates, which on drying interlaminate, and possess a delicate light yellowish green colour and silky lustre. From the turpentine it crystallises in beautiful white lance-shaped crystals congregating in tufts. Its subliming point lies considerably below its boiling point, indeed not far above its melting point.

It may be obtained perfectly white by carefully subliming the recrystallised substance at as low a temperature as possible. If the semi-purified body be recrystallised from any of the above named solvents, the mother liquors on filtering are found to have acquired a beautiful blue fluorescence, but the perfectly pure substance no longer yields a fluorescent solution.

A mixture of two parts of potassium bichromate and sulphuric acid, cause energetic oxidation of this substance, but no colouring matter is obtained by treating the product of oxidation so obtained, by Perkins' method for obtaining alizarin from anthrachinon.

Cold sulphuric acid is without action upon it. Warm sulphuric acid dissolves it, if pure, with a slight purplish colour. If containing any of the yellow substance which always contaminates the crude body, the warm acid assumes a blue colour, which on further warming becomes green and then brown.

Nitric acid oxidises it, with liberation of nitrous fumes.

Chlorine passed over it in the cold does not affect it, and apparently not even on slightly warming.

I find that it is impossible to distill naphthalin to dryness in any quantity, without this body being formed in minute quantity. If an appreciable quantity be not obtained on first distillation, it will be by transferring back the distillate to the retort and again distilling to dryness; a minute quantity of high boiling residue will then be obtained, raising the temperature towards 300° C.

A quantity of the pure substance, submitted to organic analysis, furnished the following numbers: —

		Grms.
I.	0·1240 grm. of substance gave	0·4307 CO_2
		and 0·0624 H_2O.
II.	0·1237 ,, ,,	0·4284 CO_2
		0·0626 H_2O.

	I.	II.	Calculated for $\left.\begin{matrix} C_{10}H_7 \\ C_{10}H_7 \end{matrix}\right\}$
Carbon	94·72	94·46	94·49
Hydrogen	5·59	5·62	5·51
	100·31	100·08	100·00

The hydrogen evolved in the process was collected and measured, and the following calculation made: —

Weight of Naphthalin converted, 26·30 grms. (nearly).
Volume of Hydrogen at 0° C.............. = 2359·7 cbc.
= 0·2107 grm. H.

$$\overbrace{2C_{10}H_8}^{256} = \left.\begin{matrix} C_{10}H_7 \\ C_{10}H_7 \end{matrix}\right\} \overbrace{+ H_2}^{2}.$$ ∴ 26·3 grms. lose 0·2055 grm. H.

Hydrogen actually liberated .... = 0·2107 grm.
,, calculated as above .... = 0·2055 grm.

For the formuala $7C_{10}H_8 = 5C_{14}H_{10} + 6H$ = 0·1861 grm. of H. must be liberated.

From these considerations, and seeing that the properties of the body considerably differ from those of Di-naphthyl as obtained by *Dr. F. Lossen, I propose to regard this body as an Isomer, and propose to name it accordingly *Iso-di-naphthyl.*

Prestolee Alkali Works, near Manchester,
November 29, 1870.

* Ann. der Chemie und Pharm.: Band cxliv. 71, 1867.

"Notes on the Botany of Mere, Cheshire," by Mr. GEORGE E. HUNT.

The border of Mere Mere has for long been a locality famous to the botanists round Manchester.

The first published Manchester floras bore its name as the habitat of the rare *Elatine hexandra* and *Limosella aquatica*.

In 1855, Mr. Wilson's Bryologia Britannica gave a still greater notability to the place by the record of several extremely rare mosses from thence, and among others of *Physcomitrium sphæricum*, which is thus recorded by him:—"On the dried mud of pools, Mere, Cheshire, Sept., 1834.—W. Wilson. Not found in any subsequent year: the only known locality in Britain."

The following are also recorded in the same work as occurring at Mere:—"*Phascum serratum β; Phascum sessile; Phascum rostellatum.*"

I was led, in 1864, by these various notices, to commence a systematic and continuous exploration of Mere, with the view of discovering as many of the recorded mosses as might still exist there. Some of them being exceedingly minute, it has taken a considerable time to detect all; and it may be of service to other bryologists in the district to mention those which grow there at the present date, and also the nature of soil they prefer.

1. *Physcomitrium sphæricum.* A careful search, in 1864, led to the re-discovery of this species in very minute quantity. In 1865 it was still more sparing (not above a dozen capsules). 1866 was so exceedingly wet a season that the plant could not have come up at all. 1867, it again occurred very sparingly. 1868, it was plentiful, but destroyed by the autumn rains before much of the fruit had ripened. 1869, again frequent, and would have been plentiful but the autumn rains again destroyed it whilst the fruit was even more immature than in the preceding

year. 1870, very plentiful, and abundance of it has come to maturity. This moss *always* grows on dried mud.

2. *Phascum serratum* β is frequent every autumn on clay and sandy banks at Mere; it occurs quite frequently in corn fields at Bowdon, in damp seasons, coming up a few weeks after the corn has been cut. In corn fields at Bowdon its companions are *Phascum muticum, Phascum alternifolium,* and *Pottia truncata,* and very rarely *Trichodon cylindricus*—the latter never fruits in this district.

3. *Phascum nitidum,* frequent every autumn at Mere on clay and sandy banks; it occurs elsewhere about Bowdon on newly-cut ditch banks.

4. *Phascum rostellatum,* on banks at Mere, with the two previous species, but much more sparingly. It has also been found in Sussex by Mr. Mitten, and was collected there again last year by Mr. Davies. It is one of the rarest of all the British mosses.

5. *Phascum sessile,* very rare at Mere. I collected it in the autumn of 1869, and again in November, 1870, intermixed very sparingly among *Phascum serratum,* from which it is difficult to separate it except with the aid of the microscope. With this it can be at once distinguished from that species by its longer, more rigid, almost entire leaves, with a very wide nerve. *Phascum serratum* has no nerve, and the leaves are spinulosely serrated. *Phascum sessile* was gathered in Sussex many years since, but I have not heard of its recent discovery either there or elsewhere. It is one of the rarest British mosses.

6. *Phascum patens,* on dried mud, almost every season, intermixed with *Physcomitrium sphæricum,* and usually much more plentiful than that species. This moss comes up in autumn in the Ashley district of Bowdon, although very sparingly, wherever an open drain has been cut in spring. It also springs up about Bollington, under the same circumstances.

7. *Phascum cuspidatum.* I have not yet found this *at* Mere, but it comes up on banks on the Chester Road between Bowdon and Bucklow Hill, when they have been newly made up, or plastered with mud from the road.

8. *Leskia polycarpa* fruits freely about the roots of trees on the borders of Mere, both in autumn and spring.

9. *Hypnum riparium,* a very neat variety of this moss, fruits in abundance in August and April, on clay banks and at the roots of trees at Mere.

Hepaticæ.

Riccia fluitans and *crystallina* are both frequent on dried mud at Mere, with *Phascum patens,* &c., and both species fruit freely there.

Numerous interesting flowering plants are also found, viz., *Elatine hexandra, Limosella aquatica, Peplis portula, Polygonum minus, Littorella lacustris*—all plentiful on mud; *Carex vesicaria,* fringing the woods at the edge of the Mere.

Scirpus acicularis, in vast quantity in sandy places.

Carex Œderi, in stony and grassy places. This is the true *Œderi,* and very rare. I have only seen it elsewhere on the sands on the south side of Southport, where it is very abundant and luxuriant. It appears quite distinct as a species from *C. flava* (including *C. lepidocarpa*), with which it is often placed as a variety,—

Centunculus minimus, frequent some seasons in the open pastures on the borders of the Mere.

Mentha sativa, in ditches by the road sides between Bucklow Hill and Mere Mere.

Rubus Balfourianus and *Rubas pallidus,* in thickets by the Mere.

Polygonum mite has been reported from Mere, but after searching without success for it for several seasons, I can only suppose that some of the more luxuriant forms of *minus,* frequent there, have been mistaken for it. The seeds of *P. minus*, which are *shining* black, and only half the size of those of *mite*, afford the only safe distinction.

Accompanying are specimens of the rarer mosses, from which it will be seen how minute they are, and how easily they may be overlooked without most careful search. The specimens sent were collected on Saturday, 5th November.

Mr. HARDY remarked that he had no claim whatever to be considered as the original discoverer of *Polygonum mite* in the Manchester district; for so long ago as 1828, Mr. William Wilson, of Warrington, sent the plant from a Cheshire locality, under the erroneous name of *minus*, to the late Sir William Jackson Hooker, in whose herbarium at Kew the specimens still are. Mr. Hewett C. Watson, the author of the "Cybele Britannica," mentions these specimens, and does not express any doubt of their being the *P. mite* of Schrank. Mr. Hardy found the plant at Mere in 1860, and sent specimens to the Botanical Exchange Club, then located at Thirsk: and Mr. J. G. Baker, the Curator, in his report for the next year, mentions these specimens as new to the Mersey Province. Mr. Hardy stated his belief in Mr. Watson's idea, that *P. mite* was much more difficult to distinguish from *P. Persicaria* than from *P. minus;* and he had not the least doubt, notwithstanding Mr. Hunt's objection, that, now special attention having been called to the species in question, it would be proved, in the course of another season, to be an inhabitant not only of the Mere district, but common in other stations included in the Manchester Flora.

Correction in paper on "The Hawthorns of the Manchester Flora," Proceedings, p. 37.

The locality for *Cratægus oxyacanthoides*, Thuill., lies within the Mersey province, and not, as stated, in the Trent province, the station being only a few hundred yards from the boundary of both provinces.

CHARLES BAILEY.

30th November, 1870.

Ordinary Meeting, December 13th, 1870.

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

Mr. John Angell, Science Master at the Manchester Free Grammar School, and Mr. Carl Schorlemmer, Senior Assistant in the Chemical Laboratory of Owens College, were elected Ordinary Members of the Society.

The PRESIDENT stated that the "grub," as the larva of the Harry Longlegs, the *Tipula oleracea* of entomologists, was commonly called, had made great ravages with meadow grass during the last summer. In the eastern parts of the township of Moston, near this city, some fifty or sixty acres had been for the most part destroyed. After the land had been manured in the spring, the grass showed well until the middle of May, when it began to disappear and leave the ground nearly bare. In the space of a square foot he found twelve of the grubs, and all the roots of the grass under that space appeared to be quite eaten through. Several remedies, such as salt and gas lime, have been proposed for destroying the grub, but these, although effective, exercise for the time a deleterious influence on the grass. The fancy onion growers of the district, chiefly weavers, keep them down by careful watching. He had been surprised at the growing of onions betwixt Oldham and Manchester by working men, one of whom had produced a specimen 25 ounces in weight. This did not obtain the prize, which was awarded to an onion grown at Hollinwood of 29½ ounces. For many years past the south-east part of Lancashire has been noted for growing large gooseberries and celery, and it is now equally famed for its onions.

"Some observations upon Railway Accidents, and suggestions for preventing their frequent occurrence," by W. B. Johnson, C.E.

The early history of our Railways does, I believe, show that the accidents in the first few years were mainly due to the breakage and derangement of some portions of the rolling machinery, and this to a much greater proportion than prevails at the present time. We rarely hear now of any fatal accidents arising from the breakage of the locomotive engine, yet 25 years ago they were far from being uncommon, especially if we include accidents arising from boiler explosions, and engines running off the lines.

This observation is made, because it is necessary in looking at a question such as is now under consideration, to ascertain if possible how this change has been brought about. In the first place it must be remarked that the traffic upon our railways, both goods and passenger, has increased to an almost incredible extent within the period just named; but the writer is inclined to believe that the change has not arisen from this altered condition (as regards traffic), but is to be accounted for in some degree by the very marked improvements that have been made in the locomotive engine itself: for instance, the accurate balancing of the working and fixed parts of the engine, that obtains at the present day, has done much to reduce the number of accidents arising from broken axles and running off the line. The enquiry may very reasonably be made as to whether corresponding improvements have been made in the other departments of railway construction and management. The answer is somewhat doubtful, for while the locomotive engine has steadily improved under the united and untiring labours of many able scientific engineers, the system of railway points or switches and signals remains the same in principle, if not in practice, to that in use on some of our earliest railways. The present arrangement and construction of points and signals do not appear to be adapted to meet safely the requirements of the traffic of to-day, as is too clearly demonstrated by the many recent accidents. An arrangement of points might be adopted, that would considerably reduce the number of accidents now occurring, and that by placing the points on the main lines, so that in all cases without any exception (saving at terminals having no through traffic and main junctions) they shall open in a direction opposite to that in which the trains run.

It must be apparent that under such an arrangement, accidents could not take place by a train being inadvertently turned into a siding, such as occurred at Tamworth, on the London and North Western Railway, not many weeks since; and all accidents of this class might, under such an arrangement of points just named when generally applied, be considered as impossible of occurrence. No doubt in many cases such an arrangement of the points is adopted, perhaps for the sake of convenience only, but the full benefit can only be derived by its universal practice.

More than 25 years since, the writer represented to several railway officials the security arising from the carrying out of such a system of points into general practice; but it was then considered as carrying precautionary ideas too far, and convenience had the rule, and appears to have had up to the present day. Of course the increase of traffic has materially increased the contingencies leading to accidents, and the question may be fairly raised—whether railway companies should be allowed to take any amount of traffic they may choose to do, without being compelled, by parliamentary enactment if necessary, to provide in every possible way against accident to the lives of the passengers committed to their charge. The usual objections of expense and inconvenience will no doubt be made against carrying out universally the arrangement of points now named; but whatever these objections might amount to, the writer is of opinion that in the long run its adoption would be found to be beneficial to the shareholders of our railways, and it would contribute in some degree to the safety of the travelling public.

There are two other sources of accidents on our railways that require notice—one, the system, now so prevalent, of centralising the signals, and the other, the breaking and making up of trains on the main line.

The centralising of the signal handles into one box may possibly possess some advantages in saving wages, and also in placing the signals and points connected with or dependent upon each other within the control of one man; but may not this be carried too far? When the centralising of signals requires the man in charge to have his attention directed to two different trains at the same time, and perhaps coming in opposite directions, it does create contingencies of a nature that will, at some time or other, lead to accident, and it is unreasonable to expect a man at such critical junctures always to do the right thing. Another objection to the centralising of signals arises from the working of the distant ones. The mechanism required to form the connection between the signal box and the signal itself, is on account of the distance, liable to derangement, being affected by frost, heat, and rain, and repairs and adjustments are frequently necessary, thus creating another class of contingencies that may lead to accident. And it may be further observed that it does sometimes occur that the distant signals are beyond the observation of the signalman in his box, and is always so in thick weather; so that he has no chance of knowing, in such cases, whether the signals answer to his workings in the box or not.

The breaking and making up of trains on the main line has been the occasion of many accidents, and its continuance, especially upon lines having a large traffic, must lead to similar results. It needs no argument to show that a line of railway upon which such work is never done has removed one contingency to accident, and to that extent it is a safer line to travel upon.

To these contingencies leading to accident might be added others, but the writer will now only refer to the one arising from imprudent management, in allowing slow and sometimes even luggage trains to precede an express without sufficient margin of time.

Viewing these contingencies together, as combining to bring about one result, viz.: accident, we must cease to wonder that they are so frequent, and begin to wonder that they so seldom occur.

"Contributions towards a knowledge of Anthophila (Hymenoptera Aculeata) in the Mersey Province," by Mr. F. O. Ruspini. Communicated by H. A. Hurst, Esq.

The following list is very meagre, and contains only 56 of the 220 species of bees known to inhabit the British Isles. It simply professes to be the result of one season's collecting by the author, mostly within the limits of a single parish in Cheshire.

Family I. Andrenidæ Leach.

Sub-family I. Obtusilingues Westw.

Genus Colletes Latr.

1. *C. cunicularia* Linn.=hirta St. Farg. Discovered by Mr. Nicholas Cooke near Liverpool in 1869; appears in April.

2. *C. succincta* Linn.=fodiens Curtis Brit. Ent. II. fol. 85. Abundant at Lindow, Cheshire, in August.

3. *C. Daviesana* Kirby MSS. Lindow Common, August; not so abundant as succincta.

Sub-family II. Acutilingues Westw.

Genus Sphecodes Latr.

The females appear in spring, and both sexes in the autumn.

1. *S. gibbus* Linn.=Melitta sphecoides Kirby. Plentiful

on Lindow Common and at Alderley; also taken by Dr. Simpson in Lancashire.

2. *S. rufiventris* Panz.=rufescens Smith's Monog.=gibba Fabr. and Kirby. Very plentiful all over the country.

3. *S. subquadratus* Smith=gibbus Wesmael. A rare species, occurring at Lindow.

4. *S. ephippius* Linn. ♂=divisa Kirby, and ♀ Geoffrella Kirby. Plentiful at Lindow, Cheshire.

Genus Halictus.

The females appear in spring, and both sexes in autumn.

1. *H. rubicundus* Christ. = flavipes Panz. Abundant everywhere.

2. *H. Tumulorum* Linn.=flavipes Auct. Lindow, Cheshire, and Silverdale, Lancashire—an abundant insect.

3. *H. 4-notatus* Kirby. Common at Lindow.

4. *H. cylindricus* Fabr. ♂=abdominalis Kirby, and ♀= fulvocincta Kirby. Lindow, Cheshire, and Silverdale, Lancashire—a very common insect.

5. *H. albipes* Fabr. ♀=obovata Kirby. A local species: plentiful at Lindow.

6. *H. villosulus* Kirby. Common in Cheshire and Lancashire.

7. *H. nitidiusculus* Kirby. With us the most abundant of the genus, but rare in Northumberland.

8. *H. subfasciatus* Nyl. A rare species: taken on Lindow Common.

9. *H. minutus* Kirby. Taken by Dr. Simpson in Lancashire and by the author at Lindow.

10. *H. atricornis* Smith n. s. Ent. Ann. 1870. Occurs only at Hazel Grove, near Stockport.

11. *H. Smeathmanellus* Kirby. Local; scarce at Lindow.

12. *H. Morio* Fabr. Taken by Dr. Simpson in Lancashire and by the author at Lindow.

Genus ANDRENA Fab. (in part).

1. *A. cineraria* Linn. Plentiful at Lindow in April and May.

2. *A. albicans* Kirby. Abundant everywhere in spring.

3. *A. fulva* Schrank. Common in Cheshire in spring.

4. *A. varians* Rossi. Not abundant at Lindow, appears in May.

5. *A. nigroœnea* Kirby. Plentiful at Lindow in April and May.

6. *A. Trimmerana* Kirby. Plentiful at Lindow in May; the ♀ emits a strong smell of garlic.

7. *A. denticulata* Kirby. ♀ = Melitta Listerella Kirby. A rare species, not uncommon at Lindow in May. All the specimens taken were dwarfish females.

8. *A. fulvescens* Smith. Rather scarce at Lindow; appears in June and July. One specimen, a ♂, is of stronger build than the type, and is more densely pubescent on the thorax and abdomen.

9. *A. albicrus* Kirby. Taken by Dr. Simpson, in Lancashire and by the author plentifully at Lindow in May.

10. *A. minutula* Kirby; var.=parvula Kirby. Taken at Lindow, but not plentifully, in May.

11. *A. Collinsonana* Kirby. ♀ = proxima Kirby, and var. ♀ = digitalis Kirby. A pair taken at Hazel Grove, near Stockport, by the author in July, 1870.

12. *A. xanthura* Kirby = A. chrysosceles Nyl.; var. ♂ = ovatula Kirby. Plentiful at Lindow in May.

Family II. Apidæ Leach.

Sub-family II. Cuculinæ Latr.

Genus NOMADA Fabr. (in part).

1. *N. ochrostoma* Kirby. ♀ = vidua Smith. Taken at Lindow in May and June, but sparingly.

2. *N. Fabriciana* Linn. Noticed in some numbers on a sandbank at Lindow in May, a rather unusual occurrence.

3. *N. alternata* Kirby. ♀ = Marshamella Kirby. Abundant in Cheshire and Lancashire in spring; parasitic on A. nigroænea and A. Trimmerana.

4. *N. succincta* Panz. = Goodeniana Kirby. Taken at Lindow in spring, but sparingly; parasitic on A. Trimmerana.

Genus Epeolus Latr.

1. *E. variegata* Linn. Local. The author bred a ♀ in August from cells of Colletes Daviesana found on Lindow Common.

Genus Cælioxys Latr.

1? *C. simplex* Nyl = conica Kirby; ♂ = sponsa Smith. Remains of a specimen found in an ant's nest at Silverdale.

Sub-family III. Dasygastræ Latr.

Genus Osmia Latr.

1. *O rufa* Linn. ♀ = bicornis of Linn. Dr. Simpson has a ♀ taken at Frodsham, Cheshire.

2. *O. fulviventris* Panz. = hirta Smith. ♀ = Leaiana of Kirby. Taken by the author near Alderley sparingly in June, 1867. It is a local insect.

Genus Megachile Latr. (in part).

1. *M. centuncularis* Linn. The author dug up some cells of this species on Lindow Common in August, 1870.

Sub-family IV. Scopulipedes Latr.

Genus Anthophora Latr.

1. *A. acervorum* Fabr. = retusa Kirby. Seen but not captured at Lindow in early spring.

Sub-family V. Sociales Latr.

Genus Apathus Newman.

The females appear in spring, and both sexes in autumn.

1. *A. vestalis* Kirby. Females abundant at Lindow in spring; also taken by Dr. Simpson in Lancashire.

2. *A. campestris* Panz. The author has observed it parasitic on Bombus muscorum. Many of the varieties are plentiful at Lindow.

3. *A. Barbutellus* Kirby. Sparingly at Lindow. Dr. Simpson has also specimens taken in Lancashire.

Genus Bombus Auct.

The females appear in spring, and all the sexes in autumn.

1. *B. muscorum* Linn. Abundant everywhere.

2. *B. senilis* Fabr.=muscorum Kirby. Abundant throughout our district.

3. *B. fragrans* Pallas. A local insect, found on Lindow Common. This bee when alive has an agreeable perfume.

4. *B. Derhamellus* Kirby (also Raiella of Kirby.) Scarce at Lindow.

5. *B. pratorum* Linn. ♀=subinterrupta Kirby, and ♂ = Burrellana Kirby. Very abundant in our district.

6. *B. lapidarius* Linn. Abundant with us.

7. *B. terrestris* Kirby. One of our commonest Bombi.

8. *B. lucorum* Linn. ♀ = terrestris Linn. By far the most abundant of the genus with us. The author has observed this species swarming in hedges in early spring, probably attracted by the juicy shoots of the whitethorn.

9. *B. hortorum* Latr. Plentiful in our district.

10. *B. subterraneus* Linn. Black var.=Harrisella Kirby. Somewhat local, but plentiful at Lindow, where the black variety also occurs.

Genus Apis Linn. (in part).

1. *A. mellifica* Linn.= domestica Auct.

2. *A. ligustica* Spinola = helvetica Hermann. Both these species are cultivated in our district.

Note.—The author desires to take this opportunity of acknowledging his indebtedness to Mr. Frederick Smith of the British Museum, both for types of various species and for much kind help in the determination of his captures. He further wishes to make known to collectors of Hymenoptera in this district that he will be much obliged to any of them who can communicate to him the names of any species of Anthophila taken by them in the "Mersey" province, with a view to his publishing hereafter a supplement to the foregoing list.

Fulshaw Farm, Wilmslow, Cheshire.

Ordinary Meeting, December 27th, 1870.

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

"Observation of the Eclipse of the Sun, December 22nd, 1870," by J. B. Dancer, F.R.A.S.

The eclipse of the Sun on Thursday, the 22nd of December, was favourably observed at Ardwick. Although a slight fog prevailed, all the details of the phenomenon were distinct, and tolerably well defined. A number of spots were visible on the Sun's surface, two of which were of some magnitude. The nuclei of these spots were linked together by maculæ, and surrounded by a penumbra which extended to a considerable distance. Faculæ also were very numerous and distinct. The approximate times of contact taken by a chronometer corrected by the standard clock at the Town Hall were as follows:

	H.	M.	S.
First contact of the moon's limb with the sun	11	5	49
Contact of moon's limb with nucleus of the first large spot..	11	31	36
With the nucleus of the second large spot...........	11	37	20
Last contact of moon's limb with the sun, Greenwich mean time	1	37	3

The temperature during the progress of the eclipse was taken at intervals by a mercurial thermometer with a black bulb in vacuo, exposed to the sun at the height of 4 feet from the ground.

TIME. H.	M.	S.		TEMP. DEGREES.
11	10	0	....	31·5
—	35	0	....	30·25
—	45	0	....	29·75
—	50	0	....	29·25
12	22	0	....	27·2
—	35	0	....	28·5
1	37	0	....	29·0

I had an impression that the moon's edge could be traced a short distance from the edge of the sun at the upper and lower points of contact, but this might be imagination.

The black surface of the moon appeared very uniform in colour. I tried with powers of 80 and 180 to distinguish the moon's disc, but did not succeed. Light clouds were passing over the sun's disc at this time. The diminution in light was quite perceptible at the time of the greatest phase.

Mr. Baxendell said that he observed the commencement of the Eclipse at Cheetham Hill. The first contact took place at 11h. 5m. 46·2s. G.M. Time, or 24·2 seconds later than the time calculated by Mr. Dickinson and Mr. Hind. The definition of the limbs of the sun and moon, and of the spots on the solar disc, was remarkably good, and he did not think his observation of the time of first contact could be in error to the extent of one second. The limb of the moon on the sun's disc appeared to be more sharply defined than the sun's limb. No distortion of the cusps was noticed. Unfortunately he was obliged to leave the observatory before the end of the eclipse, and therefore did not observe the time of last contact.

"Notes on some of the High Level Drifts in the Counties of Chester, Derby, and Lancaster," by E. W. Binney, F.R.S., F.G.S., President of the Society.

Introductory Remarks.

Until late years little attention has been devoted to the study of the deposits of Drift, found on the sides of the Pennine Chain, and the hills lying between Macclesfield and Buxton.

The late Mr. Joshua Trimmer drew attention to the beds of Drift on Moel Tryfaen, in Caernarvonshire so early as 1831.

In 1841 a Paper of his own was read before the Manchester Geological Society, and published in its Proceedings for that year, on the Lancashire and Cheshire Drift, wherein it was stated, that the Drift in some places as near Black Moss, above Ramsbottom, in Walmersley, and at Pikelow, near Macclesfield, reached to heights of from 1,000 to 1,200 feet above the level of the Irish Sea; and he said that he had little doubt but some of the most ancient portions of it might have passed over the Pennine Chain, through the Vale of Todmorden, by the Summit Valley, above Littleborough, as the highest part of the last-named valley was not more than 612 feet above the level of the Irish Sea.

In 1862 he took Mr. Prestwich, F.R.S., President of the Geological Society, to show him the Arnfield deposit, and in the course of conversation that gentleman mentioned to him the fact of his (Mr. P.,) having seen some fossil shells in a bed of gravel near the turnpike road, leading from Buxton to Macclesfield, about three miles from the last-named place. Accordingly, when describing the Arnfield specimens in a notice published in the Proceedings of the Society for Nov. 18, 1862, he stated that fact as having been observed by Mr. Prestwich. This notice led Mr. Sainter, Surgeon, of Macclesfield, and Mr. Green, F.G.S., of the Geological Survey, to hunt out and explore Mr. Prestwich's locality, and they soon found it in an old gravel pit below Walker Barn, above Vale Royal.

Mr. Hull, F.R.S., in his memoir of the Geology of Bolton-le-Moors, published in 1862, at page 29, notices the occurrence of Drift on Winter Hill at an elevation of 1,380 feet.

The late Mr. J. Whitaker, of Burnley, in 1863, described a bed of gravel containing chalk flints, at Barrowford, near the foot of Pendle Hill. See Vol. IV., p. 176, of the Transactions of the Geological Society of Manchester.

In November, 1864, Mr. R. D. Darbishire, F.G.S., read a paper on the Marine Shells found near Macclesfield, and in a Postcript to the Memoir printed in Vol. III. (3rd series) of the Society's Transactions, alludes to the beds near the Buxton Road, mentioned by Mr. Prestwich, which he makes to be about 1,150 feet high. He also alludes to the Vale Royal and Macclesfield beds, and gives a full catalogue of the shells found in the latter in a communication to the Geological Mazagine for July, 1865.

In March, 1865, Mr. Sainter read a paper before the Manchester Geological Society on the Macclesfield Drift Shells, wherein he alludes to Mr. Prestwich's beds. See Vol. V., p. 114, of that Society's Memoirs.

Mr. A. H. Green, in his Memoir of the Geology of Macclesfield, &c., published in 1866, notices the Vale Royal and Macclesfield beds, as well as the scattered boulders (No. 1) on the hill sides.

Mr. John Aitken, F.G.S., the President of the Manchester Geological Society, in a paper read before that body in February, 1868, and published in Vol. VII., p. 5, of its Memoirs, notices the occurrence of a thin bed of gravel in which he found a chalk flint on Holcome Hill, near Ramsbottom, at an elevation of 1,150 feet above the sea.

Mr. A. H. Green, in his interesting memoir on the Carboniferous Limestone, &c., of North Derbyshire and the adjoining parts of Yorkshire, published by the Geological Survey in 1869, notices the heights at which the drift has been found on the western side of the Pennine Chain, and gives a map showing its distribution.

General Description.

The Drift Deposits, all of which have been found at high levels, may be classed under four distinct heads, namely :— 1st. Scattered blocks of granites, greenstones, porphyries, silurians, mountain limestones, and carboniferous, now found lying on the surface of the ground without any clay or sand. 2nd. Strong bluish brown till, containing rounded and angular blocks varying in size of the above named rocks. 3rd. Stratified beds of sand and gravel, containing chalk flints generally yielding entire or fragmentary marine shells. 4th. Gravelly clay frequently containing the remains of shells in greater or less abundance.

(No. 1.)

The first named blocks of stone are found more or less on the tops and sides of the crescent of hills from south of Clulow Cross through Cheshire and Derbyshire to Rivington Pike in Lancashire, and further northwards at heights varying from 1,000 to 1,400 feet above the Irish Sea. They are found on ground higher than deposits No. 3, at Bull Strang, Vale Royal, and Bugsworth, than No. 2, at Bakstondale, and No. 4, at Arnfield. They vary in size from a hundredweight to several tons, and are probably the remains of a bed of till like No. 2, the clay and small pebbles of which have been removed by denuding causes in the course of a long period of time.

Bakstondale Section (No. 2).

At the top of a valley of this name, above Lyme Park, in Cheshire, some 1,000 feet above the level of the sea, in sinking a pit down to the Smut coal, a bed of bluish brown till, very full of granites, greenstones, and other foreign rocks, many of them weighing several hundred weights, resting on broken coal measures, was met with. No fossil shells were found in it, and it could not be distinguished from the ordinary till found near Manchester, except that the pebbles on the whole were larger and more numerous. Many of the rocks were striated and polished, whilst others were both rounded and angular. The deposit was in a sheltered spot, and appeared to be the remains of a larger bed, the greater part of which had been removed by denudation. At a higher level than this bed of till, detached boulders of considerable size were scattered over the surface, and are probably the remnants of a former extension of the till over the places where such boulders are now found. No beds of gravel or sand were seen in the vicinity, but over the hill, to the east the Bugsworth, beds are found in the valley of the Goyt.

Bull Strang Section (No. 3).

About six miles to the south of Macclesfield, on the road to Swithamley, lies Clulow Cross, near which are some singular stones known by the name of the Bull Strang. On the north side of this place is a gravel hole, having a face exposed of about 30 feet of beds of well rounded gravel, composed of granites, greenstones, porphyries, silurians, mountain limestones, coal measure rocks, and a few chalk flints, parted by beds of brown sand. In all the beds numerous fragments of shells are found, which Mr. Sainter cannot distinguish from those found by him in the Macclesfield Cemetery beds, including the *Cytherea chione* and *Cardium rusticum*, and amounting to 53, as enumerated in Mr. Darbishire's catalogue, besides 10 or 12 more species which Mr. Sainter considers to be new. The elevation of the locality is probably between 1,300 and 1,400 feet above the level of the sea, and the area occupied by this sand and gravel extends over several acres, and could be traced from a little above the Macclesfield Road to the gravel pit; but it is much greater in thickness, so far as it is exposed, for it cannot at present be seen resting upon any other deposit, on the north end of the hill, where the face of 30 feet is seen.

Mr. Sainter was so kind as to point out the section to me, and to him we owe its discovery. This section, which is most probably at an elevation equal to that on Moel Tryfaen, affords, according to that gentleman, many of the shells found at Macclesfield some 900 feet lower.

Higher up the hill than the gravel pit are seen some large boulder stones (No. 1), several of them being upwards of a ton in weight.

Vale Royal Section (No. 3).

This interesting section, for the discovery of which we are indebted to Mr. Prestwich, is found about three miles due east of Macclesfield, on the turnpike road to Buxton, in Vale Royal, below Walker Barn. It is exposed in an old gravel pit, which has been wrought for the repair of roads, and occupies the end of a knoll lying between two little valleys, in which flow small streams of water. The lowest part of the deposit is not exposed so as to allow us to see on what it rests. Higher up the hill scattered boulders (No. 1) are seen lying on the surface.

By the kindness of Mr. Sainter, the following section was obtained :—

	Ft.	In.
1. Surface soil (black mould)	1	0
2. Ferruginous clays, gravel and small boulders..	6	0
3. Red sand	0	6
4. Alternate beds of small gravel and drifted shale	4	6
5. Loamy sand	3	0
6. Drifted shale and gravel, with small boulders, and a few fragments of shells	2	8
7. Sand and loam	7	6
8. Coarse sand, with boulders and pebbles	2	0
9. Gravelly clay, with a few boulders	3	0
10. Dark sandy gravel, containing shells in plenty. Depth not ascertained	2	0
	32	2

In this locality nearly all the Macclesfield Cemetery Shells, including the *Cytherea chione* and *Cardium rusticum* have been obtained by Mr. Sainter. The elevation of the beds, which lie over the Yoredale rocks, is about 1,200 feet above the sea.

Bugsworth Section (No. 3.)

An interesting section is exposed in the valley of the Goyt, above Bottoms Hall, Derbyshire. In going along the road from that place to Bugsworth a cutting is seen on the north side which shews a section of about 25 feet of beds of well rounded gravel, composed of granites, porphyries, greenstones, silurians, mountain limestones, coal measures, and a few chalk flints, all well rounded, and capped by a deposit of brownish coloured till, with angular stones in it, of from 4 to 5 feet in thickness. A few small fragments of shells were met with in the gravel, but their genera could not be recognized.

Above the section last described, and in the cutting of the Midland Railway, just before the latter enters the tunnel, is seen a face of 40 feet of well rounded gravel, parted by beds of brown sand, very similar to the deposits below, previously described. They have a dip to the south. A few flints and small fragments of shells were also met with. The main valley of the Goyt runs here nearly north and south, and the Bugsworth valley enters it from the east. The gravel has been removed, if it ever was there, across the Bugsworth valley, but it makes its appearance again on the south side towards Whaley Bridge, and is also seen by the side of the turnpike road leading from the last named place to Chapel-en-le-Frith. The height of this deposit, at the entrance of the tunnel, is about 500 feet; much lower than the elevation of the three last described sections.

Arnfield Section (No. 4).

This was exposed in making the Hollingworth (Cheshire) Reservoir, belonging to the Corporation of Manchester, and is in the Etherow Valley a little to the west of Glossop. It was first seen in cutting the goit from the Arnfield Brook to the reservoir. A few years since, in company with Mr. Prestwich, the writer examined the deposit, which consists of a gravelly till, containing plenty of foreign rocks, four to five feet in depth of which were exposed. It evidently lies on the top of the thick bed of till which occupies the lower part of the valley of the Etherow, that was exposed in making the new reservoir below Tintwistle. In it marine shells were found in considerable abundance. Amongst others there were *Turritella communis, Fusus, Banffius, Purpura lapillus,* two species of *Tellina, Cardium edule, C. aculeatum,* and *Cyprina islandica.* The elevation of the deposit is 568 feet above the level of the sea. It lies on the extreme western edge of a deep valley between two ranges of hills, those of Staley bounding the western, and those of Hadfield the eastern sides, each about 1,500 feet in height, and is in a direct line nearly 50 miles from the Irish Sea. This gravelly till, although more clayey in character, appears to be very similar in other respects to the upper bed seen in the sections of Bull Strang, Vale Royal, and Bugsworth.

Concluding Remarks.

It has not, to my knowledge, been hitherto noticed that the high level drift beds (No. 3) in the counties of Chester, Derby, and Lancaster, which have all the appearances of ancient shingle beaches, and look as if they had never been disturbed since they were deposited, so far as yet examined contain chalk flints, although such flints are commonly found in the gravels of the Isle of Man.

The gravel of Bull Strang must be between 1,300 and 1,400 feet above the level of the sea, and consequently about the same height as the beds on Moel Tryfaen, which are 1,350 feet high. It is also clear that the fossil shells found there and at Vale Royal at 1,200 feet, are nearly of the same description as those discovered by Mr. Sainter in the Cemetery beds at Macclesfield, at a level of 500 feet, and probably with those at Bugsworth and Arnfield hereinbefore described. Mr. Darbishire, in his second Memoir, previously quoted at p. 6, says, "A very short inspection of the (Macclesfield) specimens will satisfy those who see them side by side that the Macclesfield series precisely correspond, as to their geological and zoological facies, with the Moel Tryfaen and Blackpool fossils, and may fairly rank with them." To the localities before named may now probably be added those at Bugsworth and Arnfield.

The following is a section of the drift beds perforated at the North Cheshire Brewery, kindly supplied to me by Mr. Sainter :—

	Ft.
Sand and gravel	33
Fine sand	4
Sand and gravel	4
Brick clay	18
Upper boulder clay of the Geological Survey	13
Gravel with pebbles	6
Gravel with boulders and pebbles	11
Fine gravel, containing fragments of marine shells	4
Gravel and clay	5
Fine sand	6
Coarse gravel	2
Clay and gravel	14
Brick clay	7
Sandy clay with pebbles containing shells	4
Lower boulder clay with large boulders	12
Gravelly clay with pebbles resting upon the pebble beds of the Trias	5
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These two boulder clays are placed according to the classification of the Geological Survey, but although it is very convenient to have an upper and a lower boulder clay and pack in all the sands and gravels between them, it cannot be done, for there are at places 3 or 4 boulder clays divided by sands and gravels. Certainly only two are to be found here; but there are two series of sands and gravels, the higher one being above the upper boulder clay. In this section probably the cemetery beds are represented by the higher sands and gravels.

None of the sections hereinbefore described, except that at Bakstondale, are actually seen down to the underlying rock, but it is probable that they will all be found to be similar in that respect when excavated to a sufficient depth.

The gravel beds described in this communication have, doubtless, been formed under nearly similar conditions, but at different times to those at Macclesfield. However, we are still at a loss for a theory which will satisfactorily account for all the drift phenomena found between these higher levels, and the 50 miles of country intervening betwixt them and the sea, of which the North Cheshire Brewery beds at Macclesfield afford a comparatively simple section.

Ordinary Meeting, January 10th, 1871.

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

The PRESIDENT, in the name of Mr. BERNARD HARTLEY GREEN, of this city, solicitor, presented to the Society another memorandum book of one of the original members of the Society, Mr. George Walker. In it is some curious information as to the postal system and the cotton trade a century since. As to the former, it is stated that the packet for North America is dispatched from the post office in London the first Wednesday in every month. N.B. No postage to pay with letters *in*. The mails to all parts of Europe are dispatched from the General Post Office every Tuesday and Friday night at 12 o'clock, so that letters must go into the office at Manchester on Wednesday morning and Saturday night. "Novr. 1774, Sent a Letter to Messrs. Anderson and Lothian in Glasgow, by express, Manchester to Glasgow, by way of Wetherby, Newcastle and Edinburgh, 309 miles.

Paid for 309 measured miles at 3d. per mile	£3	17	3
Paid for 20 stages		10	6
„ for sending off..		2	6
	4	10	3

Express to London at 3d. per mile	2	5	9
14 stages at 6d.		7	0
Sending off at Manchester		2	6
	2	15	3

Sep. 1772, G. W. sent an express from London to Manchester, and paid £3 5 6

This express was delivered in Glasgow in about 66 hours. The express to London went in 36 hours.

On the motion of Mr. SPENCE, seconded by Mr. BROCKBANK, it was resolved unanimously—That the thanks of the Society be given to Mr. Green for his interesting and valuable donation.

Mr. T. T. WILKINSON, F.R.S., &c., communicated the following:—

In Mr. Binney's "introductory remarks" to his "Notes, on some of the High Level Drifts," (Proceedings, vol. x. pp. 66–8.), he has given references to several memoirs and papers on the subject, ranging in dates from 1831 to 1869. He has, however, made one or two omissions which I now wish to supply, inasmuch as the first paper mentioned was omitted from the index to the fourth volume of the *Transactions of the Manchester Geological Society*. In No. V., pp. 108–113 of that volume, I published an account of "The Drift Deposits near Burnley," which, when read, gave rise to a discussion occupying pages 113 to 120. This was followed by "Additional Notes on the Drift Deposits in Burnley and the Neighbourhood," which occupies pages 65 to 73 of the fifth volume of those *Transactions*. Several of the sections contained in these papers lie much higher (750 feet) than that in which the late Mr. Whittaker found his chalk flint (440 feet), and may therefore properly be classed as high level drifts. I have since found flints on the top of Entwistle Moor, at least 1,100 feet above the sea, and the drift occupies still higher elevations in the neighbourhood of Boulsworth. Large masses of sand, occasionally discoloured by carbonaceous matter, occur all over this district at elevations not exceeding from 400 to 600 feet above the level of the sea. These are most probably ancient sea-margins, or current-deposits, belonging to the period when extensive denudation was taking place in what is now the East Lancashire basin.

The PRESIDENT said that he was quite aware of Mr. Wilkinson's observations, having been present at the reading of his paper, but he did not then state that he had found chalk flints and shells near Burnley. When he (the President) came to treat on the Lancashire drift generally he should avail himself of Mr. Wilkinson's researches.

Professor REYNOLDS described the effects of an explosion of a copper cylinder forming part of the hot water apparatus at his house, and pointed out the dangers to be apprehended from such cylinders in frosty weather.

"Notes on the Effects of Cold upon the Strength of Iron," by WILLIAM BROCKBANK, F.G.S.

The severity of the present winter has brought the question of the effects of low temperatures upon the strength of iron, very prominently before the public, and it is a curious circumstance, that a subject of so great importance should have escaped the attention of writers on iron, to such an extent, as that it is either ignored, or dismissed with a few brief remarks or inconclusive experiments, which leave the subject altogether unsettled.

After referring to the observations and experiments on the effects of low temperatures on the strength of cast and wrought iron, in the works of Sir W. Fairbairn, Dr. Percy, and David Kirkaldy, and pointing out the inconclusiveness of all the experiments hitherto recorded, the writer went on to detail the following experiments, which he had, by the kindness of the several parties named, caused to be made during the severe frosts which have recently prevailed; and which have in all cases been carried out with the greatest care and exactness.

Experiments on the transverse strain of cast iron bars were made at the works of Messrs. P. R. Jackson and Co., of Salford, and were repeated thrice, with the following results :—

In Mr. Fairbairn's experiments only one sort of pig iron was employed. It is now well known that a much sounder, and more regular casting, can be obtained by a judicious admixture of several suitable kinds of pig iron; a very probable source of error would thus occur in Mr. Fairbairn's experiments, and this will probably point to the unsatisfactory results he obtained.

The bars employed in the present experiments were made from a mixture of four pig irons of the highest class, added to some good scrap iron; they were all poured from the same ladle, and were moulded from the same model, and they were remarkably regular in size and quality, so that the results may be fairly relied on. The castings were all made on Friday, the 30th of December last, and the bars were tested on the following Tuesday, January 3, 1871. The machine used was a powerful lever or steel yard, the bars having a three feet bearing, and the results were taken with all possible care, and are detailed in the following table.

Experiments upon the transverse strain of cast iron at low temperature, made at the works of Messrs. P. R. Jackson and Co., Salford, January 3, 1871, by W. Brockbank, F.G.S.

The mixture of metals was Cleator Hematite, Ponty Pool cold blast, Blaenavon cold blast, and Glengarnock hot blast pig iron, with some good scrap iron. All the bars cast from one ladle.

No.	Size of Bar.	Deflection.	Breaking Weight.	Average.	Temperature.	Remarks.
1	3ft. by 1in. by 1in. between the supports.	0·625 in.	790 lbs.	825 lbs.	About 26° Fah.	Exposed to frost in the open yard.
2		0·562 ,,	840 ,, } 842·5		,, ,,	
3		0·687 ,,	845 ,,		,, ,,	
4		0·687 ,,	820 lbs.	845 lbs.	32° Fah.	Left in the sand in the Foundry.
5		0·687 ,,	850 ,,		,, ,,	
6		0·687 ,,	865 ,,		,, ,,	
7		0·812 ,,	950 lbs.	950 lbs.	45° Fah.	
8		0·812 ,,	945 ,,		120° to 130° ,,	This bar was warmed.
9		0·812 ,,	945 ,,		45° ,,	

The results show a gradual and considerable decrease of strength in the bars, with the increase of cold below the freezing point. They also lost their elasticity in a similar degree.

A further trial was made at Messrs. Jacksons' works with similar bars and the same admixture of metals, January 10th, as detailed in the following table. One bar was cooled to a temperature of 15° by a mixture of snow and salt.

No.	Size of Bar.	Deflection.	Breaking Weight.	Average.	Temperature.	Remarks.
1	3ft. long between Bearings by 1in. by 1in.	0·6875 in.	780 lbs.	780 lbs.	15° Fah.	
2		0·75 in.	815 lbs.	844·3 lbs.	35° Fah.	
3		0·76 ,,	840 ,,		35° ,,	
4		0·8125 ,,	878 ,,		35° ,,	
5		0·75 in.	845 lbs.	859·25 lbs.	52° Fah.	
6		0·6875 ,,	855 ,,		52° ,,	
7		0·8125 ,,	867 ,,		52° ,,	
8		0·8125 ,,	870 ,,		52° ,,	
9		0·88 in.	893 lbs.	893 lbs.	70° Fah.	

These experiments are borne out by the general experience of ironfounders, many instances having come to my knowledge during these investigations, a few examples of which may be cited.

(1) I find it a matter of general opinion that pig-iron breaks much more easily in frost than in ordinary temperatures, and that breakages of castings are much more frequent in frosty weather.

(2) In rolling mills, and particularly where chilled rolls are employed, especial care has to be used in frosty weather to warm the rolls before using them, and when in use to keep them carefully covered from the frosty air. If not properly protected and carefully managed they are found to be very liable to fracture.

(3) Mr. Edgar Gilkes, of Middlesborough, informs me that the cast iron wheels of the Chaldron wagons of the Stockton and Darlington railway are found to fracture very frequently in frosty weather, and in a severe frost it is sometimes quite a serious matter.

(4) Messrs. Peel, Williams, and Peel had a remarkable example on January 5th (20° F.). A hydraulic cylinder had been cast upon a cast iron hollow core bar 7 inches in diameter and 1¼ inches thick, coated with 1½ inches of loam and hay. It was put out in the yard to cool during the severe frost, and when they came to draw the core bar it broke by the mere torsion, and was found to be quite brittle. A portion of this core bar was warmed, and it was then found to have recovered its nature and to be quite strong and tough. The lowest temperature on this date was 19° Fahrenheit, and the casting was expossd to it for many hours. Numerous other examples could be readily furnished if required.

There can therefore be no doubt whatever that the strength of cast iron is very materially lessened by severe cold.

For experiments in wrought iron I am indebted to many friends, and the results are of similar import. My first experiments were directed to the method adopted by Mr. Kirkaldy, and I soon found that neither by torsion nor gradual tensile strain could the true result be ascertained, *as the bar almost immediately became heated under the strain, and the effects of frost at once disappeared.* The following experiments made by Mr. William Johnson, of the Messrs. Johnson's Ironworks, Bradford, near Manchester, will illustrate this conclusively.

A coil of galvanised wire $5\frac{1}{2}$ B.W. gauge was left in the open air for 24 hours during severe frost, December 24, 1870; 24 pieces 1 yard in length each were then cut off. Of these 6 were tested for tensile strength by the direct application of weights, and 6 for torsion — the same tests were used for the remaining 12 after they had been warmed to about 80°. The results were as follows :

	Tension.		Torsion.	
	At 20°	At 80°	At 20°	At 80°
	2142 lbs.	2142 lbs.	$16\frac{1}{2}$ twists	$14\frac{1}{2}$ twists.
	2114 ,,	2058 ,,	$15\frac{1}{2}$,,	$14\frac{1}{2}$,,
	2114 ,,	2086 ,,	9 ,,	$13\frac{1}{2}$,,
	2142 ,,	2086 ,,	$14\frac{1}{2}$,,	$14\frac{1}{2}$,,
	2114 ,,	2128 ,,	16 ,,	$12\frac{1}{2}$,,
	2114 ,,	2086 ,,	$18\frac{1}{2}$,,	14 ,,
Total	12740 ,,	12586 ,,	90 ,,	$83\frac{1}{2}$,,
Average	2123·3 lbs.	2097·6 lbs.	15 twists.	13·9 twists.

Thus, in both experiments, the iron tests worse when warm than when frozen. In each case the wire immediately became warm. Mr. F. Monks, of the Whitecross Wire Works, Warrington, also tested wire rods for me with precisely similar results. Finding these experiments to be unsatisfactory, I arranged for a series to be tried by the more rough and ready method of the striker's hammer, which I judged would be more likely to show the true state of the iron in its frozen condition. The result either of gradual torsion or tension is to expel the frost there may be in the bar almost immediately, so that in the further progress of the trial there is no difference between bars which were originally either cold or warm.

If low temperatures have any influence in rendering iron weaker or more brittle, the only way in which the amount of such influence can be realised is by a *sudden impact*, and the striker's hammer was the readiest appliance for the purpose. In the following experiments great care was taken that the blows should be as nearly as possible of the same force in each trial, and as the experiments were all carefully conducted, and are vouched for by the parties named, they may be fairly relied on as representing truly the facts of the case.

(1) William Bouch, Esq., C.E., Engineer of the Stockton and Darlington Railway, made the following experiment December 29th, 1870, the temperature at the time being about 26°, but it had been as low as 19° over night.

A bar of round iron, 1½in. diameter, of best quality, was taken from the yard, being then coated with ice; it had been exposed to a week's hard frost. It was held over the edge of a smith's anvil, and one blow from a 12lb. hammer by the striker, broke a piece, 4in., long short off, the fragment flying twelve yards along the floor of the workshop. The same bar was then put into the mouth of a furnace, but not in contact with any flame, for a short time, to unfreeze it. The heat received into the bar was so moderate that a smith could grasp it with his hand. It was then allowed to lie on the floor for some time, until it had quite cooled down to the temperature of the workshop. It was now placed on the anvil, and the same striker as in the first experiment, with the same hammer, gave fourteen blows without causing the slightest fracture, the bar being merely bent about two inches. Mr. Bouch adds that he has, in his experience, met with many cases nearly as convincing as the above.

(2) Mr. Robert Peel, of Messrs. Peel, Williams, and Peel, Manchester, has kindly made for me the two following experiments with boiler plate iron, as shown by the samples now on the table, viz.:—

No. 1. A strip of boiler plate, of best best quality, was taken from the open yard, where it had lain during several days of severe frost, January 5th, 1871, temperature about 20° Fah.

It was laid across the anvil, and a striker, with a single blow of a 14lb. hammer, broke off the piece now exhibited.

The fracture shows a very "short" crystalline face, without any appearance of fibre, and is torn and irregular, in remarkable contrast to the sample No. 2, which is from the same piece, viz.:—

No. 2. The remainder of the above strip was slightly warmed to dispel the frost, and then allowed to cool to the temperature of the shop. It required several blows from the same hammer, and bent considerably before breaking, being exceedingly tough and fibrous.

The fracture shows a good fibrous structure, except on the inner side of the curve, where there is a thin crystalline skin.

The difference of appearance in these two fractures is very striking and remarkable, and can only be accounted for by the action of extreme cold.

No. 1*a*. This experiment was made on January 6th, temperature about 26° Fah. A strip of Low Moor best best boiler plate was taken out of the snow, having lain there during several days of intense frost. It was laid across the anvil, and broken off short with a single blow from a 14lb. hammer. The fracture is fibrous, but with patches of crystals, especially on the edges of the plate; the general appearance is "short" or "tender," very different from the usual character of Low Moor iron in its normal state.

No. 2*a*. The remainder of the same strip was placed in the drawing office at a temperature of 70°, and allowed to lie there for some hours. It then required six blows from a 14lb hammer, the plate being reversed each time, the grain being thus severely bent backwards and forwards, under heavy blows, before it severed. The outer skin still remained in cohesion, and it had to be separated by bending backwards and forwards in the smith's hands. This fracture shows a splendid quality of iron, the fibre being bent in both directions as the blows were alternately reversed. There is a slight crystalline line on the skin of one side.

Mr. F. Monks, of the Whitecross Wire Company, Warrington, has kindly made the following experiments with wire billets, which are the very toughest form of iron manufactured. The wire exhibited is made from one of the same bars, and will clearly show the quality of the iron.

The billets are 1¼ inch square, being in the semi-manufactured form ready for the final heating and rerolling into wire. They had been lying in the open air several days during severe frost. Experiment tried January 1st, 1871, 10° lowest to 30° highest temperatures.

Three bars were broken in the open air. They failed to break with 22 blows with a 15lb. hammer. A small nick ⅛in. deep was then cut, with three light blows on a "set," in the top of each bar, and at another part of it, after which a single blow sufficed to break each bar.

The bars were then thawed and allowed to cool to the usual temperature, or about 70°. 22 blows were given to each as before, and the same nick was made on one side as nearly as possible like the frozen bars had been treated.

One bar then broke after 11 blows, one after 10 blows, and one after 6 blows.

The frosted bars are more crystalline, and show no signs of fibre; the other bars show a good amount of fibre, and are slightly crystalline in the fractures.

The following experiments with rails were made at the works of the Darlington Iron Company, November 30th, 1869.

The rails were taken promiscuously from a lot of 1,000, all supposed to be of the same quality, weight, and exact section. It had been found that the rails which were then in course of manufacture for the East Indian Railways at these works, and which were of a very high quality, failed to pass the required test in frosty weather, whereas in ordinary temperatures a failure was a very rare occurrence. The ten rails were accordingly selected to settle the question whether higher and lower temperature affected the strength of the rails. Four rails were heated up to 120° Farenheit; the other six were tested cold, the temperature of the atmosphere being about 26°.

Test of East Indian Railway Rails, 82lbs. per Yard, Nov. 29th, 1869, tested by a Falling Weight of 2,000lbs.; Centres of Support, 3 feet 6 inches apart.

No.	No. of Blows.	Height of Fall.	Permanent Set.	Temperature	Remarks.
1	1st blow 2nd ,, 3rd ,,	5ft. 0in. 5 0 7 0	7-16ths 3-4ths —	120 deg.	Not broken.
2	1st ,, 2nd ,, 3rd ,,	5 0 5 0 7 0	3-8ths 13-16ths —	Do.	Ditto.
3	1st ,, 2nd ,, 3rd ,,	5 0 5 0 7 0	3-8ths 13-16ths —	Do.	Ditto.
4	1st ,, 2nd ,, 3rd ,,	5 0 5 0 7 0	3-8ths 7-8ths —	Do.	Ditto.
5	1st blow 2nd ,,	5ft. 0in. 5 0	3-8ths broke	26 deg.	Broke with 2nd blow.
6	1st ,, 2nd ,,	5 0 5 0	3-8ths 5-8ths	Do.	Passed test.
7	1st ,, 2nd ,,	5 0 5 0	3-8ths broke	Do.	Broke with 2nd blow.
8	1st ,, 2nd ,,	5 0 5 0	3-8ths broke	Do.	Ditto ditto.
9	1st ,,	5 0	broke	Do.	Ditto with 1st blow.
10	1st ,, 2nd ,,	5 0 5 0	3-8ths broke	Do.	Ditto with 2nd blow.

At 120° all the bars stood two 5ft. blows and one 7ft. blow.

At 26° only one bar stood two 5-feet blows, three broke at the second 5-feet blow, and one at the first 5-feet blow.

At 60° all would probably have passed the test easily, many thousands having previously done so from the same lot.

It will therefore be seen that the results are in perfect agreement in all these experiments, showing that bar iron, boiler plates, wire billets, and rails are most materially weakened by the action of intense cold, losing all their toughness, becoming quite brittle *under sudden impact*, and having their structures changed from fibrous to crystalline.

Similar instances could be given in illustration of this in the daily practice of engineering. In large works the breakages of wrought iron are very considerable during frosts. Quarrymen find that their chains are very liable to fracture from the same cause; and doubtless the numerous accidents of failing tires in our railways may be attributable to it. In many cases however the contraction of iron must also be taken into account, as it is a serious item.

In conclusion, I think it cannot be doubted, after the above recital, that iron does become very much weaker, both in its cast and wrought state, under the influence of low temperatures. This subject is one of such paramount importance, that a careful series of investigations ought to be undertaken by one of our scientific bodies, to ascertain the precise nature of the changes which are thus shown to take place, as there is herein an item which materially affects the stability of all iron structures during frosty weather, and which has not hitherto been adequately recognised.

"On the Properties of Iron and Steel as applied to the Rolling Stock of Railways," by Sir WILLIAM FAIRBAIRN, **Bart., LL.D., F.R.S., &c.**

Dr. Joule communicated to me the discussion which took place at the last meeting of the Society, on the question of the effects of intense cold on steel tires. This enables me to refer to a series of experiments which had for its object the effects of various degrees of temperature on wrought iron. These inquiries are to some extent analogous to the cause of the recent accident which occurred on the Great Northern Railway, near Hatfield, by the breaking of a steel tire, which caused the death of a number of persons.

It has been asserted, in evidence given at the coroner's inquest, that the breaking of the steel tire was occasioned by the intensity of the frost, which is supposed to render the metal brittle, and of which this particular tire was composed This is the opinion of most persons, but judging from my own experience such is not the fact, and provided we are to depend on actual experiment, it would appear that temperature has little or nothing to do with it.

Some years since I endeavoured to settle this question by a long and careful series of experiments on wrought iron, from which it was proved that the resistance to a tensile chain was as great at the temperature of zero as it was at 60° or upwards, until it attained a scarcely visible red heat. To show that this was the case, and taking, for example, the experiments at 60°, it will be found that the mean breaking weight, in tons, per square inch, was in the ratio of 19·930 to 21·879, or as 1:1·098 in favour of the specimens broken at the temperature of zero.

The generally received opinion is, however, against these facts, and it is roundly asserted that the strength of iron and steel is greatly reduced in strength at a temperature below freezing. The contrary was proved to be the case in wrought iron plates, and assuming that steel follows the same law, it appears evident that we must look for some other cause than change of temperature for the late fracture of the tire on the wheel of the break-van of the Great Northern Railway.

In our attempt to investigate the cause of the failure it may be interesting to show how the experiments on wrought iron to which we have referred were obtained at various temperatures, and subsequently to give the results as found in the summary.

The immense number of purposes to which both iron and steel are applied, and the changes of temperature to which they are exposed, renders the enquiry not only interesting in a scientific point of view, but absolutely necessary to a knowledge of their security under the various influences of those changes, and when it is known that most of our metal constructions are exposed to a range of temperatures varying from the extreme cold of winter to the intense heat of summer, it is assuredly desirable to ascertain the effects produced by those causes on material from which we derive so many benefits, and on the security of which the safety of the public frequently depends. It was for these reasons that the experiments in question were undertaken, and the summary of results are sufficiently conclusive to show that changes of temperature are not always the cause of failure, as that which occurred near Hatfield on the Great Northern Railway.

That such is the fact, I may adduce several accidents of broken tires all of which occurred during the spring and summer months when the temperature was high. One of them occurred on the Lancashire and Yorkshire Railway in the summer of last year when the temperature was 50° to 60° above freezing. I could enumerate others in which the winter frosts had nothing to do with the fractures which ensued.

It might have been satisfactory to have shown the process by which the following results were obtained, suffice it to observe, that all the specimens were torn asunder with and across the fibre in oil and water baths, and those under the freezing point were made in a snow bath reduced to zero. The summary of results is as follows :—

SUMMARY OF RESULTS.

No. of the Experiments.	Temperature Fahr.	Breakage weight per square inch in lbs.	Breakage weight per square inch in tons.	REMARKS. Duration of strain in regard to fibre.
1	0°	49·009	21·879	With.
2	60°	40·357	18·001	Across.
3	60	43·405	19·377	Across.
4	60	50·219	22·414	With.
5	110	44·160	19·714	Across.
6	112	42·088	18·789	With.
7	120	40·625	18·136	With.
8	212	39·935	17·828	With.
9	212	45·689	20·392	Across.
10	212	49·500	22·098	With.
11	270	44·020	19·651	With.
12	340	49·968	22·307	With.
13	340	42·088	18·789	Across.
14	395	46·086	20·574	With.
15	Scarcely Red	38·032	16·978	Across.
16	Dull Red	30·513	13·621	Across.

From the above it will be seen that the plates from which these results are obtained are much stronger in the direction of the fibre than across it.

The above experiments are quite conclusive as regards the strength of wrought iron plates, till they approach red heat. At that temperature nearly one half the strength is lost; it becomes exceedingly ductile, and may be drawn to a considerable extent in the direction of the fibres before it breaks.

Another series of experiments were made on wrought iron bars, which indicated somewhat different results. In these experiments, the specimens from the same works attained the maximum of strength, and gave at the temperature of 415°, a resistance of 39·072 tons per square inch, and at zero, and 60° there were little or no differences, excepting in the case of temperature when the resistance was increased from 28·419 at zero and 60°, to 39 tons per square inch at 415°. This may, however, be accounted for from the increased manipulation of rolling where the fibre is drawn and elongated to a much greater extent than in plates. This does not, however, affect, to any great extent, the ratio of compression and extension as regards the effects of temperature, although I should be inclined to take the experiments on plates before that on bars, as analogous to the broken tire, which, it must be borne in mind, is without weld and perfectly homogeneous.

The danger arising from broken tires does not, according to my opinion, arise so much from changes of temperature as from the practice of heating them to a dull red heat, and shrinking them on to the rim of the wheels. This, I believe, is the general practice, and the unequal, and in some cases, the severe strains to which they are subject has a direct tendency to break the tires.

To show how easily this may be effected, let us suppose that a tire, two feet six inches or three feet diameter, is shrunk on to a wheel one-tenth of an inch larger than the tire, it then follows that the tire in cooling must be elongated to that extent, with a strain, equivalent to the force of the shrinkage, and calculated to produce that amount of molecular disturbance. It may be more or it may be less, but supposing the strain to be one-half or three-fourths of that which would break the tire, it then follows that the constant action of its irregular motion on the rails must ultimately lead to fracture.*

I am not surprised that this should be the case, as most, if not the whole, of railway tires, excepting those on engines and tenders, are not turned but selected by hand, heated and shrunk upon the wheels with every degree of tension, as suits the convenience of the workman. So long as this process is pursued, the public will be exposed to the risk of broken tires.

* From long continued action under strain, it has been proved that it is only a question of time when rupture takes place as repeated increased and diminished changes with the same load ultimately leads to fracture.

What is required in this description of manufacture is, that the rim of the wheel and the inside of the tire should be *turned to a standard gauge*, accurately calculated to give the required amount of tightness with a larger margin of strength, and this done we should attain greatly increased security to the public, and a great saving in wear and tear— to say nothing of the large sums expended by companies in the shape of compensation for injuries and loss of life.

"On the Alleged Action of Cold in rendering Iron and Steel brittle," by J. P. JOULE, D.C.L., F.R.S., &c., Vice-President.

As is usual in a severe frost, we have recently heard of many severe accidents consequent upon the fracture of the tires of the wheels of railway carriages. The common-sense explanation of these accidents is, that the ground being harder than usual, the metal with which it is brought into contact is more severely tried than in ordinary circumstances. In order apparently to excuse certain Railway Companies, a pretence has been set up that iron and steel become brittle at a low temperature. This pretence, although put forth in defiance, not only of all we know of the properties of materials, but also of the experience of every-day life, has yet obtained the credence of so many people that I thought it would be useful to make the following simple experiments:—

1st. A freezing mixture of salt and snow was placed on a table. Wires of steel and of iron were stretched so that a part of them was in contact with the freezing mixture, and another part out of it. In every case I tried the wire broke outside of the mixture, showing that it was weaker at 50° F. than at about 12° F.

2nd. I took twelve darning needles of good quality, 3in. long, $\frac{1}{24}$in. thick. The ends of these were placed against steel props, $2\frac{1}{8}$in. asunder. In making an experiment, a wire was fastened to the middle of a needle, the other end being attached to a spring weighing machine. This was then pulled until the needle gave way. Six of the needles, taken at random, were tried at a temperature of 55° F., and the remaining six in a freezing mixture which brought down their temperature to 12° F. The results were as follow:—

Warm Needles.	Cold Needles.
64 oz. broke.	55 oz. broke.
65 " "	64 " "
55 " "	72 " "
62 " "	60 " bent.
44 " "	68 " broke.
60 " bent.	40 " "
Average $58\frac{1}{3}$	Average $59\frac{5}{6}$

I did not notice any perceptible difference in the perfection of elasticity in the two sets of needles. The result, as far as it goes, is in favour of the cold metal.

3rd. The above are doubtless decisive of the question at issue. But as it might be alleged that the violence to which a railway wheel is subjected is more akin to a blow than a steady pull; and as, moreover, the pretended brittleness is attributed more to cast iron than any other description of the metal, I have made yet another kind of experiment. I got a quantity of cast iron garden nails, inch and quarter long, and $\frac{1}{8}$in. thick in the middle. These I weighed, and selected such as were nearly of the same weight. I then arranged matters so that by removing a prop I could cause the blunt edge of a steel chisel, weighted to 4lbs. 2oz., to fall from a given height upon the middle of the nail as it was supported from each end, $1\frac{1}{16}$in. asunder. In order to secure the absolute fairness of the trials the nails were taken at random, and an experiment with a cold nail was always alternated with one at the ordinary temperature. The nails to be cooled were placed in a mixture of salt and snow, from which they were removed and struck with the hammer in less than 5″.

Up to Series 10, each set of sixteen nails was made up of those of the previous set which were left unbroken, added to fresh ones to make up the number.

Series 1. Temperature of eight cold nails 10°. Of eight warm 36°. Height of fall of hammer 2 inches.

Result. No nails broke.

Series 2. Temperature of eight cold nails 14°. Of eight warm ones 36°. Fall of hammer 2½ inches.

Result. No nails broke.

Series 3. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 3 inches.

Result. One cold nail broke. No warm one broke.

Series 4. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 3½ inches.

Result. Two cold nails broke. One warm one broke.

Series 5. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 4 inches.

Result. One broke of each sort.

Series 6. Temperature of eight cold nails 0°. Of eight others 38°. Fall of hammer 4½ inches.

Result. One broke of each sort.

Series 7. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 5½ inches.

Result. No cold nail broke. One warm nail broke.

Series 8. Temperature of eight cold nails 2°. Of eight others 40°. Fall of hammer 6½ inches.

Result. Two cold nails broke. One warm nail broke.

Series 9. Temperature of eight cold nails 2°. Of eight others 40°. Fall of hammer 7½ inches.

Result. Three cold nails broke. Three warm nails broke.

Series 10. Experiment with the ten left in the last. Temperature of five cold nails 2°. Of the five others 40° Fall of hammer 8½ inches.

Result. Two cold nails broke. One warm nail broke.

Series 11. Experiment with the six left from the last

Temperature of three cold nails 3°. Of the other three 40°. Fall of hammer 10 inches.

Result. Two cold nails broke. Three warm nails broke.

Series 12. Experiment with fresh nails. Twelve cooled for four hours to 3°. Twelve others 41°. Fall 7 inches.

Result. Seven cold nails broke. Eight warm nails broke.

The collective result is that 21 cold nails broke and 20 warm ones.

The experiments of Lavoisier and Laplace, of Smeaton, of Dulong and Petit, and of Troughton, conspire in giving a less expansion by heat to steel than iron, especially if the former is in an untempered state. Such specimens of steel wire and of watchspring as I possess expand less than iron. But this, as Sir W. Fairbairn observed to me, would in certain limits have the effect of strengthening rather than of weakening an iron wheel with a tire of steel.

The general conclusion is this—Frost does *not* make either iron (cast or wrought) or steel brittle, and that accidents arise from the neglect of the companies to submit wheels, axles, and all other parts of their rolling stock to a practical and sufficient test before using them.

"On the Effect of Cold on the Strength of Iron," by Peter Spence, F.C.S., &c.

In the conversation at the last meeting of the Society on one of the causes of railway accidents, namely, the breaking of the tires of the carriage wheels, there was a general expression of opinion that the reduction of temperature during frost had the effect of reducing the strength of iron, and that this was the proximate cause of these occurrences. Dr. Joule, on the other side, stated that however general the impression might be, he knew of no experiments that tended to prove that impression to be a correct one.

It seemed to me that a few experiments on cast iron could without much difficulty be made, and which might set the matter at rest one way or the other.

I therefore decided on having some lengths of cast iron made of a uniform thickness of ½in. square, from the same metal and the same mould; these I obtained after a good deal of trouble, on account of the moulders being off work at new year's time, and this must be my excuse for not being able to give due notice of my communication.

Two of the four castings I got seemed to be good ones, and I got the surface taken off, and made them as regular a thickness as was practicable.

I then fixed two knife-edged wedges upon the surface of a plank, at exactly nine inches distance from each other, with an opening in the plank in the intervening space, the bar being laid across the wedges a knife-edged hook was hung in the middle of the suspended piece of the bar, to the hook was hung a large scale on which to place weights.

The bar was tried first at a temperature of 60 Fahr.; to find the breaking weight I placed 56lb. weights one after another on the scale, and when the ninth was put on the bar snapped. This was the only unsatisfactory experiment, as 14 or 28lbs. might have done it, but I include it among the others. I now adopted another precaution, by placing the one end of the plank on a fixed point and the other end on to a screw-jack, by raising which I could, without any vibration, bring the weight to bear upon the bar. By this means, small weights, up to 7lbs., could be put on while hanging, but when these had to be taken off and a large weight put on, the scale was lowered to the rest, and again raised after the change was made. I may here state that a curious circumstance occurred twice, which seems to indicate that mere raising of the weight, without the slightest apparent vibration, was equal in effect to an additional weight. 3¾cwts. were on the scale, a 14lb. weight was added, then 7lb., then 4lb., 2lb., 1lb., and 1lb., making 4cwts. and 1lb. This was allowed to act for from one to two minutes, and then lowered to take off the small weights, and replaced by a 56lb., intending to add small weights when suspended, raised so imperceptibly by the screw, that the only way of ascertaining that it was suspended was by looking under the scale to see that it was clear of the rest. As soon as it was half-an-inch clear it snapped, thus breaking at once with one pound less than it resisted for nearly two minutes.

Six experiments were carefully conducted at 60° Fah. the parts of the bars being selected so as to give to each set of experiments similar portions of both bars: the results are marked on the pieces. My assistant now prepared a refrigerating mixture which stood at zero and the bars were immersed for some time in this, and we prepared for the breaking trials to be made as quickly as could be, consistently with accuracy, and to secure the low temperature each bar on being placed in the machine had its surface at top covered with the freezing mixture. The bars at zero broke with more regularity than at 60°, but instead of the results confirming the general impression as to cold rendering iron more brittle they are calculated to substantiate an exactly opposite idea, namely, that reduction of temperature *cæteris paribus* increases the strength of cast iron. The only doubtful experiment of the whole twelve is the first, and as it stands much the highest, the probability is that it should be lower; yet, even taking it as it stands, the average of the six experiments at 60° Fah. gives 4cwt. 4lbs. as the breaking weight of the bar at that temperature, while the average of the six experiments at zero gives 4cwt. 20lbs. as the breaking weight of the bar at zero, being an increase of strength from the reduction of temperature equal to 3·5 per cent.

Mr. Wm. Radford, C.E., asked if Mr. Brockbank had considered what the effect must be upon iron used in Russia, Sweden, Norway, and Denmark, for if the theory which was sought to be established were true, the tires of railway wheels in those countries must fly to pieces in winter; as far as his experience went in Denmark such had not been the case on the Copenhagen Railway.

Ordinary Meeting, January 24th, 1871.

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

Mr. BROTHERS, F.R.A.S., exhibited a drawing from the fine photograph of the solar Corona, taken by him at Syracuse, during the late total eclipse of the Sun.

Mr. W. B. JOHNSON, C.E., gave an account of two cases of very narrow escapes from serious accident to railway trains, in consequence of the present faulty system of arrangement of the points or switches.

DR. JOULE, F.R.S., &c., read the following letter, dated January 21st, 1871, which he had received from Mr. WILLIAM H. JOHNSON, of Bowdon.

"Since the last meeting of the Philosophical Society I have made some further experiments on the 'Effect of cold on the strength of Iron.'

In these I have maintained a nearly fixed temperature, and thus avoided to a great extent the error occasioned by the rise in temperature, consequent on sudden torsion.

January 11th. A piece of a charcoal wire rod, ·237 of an inch diameter, gave the following results:—

	1st.		2nd.		3rd.
At about 40° F.	20 twists	...	19 twists	...	17 twists.
Adjacent 6″ at temperature of melting zinc	10 twists	...	9 twists	...	$7\frac{1}{2}$ twists.
			4th.		5th.
Twisted very slowly, surrounded by salt and snow			$19\frac{1}{4}$ twists	...	16 twists.
Adjacent 6″ at about 40° F.			15 twists	...	

The twisting under salt and snow was performed so slowly, each experiment lasting a quarter of an hour or more, that the temperature cannot have been affected by the torsion. The same care was taken at the temperature of 40° F.

The great diminution of strength at the melting point of zinc is remarkable.

I take the liberty of communicating these results to you, as unfortunately I shall be away at the next Meeting, and thus shall not have an opportunity of seeing you."

Mr. BROCKBANK remarked that these experiments did not affect the conclusions stated in his paper, read at the last meeting. He believed that the strength of Iron under torsion was most affected by the heat developed by the twisting, and that the cooling mixture employed by Mr. Johnson would have the effect of making the wire stand a greater number of twists by counteracting the excessive heat produced by the torsion.

Mr. BROCKBANK, F.G.S., exhibited a drawing of the machine used by him in his experiments on the strength of Cast Iron at different temperatures.

"Experiments on the Oxidation of Iron," by Professor F. CRACE CALVERT, Ph.D., F.R.S., &c.

Some two years since, Sir Charles Fox inquired of me if I could give him the exact composition of iron rust, viz., the oxidation found on the surface of metallic iron. I replied that it was admitted by all chemists, to be the hydrate of the sesquioxide of iron, containing a trace of ammonia; to this, he answered, that he had read several books on the subject, in which the statements referring to it differed, and from recent observations he had made, he doubted the correctness of the acknowledged composition of iron rust. He further stated that if he took a bar of rusted wrought iron, and put it in violent vibrations, by applying at one end the fall of a hammer, scales would be separated which did not appear to him to be the substance I had described.

This conversation induced me to commence a series of experiments which I shall now detail. I first carefully analysed some specimens of iron rust, which were procured, as far as possible, from any source of contamination. Thus one of these samples was supplied to me by Sir Charles Fox, as taken from the outside of Conway Bridge, the other secured by myself at Llangollen, North Wales. These specimens gave the following results when submitted to analysis:

	Conway Bridge.	Llangollen.
Sesquioxide of Iron	93·094	92·900
Protoxide of Iron	5·810	6·177
Carbonate of Iron	0·900	0·617
Silica	0·196	0·121
Ammonia	Trace.	Trace.
Carbonate of Lime		0·295

These results clearly show the correctness of Sir Charles Fox's foresight, that the composition of the rust of iron is far more complicated than is stated in our text books. Therefore the question may be asked, is the oxidation of iron due to the direct action of the oxygen of the atmosphere, or to the decomposition of its aqueous vapour; or does the very small quantity of carbonic acid which it contains determine or intensify the oxidation of metallic iron? To reply to it I have made a long series of experiments, extending over two years, and which I hope will throw some light on this very important question.

Perfectly cleaned blades of steel and iron having a gutta percha mass at one end, were introduced in tubes which were placed over a mercury trough, and by a current of pure oxygen conducted to the top of the experimental tube, the atmosphre was displaced, and it was then easy to introduce in these tubes traces of moisture, carbonic acid, and ammonia,

After a period of 4 months the blades of iron so exposed gave the following results:—

Dry Oxygen	No oxidation.
Damp "	In three experiments only one blade slightly oxidised.
Dry Carbonic Acid	No oxidation.
Damp "	Slight appearance of a white precipitate of the iron, found to be carbonate of iron. Two only out of six experiments did not give these results.

Dry Carbonic Acid and Oxygen...No oxidation.

Damp Oxygen and Carbonic Acid { Oxidation most rapid, a few hours being sufficient. The blade assumed a dark green colour, which then turned brown ochre.

Dry Oxygen and Ammonia.........No oxidation.

Damp „ „ ....No oxidation.

The above results prove that under the conditions described, pure and dry oxygen does not determine the oxidation of iron, that moist oxygen has only feeble action; dry or moist pure carbonic acid has no action, but that moist oxygen containing traces of carbonic acid acts most rapidly on iron, giving rise to protoxide of iron, then to carbonate of the same oxide, and last to a mixture of saline oxide and hydrate of the sesquioxide of iron.

These facts tend to show that carbonic acid is the agent which determines the oxidation of iron, and justifies me in assuming that it is the presence of carbonic acid in the atmosphere, and not its oxygen or its aqueous vapour, which determines the oxidation of iron in common air. Although this statement may be objected to at first sight, on the ground of the small amount of carbonic acid gas existing in the atmosphere, still we must bear in mind that a piece of iron, when exposed to atmospheric influences, comes in contact with large quantities of carbonic acid during 24 hours.

These results appeared to me so interesting that I decided to institute several series of experiments.

When perfectly clean blades of the best quality of commercial iron are placed in ordinary Manchester water they rust with great facility, but if the water is previously well boiled and deprived of oxygen and carbonic acid, they will not rust for several weeks. Again, if a blade of the same metal is half immersed in a bottle containing equal volumes of pure distilled water and oxygen, that portion dipping in the water becomes rapidly covered with the hydrate of the peroxide of iron, whilst the upper part of the blade remains for weeks unoxidized; but if a blade be placed in a mixture of carbonic acid and oxygen, a very different chemical action ensues, as not only that portion of the blade dipping in the water is rapidly attacked, but the upper part of it immediately shows the result of chemical action, and also the subsequent chemical re-actions are greatly modified by the presence of the carbonic acid. For in this case that portion of the blade is only covered with a film of carbon, together with a dark deposit, composed of carbonate of the protoxide and hydrate of the sesquioxide. The fluid, instead of remaining clear, becomes turbid.

These series of experiments substantiate the interesting fact observed—that carbonic acid *promotes* oxidation.

A long series of experiments were also made to try and throw some light on the curious fact, first published by Berzelius, subsequently studied by other chemists, and well known to soap and alkali manufacturers, namely, that caustic alkalies prevent the oxidation of iron; my researches can be resumed as follows:—

1st. That the carbonates and bicarbonates of the alkalies possess the same property as their hydrates; and

2nd. That if an iron blade is half immersed in a solution of the above-mentioned carbonates, they exert such a preservative influence on that portion of the bar which is exposed to an atmosphere of common air (oxygen and carbonic acid), that it does not oxidize even after a period of two years.

Similar results were obtained with sea water, to which had been added carbonates of potash and soda.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 9th, 1871.

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

"On *Carex flava* L., and its allies, of the Manchester Flora," by Charles Bailey.

Some discussion having taken place at a recent meeting of the section, in regard to the distribution of the *Carex flava* group in this district, I present the following notes upon the matter, illustrating them by a large suite of specimens.

The prevailing form in the district, and one very common to the south of Manchester, is the *Carex lepidocarpa* Tausch.; this is the *C. Œderi* Sm., and of Grindon's Manchester Flora, and the *C. flava* var. *β* of Buxton's Guide. The true *C. flava* (*a. genuina* E.B.), as stated long ago by Mr. Buxton, is nowhere met with in the district. Specimens of *C. Œderi* Ehrh., from Mere Mere, the locality mentioned in Buxton's Botanical Guide, were recently exhibited at a meeting of the Society, and the sandhills at Southport are, so far as I know, the only other locality in the neighbourhood for this species.

There is some confusion in the nomenclature of the group, and the characters given in our standard authority—English Botany, 3rd edition—do not altogether dispel it. In that work, Dr. Syme describes *C. eu-flava*, *β. lepidocarpa* as usually having the male spikes sessile or subsessile, and the female spikes as being all approximate, or the lowest a little remote when its stalk is said to be wholly included within the sheath. The Manchester plant however has the male spike stalked, the peduncles being often of great length, while the female spikes are scarcely approximate, but rather scattered, and the lower spike is generally produced, its stalk being conspicuously exserted. The fruits are more narrowed at the base than represented in "English Botany," and the bracts are very long, much exceeding the male spike.

There are two forms of *C. lepidocarpa* Tausch. in the district; the more common one, which occurs in fields and open ground, has the leaves as long as or longer than the somewhat thick and rigid stems, but the latter are without the roughness at the summit described by Grenier and Godron, in their Flore de France; the fruit is slightly inflated, and the beak long but straight. The single specimen which I possess of Billot's No. 2159 (Fl. Gall. et. Germ. exsicc.)

closely approaches this form, but it is less rigid, and has only a single spike of fruits.

The other form, occurring in damp ground amongst long grass, is much taller and more slender than that just named; its stems exceed the leaves, and the fruit is less inflated, so as to be gradually attenuated into a beak. Some plants of this form, which I collected at Oakmere, Cheshire, and at Whaley Bridge, Derbyshire, near the reservoir, agree very well with the plant issued in Wirtgen's Herb. plant. select., Fasc. VI., No. 287, the chief difference being that the Rhenish plant has the beak more recurved.

Billot's specimens of *C. flava* L., from the fosse of the citadel of Strasbourg (No. 2158), quoted by Dr. Syme as synonymous with his var. *a. genuina*, do not quite agree with any Scotch or north English plant which I have gathered or seen. Dr. Syme describes the female spikes of *genuina* as not contiguous, but they are all contiguous in the Strasbourg plant, while the leaves are rather longer than the stems, and the lowest bract greatly exceeds the male spike—the contrary being stated in E.B. to be the case.

It may be mentioned that Godron, in the Fl. de France. t. III., p. 424, like Dr. Syme, divides *C. eu-flava* into var. *a. genuina* and *β. lepidocarpa*, the former having approximate, and the latter slightly scattered spikes, while the var. *a. genuina* of E.B. has the spikes not contiguous, and *β. lepidocarpa* all approximate. The plants of the north of England which I have examined agree better with Godron's characters.

The figure of *C. Œderi* Ehrh., given in "English Botany," No. 1674, very accurately represents the plants of Mere Mere and Southport, which also agree with Belgian specimens published in Van Heurck's "Herbier des plantes rares ou crit.," No. 189. But Dr. Syme quotes Billot's plant (Fl. Gall. et. Germ. exsicc., No. 1352) as identical with this species, whereas the specimens in my set differ greatly from the E.B. plate and description. In Billot's plant the male spikes are on long stalks, while the female spikes are widely separated from each other, and are not as spreading as they are represented in "English Botany"; the fruits also differ in not being abruptly narrowed or inflated, and the beak, instead of being short and straight, as in the Manchester plants, is somewhat long and slightly recurved. It is worth noticing, as bearing upon the specific distinctness of this plant, that M. Crépin, in his "Manuel de la Flore de Belgique," mentions that it is remarkable in its shoots, putting forth every year new tufts of leaves and new stems,—which I understand to mean that fresh stems appear simultaneously with the new leaves, instead of the stems being produced from the tufts of the preceding season, as in most sedges.

Mr. SIDEBOTHAM said that this group of plants was in considerable confusion, some botanists classing all together, and scarcely noticing the different forms even as varieties; others, both British and Continental, whilst distinguishing the forms, were by no means agreed as to the nomenclature.

Mr. Sidebotham exhibited a large series of each of the plants from various localities, and gave it as his opinion that they were three distinct species, not difficult to separate, even in their extreme forms, and he extended to all three the remark of Professor Syme, in the new edition of "English Botany," where he says, that although it might sometimes be difficult at first sight to distinguish the species, when a dried specimen only was seen, he had never found the least difficulty when the plants were growing.

The following short characters were, he thought, quite sufficient to separate the species from each other.

Carex flava. Fruit yellow, nuts large, beak very long, deflexed.

Carex lepidocarpa. Fruit pale green or yellowish green, nuts smaller and beak shorter than in *C. flava*, beak straight.

Carex Œderi. Fruit pale yellow, nuts very much smaller than preceding, and more globular, beak very much shorter, straight.

Mr. Sidebotham had never gathered *Carex flava* in the Manchester district, although abundant in the north of Lancashire, and he reported *Carex Œderi* as occurring abundantly at Llandudno.

Ordinary Meeting, February 7th, 1871.

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

"On the Organisation of an Undescribed Verticillate Strobilus from the Lower Coal Measures of Lancashire," by Professor W. C. WILLIAMSON, F.R.S., &c.

The author directed attention to the existing state of knowledge in reference to internal structure of the organisms long known as Volkmanniæ, pointing out the publication, 1st, of one form, by Mr. Binney, subsequently described also by Mr. Carruthers from Mr. Binney's sections, and 2nd, of a second type published by himself. He then proceeded to describe a third type from a specimen discovered in the lower coal measures by Mr. J. Butterworth. This is an oblong strobilus of a lax and slender habit. Its central axis consisted of a bundle of vessels the transverse section of which was a triangle with concave sides and truncated angles. This was surrounded by a broad cylinder of delicate cellular tissue, which again was enveloped by an outer cylinder of prosenchymatous cellular tissue of a dense character. At each node this latter tissue extended outwards as a thick continuous disk, which, at a little distance from the central axis, became subdivided into a peripheral circle of stiff prosenchymatous bracts, the flattened extremities of which stretched upwards and outwards. The upper part of each undivided disk gave off a large number of slender sporangiophores, many of which ran along the upper surfaces of the disks and bracts to reach the more peripheral sporangia. These sporangia were large and conspicuous—those belonging to each segment being arranged nearly in a plane parallel to the disk—and in four irregular concentric circles. Each sporangium appears to have been attached to the disk by a separate sporangiophore. The spores were very numerous and perfectly orbicular, but their minute organisation, like that of the cells and vessels of the organism, was masked by the mineralisation which it had undergone, being preserved in a highly crystalline carbonate of lime. The author then proceeded to examine the probable affinities of the several forms of strobilus of which the structure is now known. One, which he previously described in the Memoirs of the Society he assigns to Calamites. The other two, viz., that originally figured by Mr. Binney and that now described, he believes to belong to the Annularian forms of vegetation. Two varieties of verticillate foliage have most probably been confounded under the names of Asterophyllites and Annularia—the one being that of the Calamitean plants, the other belonging to the genera of Asterophyllites and Sphenophyllum, and it is to one or the other of these two genera that the strobilus now described, as well as that figured by Mr. Binney, appear to belong. The structure of their central axes is what the author chiefly relies upon in arriving at these conclusions. The name of *Volkmannia Dawsoni* is provisionally proposed for this new strobilus, in honour of the distinguished Principal of M'Gill College at Montreal, and in recognition of his valuable elucidations of Canadian phytology.

"The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena, Part II.," by Professor OSBORNE REYNOLDS, M.A.

In the paper which I read before this society, on the 29th of November last, I endeavoured to show that it is probable that these phenomena are a species of that action known as the electric brush taking place in the medium which fills space, be it ether or simply gas, or both. The reasoning I made use of was, essentially *a fortiori*. I pointed to the fact that the electric brush as seen in the Geissler tubes exhibits similar appearances, and that at the times of greatest display on the part of comets and the aurora similar conditions are present, such as a change in the action of the sun, conditions which, to say nothing more, are favourable to electric disturbance. I purposely avoided all attempts to explain how the brush may be produced, feeling that it was sufficient to point to the aurora, which is universally admitted to be electrical, as a proof that such phenomena do exist even if we cannot explain how. This proof, however, is perhaps not quite satisfactory. In order that it may be complete, the other phenomena must be produced in the same way as the aurora, and this, although possible, is not necessary. An assumption which is commonly made respecting the phenomena of the aurora cannot be made with respect to the others. This assumption assigns the two magnetic poles of the earth as the two electrodes between which the electric discharge takes place, which forms the aurora borealis and the australis. If this assumption be maintained, some other explanation must be found for the manner in which electricity may form the tails of comets and the corona. It is quite clear that the tail of a comet cannot be due to a discharge between two electrodes situated on the comet itself. In the same way, from the position occupied by the corona, it can hardly be due to electricity passing between two electrodes on the sun. In fact, if a comet's tail is electrical, it is due to a discharge of electricity of one kind or another from the comet, which for the time answers to one of the electrodes only. The same may be said of the corona and the sun. If we could observe the aurora from a point distant from the earth, it is very probable that we should find the same to be the case, but whether this would be so or not, an assumption has been made as to the cause and nature of the aurora, which will answer just as well for the corona and comet's tails: it is, that the sun acting by evaporation or otherwise, causes continual electric disturbance between the earth and its atmosphere, the solid earth being negatively charged and the atmosphere positively, and that the aurora is the reunion of these electricities taking place in the atmosphere.

Now as has been already said, this assumption will serve for the comets and the sun as well as for the aurora. If there is a continual electric disturbance between the sun and the medium in which it is placed, so that the sun becomes negatively and the medium positively charged, the reunion of these electricities would form the corona. It must not be supposed that I assume the sun to be a reservoir of electricity which it is continually pouring into space. I consider that the supply of electricity in the sun is kept up by some physical action going on between the sun and the medium of space, whereby the sun becomes negatively charged, and the medium positively.

This may be well illustrated by reference to the common electrical machine: here the motion of the glass against the rubber causes the glass to become positively and the rubber negatively charged; and these electricities do not unite instantly there and then, but remain and accumulate in the respective bodies, until collected and brought together again by the conductor.

Assume then, that the sun is in the position of the rubber, while the ether is in that of the glass: then the corona corresponds to the spark or brush which leaves the conductor. On the same assumption the negative electricity of the comet would be more and more set free by the inductive action of the sun as the comet approached it, and would also be driven off by induction in a direction opposite to that of the sun; and combining with the positive electricity in the ether would form the tail of the comet, in a manner anologus to that in which a negative spark is given off by the lid of the electrophorus.

I think that a rational account may in this way be given of the manner of the electrical action to which I have attributed these phenomena, but I do not consider that the probability of the truth of this electrical hypothesis depends on the value of such an explanation. It is an assumption based on the manner in which it fits into its place, and explains the appearances presented by these beautiful phenomena.

Since this paper was written, my attention has been called to the fact that Mr. Richard Proctor has published views of these phenomena, which somewhat resemble mine. He attributes them in part to electricity and in part to meteors. There is however this fundamental difference between our views, that he considers the tails of comets as consisting of cometary matter, the difficulty of conceiving which was the origin of these speculations. Moreover, I can conceive no electric discharge between two meteors without a medium between them, and if there is a medium, why is there any necessity for meteors? If, as I see good reason to suppose, gas, when glowing with electricity, reflects or scatters rather than absorbs light of the wave-length which it radiates, that portion of the coronal light, which is polarized and assumed to be reflected, will be accounted for. I think that the recent observations have confirmed the probability of these speculations, inasmuch as they have confirmed the facts on which these speculations were based. There is one point which has not been already noticed, but which seems to me to be of some importance.

If the corona be an electric discharge, the electricity will be continually carrying off some of the elements of the sun into space where they will be deposited and condensed. May not this stream of matter be the cause of the existence of small meteors, and supply the place of those which continually fall into the larger bodies?

"Further Experiments on the Effects of Cold upon Cast Iron," by PETER SPENCE, F.C.S., &c.

In resuming these experiments upon the effects of cold on cast iron, it is not necessary for me to say that I was led to resume them from the apparent undecisiveness of all the experiments brought before the Society some time ago, my own being included in that category, none of them being so free from possible sources of error as to be fitted for finally settling the matter.

In the experiments which I have now to bring before the Society I have limited my aim to a single point, namely, as to whether the reduction of temperature has any, and if so, what effect on cast iron in regard to its powers of resisting transverse strain either of weight or pressure, and it appears to me that if this point can be satisfactorily settled it will go a long way in settling the other points now in dispute.

As my object, in showing that I have in these experiments eliminated as far as seems possible all sources of error, will be best effected by minute detail, you will excuse anything that may seem trifling. As I was not trying the absolute strength of any sort of cast iron I did not see the force of Mr. Brockbank's objection to my using ½in. bars instead of the orthodox 1in. bars. I could obtain ½in. bars equally good castings, and the machinery for breaking them was more manageable and in my opinion more exact.

Messrs. Rye, Son, and Ogden, of Newton Heath, kindly undertook to make for me 50 bars, each 3ft. long by ½in. square, all out of one ladle, and of No. 3 Glengarnock pig and Kirkless Hall common pig—I name these although it does not seem of importance; all I wanted was good, sound, clean, and equal castings; and, knowing the purpose for which they were intended, with great care they turned them out so good that not one of those sent to me was rejected. I now cut each of these bars into three lengths of 1ft. each, and as they were cut they were thrown into a heap making nearly 150 pieces. They were now taken and all their ends covered with paint, in order that the new fracture might be examined as they were broken. The heap was then brought into the laboratory, having thus had three chances of perfect mixing. A boy of 11 years of age now handed me the pieces singly from the heap, and as I received them I placed them alternately one by one in two lots, until I had got 70 pieces in each lot. One of these was now taken and put into a cask capable of holding 2cwt. to 3cwt. of freezing mixture composed of pounded ice and chloride of sodium (which instantly reduces the temperature to zero), and being surrounded with sawdust, they were kept there for nearly 48 hours.

The other 70 were now put into water at 70° Fah, and this was done chiefly in order that they might be broken wet, as those would necessarily be when taken out of the freezing mixture.

The mode of breaking was this:—I put a bar on the suspending wedges, then hooked on the weight scale, and with a number of weights much under the breaking load, raised the loose end of the plank by the screw jack so as to bring the weights to bear. I now added single pounds or 2lb. weights till 15lb. were put on, these were then taken off and a 14lb. weight was placed and single pounds again put on, thus regularly adding till the bar snapped; I then recorded the breaking weight, my assistant meantime putting on another bar. I spent nearly eight hours in breaking these 70 bars, and every one got an equal amount of care.

On opening up the freezing mixture 44 hours after enclosing it, I found it in perfect condition, little solution and no increase of temperature having taken place. The bars were taken into the laboratory in small lots and immersed in another freezing mixture, from which they were withdrawn singly with pliers. Having seized one piece with too firm a grasp I found that my fingers grew white and produced an intense pain as if burned. Some of the freezing mixture was spread on each bar by a spatula while on the the machine, so that every one was broken at a temperature within one or two degrees of zero. The mode of breaking was exactly similar to that employed with the other lot, and equal care was given to every bar. This I can affirm, as every one of them was broken by myself, and all entries made by myself.

The results are before you, and to me it was a matter of surprise, when both sets were completed and added up, to find that they almost exactly corroborated my previous experiments, which I do not think were fallacious in their character, but merely defective in their not covering a sufficient amount of ground to give certainty to the result. I have however so much confidence in those now detailed, that I have no hesitation in giving it as an ascertained law, that a specimen of cast iron having at 70° Fah. a given power of resistance to transverse strain, will on its temperature being reduced to zero have that power increased by 3 per cent.

Breaking weight of ½in. square, cast iron bars, 9in. between points of suspension at 70 deg. Fahr.

	Cwt.	Qrs.	Lbs.		Cwt.	Qrs.	Lbs.
				Bt fd.	129	2	21
No. 1	4	0	14	No. 36	3	3	14
2	4	3	26	37	4	2	24
3	3	2	2	38	4	1	1
4	3	0	14	39	4	1	14
5	3	3	16	40	3	3	12
6	3	2	14	41	4	0	14
7	3	2	10	42	3	0	14
8	3	0	0	43	4	0	6
9	3	3	0	44	3	2	1
10	3	1	1	45	4	0	0
11	3	1	14	46	4	0	27
12	3	1	14	47	4	0	22
13	3	1	24	48	3	0	2
14	3	0	14	49	3	3	14
15	3	3	0	50	4	1	8
16	2	3	14	51	4	1	15
17	4	2	8	52	4	0	24
18	4	1	1	53	3	0	5
19	3	1	0	54	3	2	27
20	3	3	20	55	4	3	0
21	4	1	0	56	3	2	4
22	3	0	12	57	4	0	12
23	4	1	0	58	4	0	14
24	3	3	14	59	4	0	0
25	4	0	0	60	4	1	1
26	3	2	14	61	4	0	18
27	3	1	18	62	4	0	12
28	3	2	22	63	4	0	11
29	4	0	14	64	4	0	4
30	3	3	1	65	3	3	10
31	4	0	26	66	3	1	18
32	3	3	8	67	3	1	7
33	4	2	7	68	3	2	6
34	2	2	14	69	4	3	0
35	3	2	1	70	4	1	0
Ford.	129	2	21		268	3	18

Breaking weight of ½in. square cast iron bars, 9in. between points of suspension at Zero.

	Cwt.	Qrs.	Lbs.		Cwt.	Qrs.	Lbs.
				Bt.fd.	139	1	22
No. 1	4	1	15	No. 36	3	2	25
2	4	0	14	37	5	0	14
2	3	0	10	38	4	0	4
4	3	0	6	39	3	3	4
5	2	3	20	40	4	1	15
6	3	3	18	41	3	0	12
7	3	1	12	42	3	1	0
8	4	2	14	43	4	2	8
9	4	0	22	44	3	3	22
10	4	1	15	45	3	2	0
11	4	0	14	46	4	2	1
12	4	2	1	47	4	1	1
13	3	1	26	48	4	0	4
14	4	0	4	49	4	0	3
15	3	2	8	50	4	2	14
16	4	3	0	51	4	0	12
17	4	1	15	52	3	0	18
18	3	2	1	53	3	2	8
19	4	1	15	54	4	0	15
20	4	2	1	55	4	1	12
21	4	2	24	56	3	1	13
22	3	2	26	57	4	1	26
23	4	1	1	58	2	3	10
24	4	1	26	59	4	1	26
25	3	3	12	60	3	2	20
26	3	0	14	61	4	0	0
27	4	1	15	62	3	2	24
28	3	0	12	63	3	3	15
29	4	2	14	64	3	0	20
30	3	2	15	65	4	0	14
31	3	0	22	66	4	0	4
32	4	3	13	67	3	0	20
33	4	1	14	68	3	2	12
34	4	0	13	69	4	2	1
35	3	2	18	70	4	0	1
Ford.	139	1	22		276	3	0

Fahr.	Cwt.	Qrs.	Lbs.
At 70 deg.	268	3	18
At Zero	276	3	0

Mr. Thomas Carrick called attention to the fact that the tabulated statement of Mr. Spence's experiments showed a maximum breaking weight of about 5cwt. and a minimum of about 3cwt. The mininum breaking weight was therefore 40 per cent less than the maximum. With experiments showing such an excessive range in the breaking weight of bars, which from the care taken in their production ought presumably to have been homogeneous in quality, it was very unsafe to rely upon a resulting difference of only 3 per cent derived from separately adding the breaking weights of each set together and comparing the gross results. The iron used was obviously of an inferior quality and quite unsuitable for the purpose of reliable experiments.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 30th, 1871.

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

Mr. BOYD DAWKINS, F.R.S., exhibited sections of the calcareous nodules from the Gannister coals of lower coal measures of Oldham, in which the intimate structure of the various forms of carboniferous vegetation were admirably preserved. He also brought before the notice of the Society a series of microscopical sections of coal prepared by Mr. Newton, in which the spores and sporangia present in all bituminous coals, and from which a large percentage of their bituminous properties was derived, were clearly to be seen. None of these minute bodies have been discovered in anthracite.

Mr. Boyd Dawkins, F.R.S., then called the attention of the Society to a series of fossils on the table in which the original matter of the hard parts of the living creature had been more or less removed, and replaced by various minerals which happened to be in solution in the matrix in which they were imbedded. Thus the *Trigonia Moretonis* of the Stonesfield slate was proved to be a mere cast in calcite of the space once occupied by the shell of the creature. The calcification of the ligament in *Cypricardia rostrata* and *Cardium Stricklandi* from the great oolite of Enslow Bridge (Oxon), and its identity of structure with the valves, showed also that the whole of the original hard parts had disappeared before their replacement by calcite. The same fact was shown to hold good in the case of the corals, which only show organic structure on the outside of the fossil. In some cases, however, the structure of the outer surface has been carried inwards by the petrifying material, as in a case of *Nuceolites dimidiatus* from the coral rag, in which the ambulacral pores and the shape of the angular plates composing the test were carried inwards to the centre of the calcareous spar which now fills the space occupied by the soft parts. Other specimens showed that the calcareous shell had been replaced by sulphide of iron, phosphate of lime, sulphate of baryta, or by silica. The hard parts of the vertebrata are better preserved in their original condition than fossil shells, from the insolubility of the phosphate of lime in the bones and teeth.

A comparison of the various substances of which fossils are composed leads to the conclusion that very little of the original matter of the hard parts is preserved, and that very generally the fossil is a mere cast of the original, filled with whatever mineral happened to be in solution in the stratum in which it is imbedded. In some cases the cast exhibits the minute structure of the original, as in the case of the Yorkshire hazel nuts in the Oxford Museum, in which the kernels have been converted atom by atom into calcite without the cellular arrangement of the original being disturbed, and without the shell being altered in any degree. The fact that our knowledge of animal life in past time depends principally on mere casts of the hard parts which happened to be imbedded in the strata demonstrates the truth of Mr. Darwin's view that the geological record is very imperfect.

Professor W. C. Williamson, F.R.S., exhibited some specimens of Stigmaria, and indicated their bearing upon views advanced even by the most recent writers on the subject. He demonstrated that the centre of the axis was occupied by a pith of delicate parenchyma, wholly devoid of the vessels described and figured by Goeppert, and which certainly never belonged to the part of the plant in which he figured them. The lenticular spaces long known to exist in the lignous zone surrounding the medulla, Professor Williamson showed to be true medullary rays, occupied by mural cellular tissue prolonged directly from the medullary parenchyma. Besides these, smaller or secondary medullary rays separate many of the individual laminæ of the vascular tissue. He then pointed out the true source of the vascular bundles, which proceed to the large cylindrical rootlets of the plant. The radiating series of vessels which are immediately vertical to each of the quincunctially disposed lenticular medullary rays are projected downwards for a short distance, like a tongue, into the lenticular spaces. Down to this point, the component vessels are disposed vertically, but they became suddenly deflected outwards, at right angles to their previous course, to reach the rootlets for which they are severally destined. The deflected vessels are very numerous, but the greater part of them disappear in succession, only a limited number finally constituting the bundle occupying the centre of each rootlet.

Professor Williamson pointed out the important bearing which these facts have upon the affinities of the Sigillaria of which Stigmaria is the root. He showed that not only the true Lepidodendra, but also the Lepidodendroid stems which Mr. Binney has described under the name of Sigillaria Vascularis, never could have belonged to the same plant as these Stigmarian roots. In the plants indicated the central or medullary axis is occupied by scalariform vessels intermingled with remarkable forms of scalariform cells, as already shown in the case of Lepidodendra by Mr. Carruthers, and which equally characterise the other plants referred to. It appears improbable, being contrary to all known facts, that the ærial stem should have such a structure, whilst in the roots its vascular scalariform tissues were replaced by cellular parenchyma of an altogether different type and character. The conclusion to be drawn from these observations is that we are yet as far as ever from all actual knowledge of the internal organisation of the Sigillariæ. For the two principal specimens from which the above conclusions were drawn, Professor Williamson was indebted to Mr. Whittaker, of Oldham, and for others of a similar kind to Mr. Butterworth, of Shaw.

"On the Cultivation of Madder in Derbyshire," by JOSEPH SIDEBOTHAM, F.R.A.S.

Several attempts have been made to cultivate madder in England and Ireland, but the records of the experiments are very meagre and unsatisfactory, and one can only judge their want of success from the fact that they are not repeated.

Being desirous of ascertaining the capabilities of our soil and climate for this branch of farming, and having suitable land at our disposal at Strines, Mr. Nevill and I determined to try the experiment, the results of which I have now the honour to lay before you.

In order to make the matter plain to those who do not understand the different qualities of madder, it is necessary to give a few words of explanation.

Madder is the root of *Rubia tinctoria*, by some authorities supposed to be a mere cultivated variety of a plant indigenous to this country, and found wild in many places in the limestone districts on rocks and walls. This plant is cultivated for the purpose of dyeing in many parts of Europe and in India.

Its qualities vary much; that from Holland, called Dutch Madder, will dye red, but not purple, and the colour is not fast; that from Italy, called Naples Madder, dyes good reds and purples, but the colour is also loose; that from Turkey, dyes good reds and purples, and is very fast; from France we get two qualities, called respectively rosés, from their dyeing beautiful reds and pinks, and Paluds, the latter being a name given because the roots are grown on marshy land. The latter yield, besides the fine reds, also a good purple, nearly allied to that produced by Turkey roots.

For the purpose of the experiment we selected a piece of rich land, near the river, at Strines, a little less than an acre, and having prepared it in the usual manner, we had it sown with seed from fine Palud madder, early in the Spring of 1868. The weather was unusually dry and the ground produced a crop of remarkably fine polygonum aviculare, which almost choked the young madder seedlings. (I am inclined to think the seed of this polygonum were mixed with the madder seed.) In the Autumn the madder plants came into flower, and the roots of some pulled up measured 13 inches in length.

The field was weeded, and the plants came up in the Spring of 1869, very strong and healthy, and so on until August, 1871, when we had them dug up. To produce the best results the roots should have remained another year in the ground, but for the purpose of our experiment this growth was considered sufficient. As to yield, the quantity produced was small, probably owing to the very dry season after sowing; in appearance and size the roots were about equal to fine French roots, but on breaking them, instead of the deep red colour in the best French roots these were orange, or yellow.

The dyeing properties were of a very disappointing nature: out of the dye the colours looked full, but on being cleared with soap they were found to be loose, and precisely in character like Dutch madder, the reds and pinks being weak and loose, and the purple element entirely wanting. From a single experiment it would be unwise to do more than hazard an opinion, as more extended experiments might lead to other results; but I think it probable that the deficiency of colouring matter in these English madder roots is owing to a deficiency of sun and heat. It would not be easy in this country to select a more likely soil for the purpose than that at Strines, and the seed was obtained from a district where the best quality of French madder is grown.

It is said to be a fact that French seed when sown in Holland does not produce a French quality of roots, but one similar in every way to the usual Dutch madder. This, if correct, would support my opinion.

I have here to illustrate this subject, specimens of the various madders in the root and ground state, also the colours produced by each, and the relative degree of fastness, exhibited by portions of each being subjected to boiling soap.

Ordinary Meeting, February 21st, 1871.

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

"The Overthrow of the Science of Electro-Dynamics," by JOHN HOPKINSON, D.Sc.

In science no theory should be considered unquestionable and no man's work held sacred from attack, and our scientific periodicals should afford the freest scope to discussions no matter how hostile to established notions. Still it is evident that the journals ought not to publish everything that may come to hand; they should at least take care that a hostile critic understands the meaning of what he criticises.

Two papers appeared last month in the "Quarterly Journal of Science" and the "Chemical News" respectively, in which the author (the Rev. Mr. Highton) somewhat summarily disposes of the science of Thermodynamics, fancying he has disproved the equivalence of heat and work. I will only trouble you with one or two quotations with a view to support my opinion that the papers in question ought never to have been permitted to appear in any journal pretending to scientific position.

In the "Chemical News," p. 42, we find, speaking of Joule and Scoresby's experiments on electro-dynamic engines—"They say that 'the quantities of zinc consumed' (that is, respectively, when the engine is at rest and doing work) 'being as a to b, $(a—b)$ represents the quantity of heat converted by the engine into useful mechanical effect.' Therefore, since on the supposition of a mechanical equivalent of heat a grain of zinc consumed equals 158 foot pounds, if $x =$ pounds raised a foot high per consumption of a grain of zinc in the battery,—

$$x = \frac{(a-b)\ 158}{a}$$

Hence the authors draw the conclusion:—'Therefore when b vanishes, or becomes infinitely small, the economical duty is a maximum.' Certainly this is a most startling result; that the maximum of work should be done when no zinc at all is consumed." The last sentence is a misstatement of the conclusions of Joule and Scoresby's paper, in which (Philosophical Magazine, vol. 28, p. 451) it is stated that "the economical duty will be a maximum when b vanishes or becomes infinitely small in comparison with a. In this case $x=158$, while the power of the engine will become infinitely small with regard to work performed in a given time." Comparing the phrases 'economical duty' and 'maximum of work,' as he uses them, he evidently confuses the duty of an engine with the whole work done by it.

A little further on we have—"They calculate the maximum theoretical power of a grain of zinc to be 158 foot pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than $\frac{1}{4}$ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a-b)$ used to produce work W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electro-magnets passed $\frac{1}{8}$ of an inch from them, doubtless W would be greater, but so also would $(a—b)$, and it does not follow that $\frac{W}{(a-b)}$ with which we are concerned would be at all changed. What becomes then of the dogmatic assertion that the duty of a grain of zinc could be increased tenfold ?

Now let us turn to the paper in the "Quarterly Journal." Here we may find enough in one article for our present purpose, taking chap. II. Art. 2. — "Why are we forced to suppose . . . . . . that the same amount of fuel produces the same amount of energy, whether it is consumed in the steam engine, the horse . . . . . . the gnat ? At any rate, we may observe that the very phrase is certainly a misnomer, and a misnomer of such a kind as to have a fatal effect in producing a false conception of things. For mechanical energy just as often produces cold as heat; it may produce either heat or cold, or neither. In fact, as a general rule, though with notable exceptions, every pushing or compressing force produces heat, and every pulling or expanding force cold. Place a weight on a pillar, and the weight produces heat in the pillar; hang it on a wire and it cools the wire." "In exactly the same way, in a fire-syringe use force to press down the piston, it produces heat—heat enough to kindle tinder; but use the same force to pull up the piston, and it produces cold." Surely this is enough to show that the author's notions of what he is attacking are, to say the least of it, shallow; for what he quotes as paradoxes are simple deductions from the two laws of Thermodynamics. That a wire is cooled by stretching follows from the fact that heat expands it. In the case of the fire-syringe the case is simpler. The working body is the air in the syringe; on pulling up the piston this air does work, and therefore uses up heat and is cooled. Mr. Highton seems to imagine that because the arm of the experimenter does work, it is done *on the air in the syringe*, whereas this column of air and the observer are really co-workers in raising the air *external* to the cylinder.

To point out all the fallacies of these papers in detail would take too mueh of your time. My object was to show that if the "Quarterly Journal of Scienee" and the "Chemical News" are to represent scientifie opinion with any degree of truth, they would do well to use a little discretion as to what they print.

"Remarks on Mr. Spence's Experiments on the Effeets of Cold on the Strength of Cast Iron," by JOSEPH BAXENDELL, F.R.A.S.

In eoncluding his paper read at the last Meeting of the Soeiety, Mr Spence stated that "he had so much confidence in the experiments then detailed, that he had no hesitation in giving it as an ascertained law, that a speeimen of cast iron, having at 70° Fah. a given power of resistance to transverse strain will, on its temperature being reduced to zero, have that power increased by 3 per cent." Now, in physical investigations it is often very hazardous to rely too mueh on the simple means of sets of experiments or observations, however numerous, unless the theory of errors has been employed to test their value; and in the inquiry as to the effeet of cold on iron, this remark applies with peculiar force.

Mr. Carriek has objected to Mr. Spence's experiments that the differenees between some of the breaking weights are very large; and also that the iron used was of an inferior quality; but the quality of the iron, unless it is actually very bad, is a matter of seeondary importanee, since its only effeet will be to increase the range and diminish the average of the breaking weights; and with respeet to the wide differences between some of the results, this is more than eompensated for by the number of the experiments which is sufficiently great to afford the means of determining approximately the law of error to whieh they were subject, and thus of ascertaining whether the final results are entitled to the high degree of confidenee whieh Mr. Spenee has placed in them. When, however, I ran my eye over the columns in Mr. Spenee's table after the reading of his paper, it at onee struck me that the differenees of the individual breaking weights from the mean values in both sets of experiments, when calculated out, would be found to indicate a law of error, differing considerably from the ordinary law of simple errors of experiments or observations, and that the mean value of the *minus* differences would be very sensibly greater than that of the *plus* differences. I therefore calculated the means of the two sets of experiments, and the differences of all the breaking weights from these means, and grouping these differences according to their order of magnitude, I projected the results on ruled paper, but instead of a tolerably regular curve having only one maximum I obtained a curve having two well marked maxima. It was therefore at once evident that some unsuspected condition or disturbing cause had operated during the experiments to produce an undue number of breaking weights considerably above, and also considerably below, the general average. The effect, in fact, was somewhat similar to that which would be produced by a series of throws of a number of dice, some of which were weighted on one side, while others were weighted on the opposite side. I concluded, therefore, that many of the bars used by Mr. Spence had their sides of very unequal strength, and that it depended upon the position in which a bar was placed when tested, whether its breaking weight would be high or low. With the strongest side of the bar placed downwards the breaking weight would be high, but with the weakest side downwards the breaking weight would be low. Either of the other two sides placed lowest would in general give a breaking weight of intermediate value. If in two sets of experiments A and B a greater number of bars happened to be placed with their weakest sides downwards in set A than in the set B, then the mean of A would be less than that of B; and this, in fact, appears to have actually taken place in Mr. Spence's experiments. Thus, if we divide the set of 70 experiments made at a temperature of 70° Fahr. into two sets of 35 each, the mean breaking weight of the first 35 is 3cwt. 2qr. 23lbs., and that of the second 35 is 3cwt. 3qr. 25·5lbs., the difference being 1qr. 2·5lbs., or 2½ times greater than the difference between the means of the two sets of 70 each made under a difference of temperature of 70° It is obvious, therefore, that Mr. Spence's experiments, though evidently made with great care, afford no certain evidence that any sensible change takes place in the strength of cast iron when its temperature is reduced from 70° to zero of Fahrenheit's scale.

As showing the little reliance to be placed, in certain cases, on results derived from short series of experiments, I may mention that in Mr. Spence's experiments, notwithstanding the very great diversity in the breaking weights of the bars used, and the care taken to mix them as much as possible before testing, there is in one case a run of eleven consecutive experiments in all of which the breaking weights are below the general average; while in another there is a run of eight in which the breaking weights are all above the average. Similar runs of six and five each occur several times. Facts like these will show to those who have little experience in the application of the theory of errors how necessary it is, in some inquiries at least, to multiply experiments as much as possible before proceeding to deduce results and draw conclusions. Taking all the experiments on the effect of cold on iron which have yet been brought before the Society, they can only be regarded as indicating that if any effect at all is produced, it is more apparent on iron of good quality than on inferior iron, but that its amount is so small as to be wholly inadequate to account for the railway and other accidents which have been attributed to it.

Mr. Brockbank stated that at the time he entered upon the experiments communicated to the Society, he had no knowledge of those made by Mr. Knut Styffe of Stockholm, and C. P. Sandberg, A.I.C.E., of London, as detailed in the English translation of Mr. Styffe's work on the Strength of Iron and Steel. He was however pleased to find that the researches of these gentlemen confirmed the conclusions drawn from his own experiments; and he especially pointed out that in Mr. Sandberg's experiments on the Strength of Rails, the objection raised as to the hardness of the ground by frost was obviated, as the experiments were performed upon a solid granite rock *in situ*, and this could not be hardened by cold to any considerable extent so as to affect the results, and yet in these experiments the rails are shown "to exhibit only from one-third to one-fourth the strength at 10° Fahr. which they possessed at 84° Fahr."

Dr. JOULE observed that the admitted fact that the supports of the bars in Mr. Sandberg's experiments were in a different condition at the two temperatures rendered the results arrived at with them valueless as evidence on the question at issue.

"Further Observations on the Strength of Garden Nails," by J. P. JOULE, D.C.L., F.R.S., &c.

Since communicating the paper on the Alleged Influence of Cold in giving Brittleness to Iron, I have collated the results with cast iron nails in order to show the range of strength in such specimens.

Height of Fall of Hammer.	Percentage of Fractures.
2 inches	0
2½ "	0
3 "	6·25
3½ "	23·5
4 "	30
4½ "	36·4
5½ "	37·5
6½ "	48
7 "	62·5
7½ "	64·3
8½ "	75
10 "	92·8

I chose the garden nails for experiment after some thought, as presenting a marked variety of metal in contrast with the iron and steel wire, tempered and untempered. I did not expect them to possess great strength, but having found them to require a heavier blow than I expected to fracture them, I have had the curiosity to make some experiments on them which may be interesting to the Society.

I took pairs of the nails, placed them head to point parallel to each other so that pressure applied in the middle by pincers sufficiently forcibly would fracture one of them.

Paper slips were pasted on the edges of the nails, and their distances asunder measured by a microscope with micrometer eyepiece divided by lines corresponding to $\frac{1}{800}$ of an inch. Weights were gradually added to the lever of one arm of the pincers until fracture took place, which was always accompanied with a sharp report. The observed deflection or bending of the nails was taken continuously as the weights were laid on, and the calculation of what it would have been at the moment of rupture taken from the immediately preceding observations. The amount of deflection was almost exactly proportional to the weight laid on in each experiment.

No. of Experiment.	Length of Nail between Supports.	Breadth of Nail in Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. Lbs.
1	1·05	0·13	0·127	·0062	145·5
2	1·1	0·114	0·125	·0067	141
3	1·1	0·120	0·115	·0090	171
4	1·08	0·111	0·106	·0073	142·5
5	1·12	0·122	0·145	·0098	189
6	1·06	0·138	0·120	·0087	184·5
7	1·08	0·150	0·118	·0095	201
Average	1·084	0·1264	0·1223	·0082	167·8

If we compare the above with Mr. Brockbank's experiments we shall find, approximately, on reducing them to the dimensions he adopted, viz. 3 feet between supports and 1 inch section —

	Breaking Weight.	Deflection.
Mr. Brockbank's, with large bars...	860·7	·740
My own, with nails	2673·	1·106

The metal, in the form I used it, was therefore more than three times as strong as that of the large bars to resist a compressing and tensile force, while its extent of spring at the breaking weight was half as much again. Therefore, so far from being of inferior quality, it would sustain a very much heavier blow without fracture.

"On the Action of Sulphurous Acid on Phosphates," by Dr. B. W. Gerland. Communicated by Dr. R. Angus Smith, F.R.S., &c.

The abstract of this paper will appear in the next number of Proceedings.

Ordinary Meeting, March 7th, 1871.

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

"The Action of Sulphurous Acid on Phosphates," by Dr. B. WILHELM GERLAND, Macclesfield. — Communicated by R. ANGUS SMITH, Ph.D., F.R.S.

The researches which this paper describes, lead to the following conclusions :—

1. An aqueous solution of sulphur dioxide acts upon several phosphates, not by decomposing them, like other strong acids, but by combining with them forming, soluble compounds. Basic phosphates require from 4 to 6, and neutral phosphates 2 mols. of sulphur dioxide for solution. These solutions part less readily with their sulphur dioxide than the simple aqueous solution of the latter, and those of the neutral phosphates more easily than those of the basic phosphates. From some of these solutions the original phosphate can be again obtained, from others a less basic salt, but the decomposition in the solutions of this class does not proceed to the formation of phosphoric acid.

The following phosphates belonging to this class have been examined:

a. Tricalcium phosphate is abundantly soluble in water and sulphur dioxide. The concentrated solutions undergo a slow decomposition at temperatures above 18° C., and form besides calcium sulphite, dicalcium- and monocalcium-phosphate. Both concentrated and dilute solutions deposit mixtures of calcium sulphite and dicalcium hydric phosphate by addition of alcohol, by exposure in vacuum, or by boiling under reduced pressure. Boiling under atmospheric pressure on the other hand causes the formation of the new compound: tricalcium phosphate sulphite, $Ca_3 P_2O_8, SO_2, 2H_2O$, as a crystalline precipitate which is distinguished from the above mentioned mixtures of dicalcium phosphate and calcium sulphite by its great stability. It claims more general interest as being an active manure and disinfectant. The unusual composition of this substance made it desirable to prepare corresponding compounds of other metals, but all attempts in that direction have been unsuccessful.

Dicalcium hydric phosphate is readily soluble in water charged with sulphur dioxide. From the solution the original phosphate can be easily obtained.

b. Trimagnesium-, dimagnesium-, and magnesium-ammonium- phosphate are dissolved in large quantities by water charged with sulphur dioxide; the first two without decomposition, but if an excess of the latter has been used. dimagnesium hydric-phosphate is left undissolved. All these solutions have a great tendency to deposit dimagnesium hydric phosphate in crystals.

c. Tri- and di- manganese phosphate are very soluble in sulphur dioxide and water. Both solutions give crystals *in vacuô*, consisting principally of dimanganese phosphate, but by boiling, precipitates of trimanganese phosphate are formed.

d. Copper phosphate is soluble, although in smaller quantity, in an aqueous solution of sulphur dioxide without decomposition. The solution deposits at summer temperature in course of time crystals of cuprous and cupric-sulphite, and by boiling, cupric phosphate.

e. Uranium phosphate is very slightly soluble in water charged with sulphur dioxide. The phosphate of the original composition separates again from the solution after the removal of the sulphur dioxide.

f. Crystals of trisodium phosphate absorb sulphur dioxide in such quantity that it would suffice to convert all the sodium present into sodium hydric sulphite. However, alcohol separates sodium dihydric phosphate from the solution, and less than $\frac{5}{6}$ths of the sulphur dioxide are expelled by boiling. The concentrated solution obtained by saturating the crystals with the gas, shows the peculiar phenomenon of separating into two distinct liquids by gravitation; agitation unites these again to a perfectly homogeneous liquid.

2. Sulphur dioxide in aqueous solution has no action upon bismuth-, stannous-, stannic-, and metastannic- phosphate.

3. Sulphur dioxide and water act upon some phosphates in the same manner as other strong acids by forming a sulphite and phosphoric acid. The phosphates of barium, silver and lead have been observed to undergo this decomposition.

4. Calcium arsenite, calcium arseniate, and cupric vanadate are dissolved like the first group of phosphates, without decomposition by sulphur dioxide and water. The solution of the first forms calcium sulphite by boiling, the second begins soon to deposit calcium sulphate, owing to the reaction of arsenic acid on sulphurous acid, and the solution of the vanadate on boiling deposits beautiful golden colored scales, which are, probably, copper vanadite sulphite.

5. Calcium oxalate is dissolved, in very minute quantity, by water charged with sulphur dioxide, and is deposited unchanged after expulsion of the gas.

*** This paper was read at the Meeting of the Society, held on the 21st February, 1871.

"Further Observations on the Strength of Garden Nails," by J. P. Joule, LL.D., F.R.S., V.P.

The author thought it desirable to ascertain how far hardness had to do with the strength and elasticity of these small specimens of cast iron. For this purpose he plunged some of them at a heat near the melting point into water, then selecting those which had been hardened sufficiently to resist the action of the file. Others he cooled slowly from a bright red heat. The experiments were conducted in the manner described in the last number of the Proceedings.

	No. of Experiment.	Length of Nail between Supports.	Breadth of Nail at Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. Lbs.
Hard Nails.	1	1·0	0·11	0·122	·0067	129
	2	1·04	0·12	0·12	·0037	84
	3	1·0	0·12	0·122	·0028	81
	4	1·02	0·143	0·102	·0077	129
	5	1·1	0·138	0·13	·0071	203
	Average	1·032	0·1262	0·1192	·0056	125.2
Soft Nails.	6	1·0	0·112	0·117	·0088	141
	7	1·05	0·139	0·114	·0087	150
	8	1·02	0·130	0·138	·0051	176
	9	1·04	0·117	0·090	·0101	101
	10	1·04	0·121	0·108	·0073	113
	Average	1·03	0·1238	0·1134	·008	136.2

Reducing to a length of 3 feet and 1 inch square section, and making a deduction of $\frac{1}{6}$ from the deflections, on account of the taper of the nails, the above results, along with those in the last number of Proceedings, become

	Breaking Weight.	Deflection.
Nails in original state	2673	·922
Hardened ditto	2002	·677
Softened ditto	2448	·924

Dr. Joule exhibited three photographs of the sun taken on the 1st December, 1858. The images, ·43 in. diameter, were produced by the achromatic object-glass of a telescope with half-inch stop. Exposure, by means of an apparatus completely detached from the camera, during a small fraction of a second. He had been induced to examine them after seeing the beautiful photograph of the late eclipse by Mr. Brothers. On examining the three images a nebulosity is observed, very similar to that in Mr. Brothers's photograph In all three, taken at an interval between each of about a minute and a half, the nebulous appearance appears situated on three quarters of the limb, the remainder being quite free. There are also indications of a radial structure, so that he thinks it highly probable that the representations are actually those of the corona.

Since communicating the above, he has carefully examined the two other photographs of the sun which he possesses, and which were taken early in the month of November, 1858. These, one of which must have been exposed at about 2 hours 20 minutes after the other, present nothing remarkable to the naked eye; but when viewed through a glass of moderate power, a thin crescent-shaped envelope is observed on each, with this remarkable circumstance, viz., that in the two it appears on opposite limbs, suggesting the idea of a semi-revolution in the above interval of time at a velocity not much less than that due to Kepler's law of planetary motion. In one of the photographs there is, under the crescent and apparently on the rim of the sun itself, a narrow band in breadth about $\frac{1}{300}$ of the diameter of the disk, and of at least double the intensity of the sun. This may probably be referred to the actinic action of the chromosphere and the red flames.

"On Anthraflavic Acid, a Yellow Colouring Matter accompanying Artificial Alizarine," by EDWARD SCHUNCK, PH.D., F.R.S., V.P.

The artificial formation of alizarine is a process of so much importance both theoretically and practically, being in fact the first instance in which a natural colouring matter has been produced by artificial means, that everything connected with it must in the eyes of the chemist possess more or less importance, especially when we consider that it is chiefly to alizarine that madder owes its valuable dyeing properties. The process itself, as described by its discoverers, Gräbe and Liebermann, seems exceedingly simple, and consists in the conversion of the hydrocarbon anthracene $C_{14}H_{10}$ by the action successively of an oxidising agent, of bromine or sulphuric acid and of caustic alkali into alizarine $C_{14}H_8O_4$. Nevertheless, the product obtained on a large scale for the use of dyers and printers by this process is very far from being pure alizarine, so far indeed that some persons are inclined to doubt its perfect identity with the natural substance. Its solution in caustic alkali, for instance, has not the fine violet colour of a solution of pure alizarine, but is more or less purple or even red, and it differs in other respects. Now, though I have never entertained much doubt as regards their identity in the main, it might, I fancied, be interesting to ascertain whether the differences observed between the natural and artificial products were due to impurities accompanying the latter or not, for though these impurities, if present, might not cause any injury or inconvenience during the dyeing process, they might possibly be formed at the expense and take the place of alizarine, and thus be a source of loss to the manufacturer. Now a few simple experiments are sufficient to prove that artificial alizarine as ordinarily prepared is always accompanied by other substances, some of which are coloured while others are colourless or nearly so. My object on the present occasion is to describe one of these substances and to point out the relation in which it stands to alizarine.

My attention was first directed to this part of the subject by the results of some experiments made on a small scale to obtain alizarine from anthracene according to the directions of Gräbe and Liebermann. I was surprised to find that in spite of all the precautions taken I always obtained besides alizarine a notable quantity of another body also crystalline, but dissolving in alkalies with a yellow colour. This body bore so strong a resemblance in some of its properties to several of the rubiacine class of colouring matters, substances which are contained in madder along with alizarine, that my curiosity was excited. Having communicated this fact to Mr. Perkin, who, as is well known, is engaged in the manufacture of alizarine on a large scale, he kindly sent me for examination a specimen of the residue obtained by him in evaporating the mother liquors of alizarine. This residue was a crystalline, reddish-brown mass soluble in alkalies, with a cherry-red colour. I found it to contain in addition to alizarine a quantity of a substance apparently identical with that I had previously obtained directly from anthracene. I afterwards found the same body in commercial alizarine, both in that manufactured by Mr. Perkin and in a sample from a continental firm. I therefore requested Mr. Perkin to supply me with a quantity of his alizarine sufficient to enable me to prepare a pure specimen of this body, a request to which he very kindly acceded.

This alizarine, which was a yellow, almost amorphous powder, was in the first place treated with dilute caustic soda, in which it dissolved for the most part, yielding a dark purple solution. A small quantity of a pale yellow powder was left undissolved, which was filtered off, washed, dried, and heated, when it yielded crystals of anthraquinone. To the purple liquid there was added an excess of acid, which produced a bulky brownish-yellow precipitate. This was filtered off and treated with boiling alcohol until the whole was dissolved. The alcohol on cooling deposited a quantity of almost pure alizarine in small mica-like scales. The mother liquor of course contained alizarine, and in order to separate it acetate of lead was added, which gave a bulky purple precipitate of the lead compound of alizarine. The filtered liquid, which had a dark yellow colour, was evaporated, when it left a yellowish-brown residue, consisting for the most part of the yellow colouring matter or acid. In order to separate the latter from the impurities accompanying it the residue was treated first with water, and then with cold alcohol. It was then dissolved in dilute caustic soda, and to the boiling solution chloride of barium was added. The filtered liquid deposited on cooling a mass of small shining crystals of the barium salt of the acid. These were purified by recrystallisation from boiling water, and then treated with hydrochloric acid. The lemon-yellow flocks left by the acid were filtered off, washed, and dissolved in a little boiling alcohol. This on cooling deposited a quantity of yellow silky needles, consisting of the acid, which I have named *Anthraflavic Acid*, in order to indicate its source and its most obvious external property.

The chief properties of this acid are these: — When crystallised from alcohol and dried, it has the appearance of a dark lemon-yellow silky mass, which under the microscope is seen to consist of slender four-sided prisms. When heated on platinum foil it gives off copious yellow fumes and then burns with a luminous flame without leaving any residue. When cautiously heated in a tube or between two watch glasses, it may be almost entirely volatilised, yielding a vapour which condenses in the form of a yellow sublimate. This sublimate consists of small lustrous crystalline plates, which, examined under the microscope, exhibit very regular outlines. The acid is only slightly soluble in boiling water, and almost insoluble in cold. It is more soluble in alcohol and ether, but insoluble in boiling benzol and sulphide of carbon. It dissolves readily in concentrated sulphuric acid even in the cold, forming a dark yellow solution, from which it is precipitated by water in yellow flocks. It is not much affected by dilute nitric acid even on boiling. With fuming nitric acid it yields a so-called nitro-acid, to which I shall return presently.

It is the fact of this substance yielding with bases compounds of well defined character, some of them being regularly crystallised, that entitles it more especially to be classed among acids. When an alcoholic solution of anthraflavic acid is mixed with an alcoholic solution of potash, it assumes a dark yellow colour, and deposits on standing long orange-coloured needles arranged in stars and possessed of considerable lustre. The sodium compound prepared in the same manner crystallises in needles and resembles the potassium salt, but is lighter in colour. The ammonium salt may be obtained by dissolving the acid in boiling absolute alcohol and adding a slight excess of ammonia; on cooling, the solution deposits dark yellow lustrous crystals. These crystals however, after a short exposure to the air, lose the whole of their ammonia, leaving a yellow residue consisting of the acid itself. This inability to retain ammonia even at the ordinary temperature of the atmosphere is a proof of the feeble nature of the acid. The potassium and sodium salts are also rather unstable compounds, for if it be attempted to recrystallise either of them from boiling water a portion of the acid separates, the solubility of the base in water being sufficient to overcome its affinity for the acid. Anthraflavate of barium may be obtained by dissolving the acid in boiling baryta water, or by adding chloride of barium to a solution of the substance in caustic alkali. It is deposited from its watery solution in small shining plates, and after being filtered off and dried has a fine maroon colour. Under the microscope it is seen to consist of small crystals with very regular outlines. It may be recrystallised from water without decomposition. The strontium salt is very similar, being soluble in boiling water and crystallising in long needles. The calcium salt is, however, insoluble in water, and is precipitated in orange-coloured flocks on the addition of chloride of calcium to an ammoniacal solution of the acid. On adding sulphate of magnesium to a solution of the acid in ammonia no precipitate is produced, but on standing some time the magnesium salt is deposited in dark yellow crystalline plates and needles arranged in star-shaped clusters and possessed of much lustre. The aluminium compound, when prepared in the same manner, appears as a yellow deposit consisting of microscopic crystals. The ammoniacal solution gives with acetate of lead a voluminous orange-coloured precipitate, with acetate of copper a light brown, and with nitrate of silver a reddish-brown precipitate. The other compounds are of no particular interest.

All the compounds of the acid which are soluble in water yield yellow solutions; none are red. It is chiefly the presence of this acid in crude alizarine which affects the colour of the alkaline solution, changing the violet due to alizarine into purple, or when present in larger quantity, into red. For the same reason an alkaline solution of crude alizarine does not show the absorption bands in the spectrum so distinctly as one of pure alizarine. Alkaline, as well as alcoholic solutions of anthraflavic acid, absorb the blue end of the spectrum very powerfully, though no bands are visible, even with very dilute solutions. A solution of the acid in concentrated sulphuric acid, if not too dark, shows, however, a broad but well-defined absorption band at the extreme edge of the blue bordering on the green, accompanied by a total absorption of the violet as seen with the other solutions.

Anthraflavic acid dissolves very readily in fuming nitric acid even in the cold, yielding a deep yellow solution, which, on standing for 24 hours, becomes lighter in colour, without evolving any gas. On now adding water a quantity of light yellow shining crystals is deposited. These, when filtered off, washed, and dried, resemble anthraflavic acid. They are, however, totally different in their properties, and consist, there can be no doubt, of a so-called nitro-acid, in which one or more atoms of hydrogen are replaced by NO_2. When heated they deflagrate, and they give a potassium salt crystallising in yellow needles, very little soluble in water, and resembling picrate of potassium. Want of material has prevented my examining this product more fully.

Though anthraflavic acid yields intensely yellow compounds with bases, it seems to possess no dyeing properties. The freshly precipitated acid suspended in water communicates not the least tinge of colour to alumina and iron mordants on calico, however long the liquid may be boiled. Its presence in artificial alizarine is therefore of no consequence as regards the dyeing qualities of the latter.

The composition of anthraflavic acid is expressed by the formula $C_{15}H_{10}O_4$. That this is the true formula was proved by an examination of the silver and barium salts. The formula of the first is $C_{15}H_8Ag_2O_4$; that of the second $C_{15}H_8BaO_4 + H_2O$. The additional molecule of water attached to the barium salt is not driven off by heating to a temperature of 120°C. The acid is therefore bibasic. Hence it appears that this substance and alizarine stand in a very simple relation to one another. They are homologous bodies. Anthraflavic acid may be viewed as alizarine in which an atom of hydrogen is replaced by methyl. Though the great difference in properties, and especially the far greater stability of the acid, might lead to the inference that it is only as regards their composition that the two substances approach one another, a very simple experiment is sufficient to prove that they are in fact very closely related. If pure anthraflavic acid be dissolved in an excess of caustic potash, and the solution be boiled down to dryness, a yellow residue is left, which, after being carefully heated almost to fusion, dissolves in water with a red colour. This solution contains alizarine, as it shows the absorption bands in the spectrum peculiar to the latter, though not very clearly on account of undecomposed anthraflavic acid still present. Pure alizarine may, however, be obtained from it, by simply adding an excess of acid, filtering off the flocculent precipitate, dissolving the latter in alcohol, and adding to the solution acetate of lead, when a purple precipitate falls, which contains the whole of the alizarine, the excess of anthraflavic acid remaining in solution. From the lead precipitate alizarine may be obtained having all the properties of that substance. It is certain, therefore, that by the action of caustic potash, anthraflavic acid is converted into alizarine, the process being doubtless one of oxidation, though it should be stated that the conversion is never complete, probably because the action, if carried far enough to convert the whole of the acid, leads to the decomposition of the alizarine already formed. I am at present occupied with some experiments for the purpose of substituting an atom of hydrogen in alizarine by methyl, and thus forming anthraflavic acid synthetically. It is evident that the acid cannot be considered as a methylic ether of alizarine, since both substances combine with two atoms of base to form neutral compounds. If the substitution by methyl be possible, it must therefore take place in the radical of alizarine. The possibility of such substitutions is allowed by Gräbe and Liebermann, who consider chrysammic acid for instance as anthraquinone in which 4H are replaced by $4NO_2$. Should the synthesis just mentioned succeed, it will, I imagine, throw some light on the constitution of the so-called yellow colouring matters of madder, such as rubiacine and rubiadine, which certainly contain 16 atoms of carbon, and may possibly turn out to be substitution products of alizarine.

In what manner anthraflavic, with its 15 atoms of carbon, is formed from anthracene, which contains only 14, is not very clear. I imagined it to be just possible that the anthracene employed for preparing the alizarine supplied to me might have contained a higher hydrocarbon, say $C_{15}H_{12}$ or methylanthracene, which, by oxidation, would yield methylanthraquinone, and at the end of the process methylalizarine. On requesting Mr. Perkin to favour me with his opinion on this point, he informed me, however, that my supposition was improbable, because the alizarine which he sent me was prepared from nearly perfectly pure anthraquinone, which had been distilled and crystallised from benzol.

Another point remains to be considered in connection with this subject. It is well known that the beautiful discovery of the mode of forming alizarine was the direct result of a previous one, viz.: that of the reduction of the natural product by means of metallic zinc to anthracene. The question therefore naturally suggested itself: what is the nature of the hydrocarbon formed by the same process from anthraflavic acid? Is it anthracene or something else? In order to decide this question I took a quantity of the acid and heated it with 50 times its weight of zinc powder, in the manner described by Gräbe and Liebermann. I obtained a quantity of a brownish crystalline sublimate, amounting to about 10 per cent of the acid employed, which was purified by sublimation and washing with ether. It still retained the yellowish tinge which, according to Gräbe and Liebermann, adheres so pertinaciously to anthracene, but it did not differ in other respects from the pure substance. It melted at the same temperature as anthracene, and began to sublime before fusing, it dissolved in boiling alcohol, but more readily in benzol, and was deposited from these solutions in lustrous crystals of a very regular form, and it gave, like anthracene with picric acid, a compound crystallizing in long red needles. I wish to speak with some reserve on this point, as the quantity of material at my disposal was not sufficient for an analysis, but should it turn out that my product is identical with anthracene, this fact would throw doubt on some of the reasoning of Gräbe and Liebermann, who assume that if an organic substance yields a definite hydrocarbon by the action of metallic zinc, the latter contains the same number of atoms of carbon as the original substance.

The PRESIDENT stated that in looking over the Memorandum Book of Mr. Walker, one of the original members of this Society, kindly presented by Mr. Green, he had met with some interesting facts connected with the Cotton Trade a century ago. At that time the only places from which Manchester received cotton, except from continental ports, were Turkey, the West Indies, Brazil, and Demerara. To show the value the following extract is given.

"1772, July 15th. Prices of cotton wool at London:—

	d.		d.
St. Domingo	12½	to	13
Dominica	12		
Grenada	11	,,	12
Tortola	10½	,,	11
Jamaica	12	,,	13
French	12		
Smyrna	9	,,	9½
Solonica	8¼	,,	8½
Adonia	8	,,	8¼
Brazil at Manchester	13		

"All the above prices present payment. In the year 1771, Joshua Holt bought in Liverpool Tortola at 8½d., Grenada at 9d. to 10d., Tarlton's M.P.'s 12d., St. Domingo, good, 13d."

At that period other European countries imported cotton, as shown by the following extract:—

"From Berbecia all the cotton is sent to Holland, and the quantity rarely exceeds 150,000 lbs. annually, and some years when crops fail the quantity imported is not above 5,000 lbs. weight. Surinam at most 100,000 lbs. in one year. Essiquibo and Demerary not more than 50,000 lbs. in one year. These cottons are mostly consumed in Switzerland and at Brabant."

"Prices of cotton at Amsterdam, 15th November, 1774, from Rivoire and Van Heyst.

Smyrna			22	Groots per lb. of Holland. 40 groots = 1 guilder or florin, and 2 groots = 1⅛d.
Essequibo	39	to	40	
Demerary	39	,,	40	
Surinam	39	,,	41	
Berbecia	44	,,	45	
Curacoa	50	,,	55	

"Cotton was packed in bags which ran from 280 to 300lbs., and in pockets varying from 60 to 70lbs. each in weight."

The following is an account of the cotton imported at Bordeaux in the year 1774:—

	From St. Domingo.		From Martinique.		From Guadoloupe.	
	Bags.	Pockets.	Bags.	Pockets.	Bags.	Pockets.
"January..	47	12				
February...	38	122		11		
March......	26	315				
April	37	86				1
June	64	124			71	
July	59	47	10	3	29	
August ...	95	224	29		108	3
September.	49	232	20	1	118	13
October ...	2	1			3	1
November.	20	4			3	
	437	1167	59	15	332	18"

In order to show the route the Turkey cotton came to England, an extract of a letter from Otto, Franck, and Co., of Leghorn, dated March 24th, 1775, is given; it is as follows: "We have very good friends at Smirna, whose solidity and zeal can be depended on. If you choose to speculate from thence you are undoubtedly informed that not cotton only, but all products of the Levant cannot be sent from thence to England direct, when bought with Bullion or Bills of Exchange they must be landed and re-shipped here. To that effect we annex the following invoice account of freights and charges generally attending such transactions for your government. Talleris (a species of coin sent up thither) are at present very cheap, per 113 per cent., so that it would turn better to account to purchase them at this place and remit them to Smirna, than for the friends to draw."

It appears that in olden times there was a fair proportion of reckless speculators to honest traders, as is the case now. This is shown by extract from a letter addressed by Becker, Smith, and Buckholm, of Leghorn, to Mr. Walker, dated September 20, 1775.

"The many spoil-traders in the Levant (and particularly the Jews) are the chief cause of those high prices which the products there now bear. For buying up cottons and other goods, and drawing for the same, it very often happens that the Receivers in Leghorn are obliged to sell them under prime cost, in order to raise money to pay those drafts. By the last advices from Smyrna they quote the price of first sort 40L$, which would cost in warehouse here $18\frac{1}{8}$d. to $18\frac{1}{4}$d. But the last sold here was $16\frac{3}{4}$d. to 17d., so that there is a difference of 10 per cent. Indeed it often happens that articles from the Levant, especially cottons, generally sell here for less than they really lye in.

"The advantages arising to the English and French by their traffick in the Levant is owing to the goods they send thither for sale; such as woollen goods and other manufactured articles, which fetch a good price there, and the agents often barter them for cotton wool. But notwithstanding these advantages both the English and French frequently apply to this city for Turkey cotton, which makes it evident that they can get it cheaper here than they could import it from the Levant."

Here is an account from Messrs. Mayler and Maxse, of Bristol, dated October 4th, 1774.

"Cotton imported into Bristol this year:—

June 10.	From Nevis	2 B.	sold 15d.	very foul and stained.
July 18.	,, Tobago	12	,, 18d.	clean and good staple.
,, 28.	,, Tortola......	3	,, 17d.	middling quality.
Aug. 23.	,, Dominica...	1	,, 17d.	middling quality.
,, 29,	,, Grenada ...	150	,, 20d.	very good parcel.
Sep. 13.	,, Nevis	3	,, 16d.	very dirty and discoloured.
,, 24.	,, Jamaica...	12	Not yet sold, but engaged at market price, supposed 19 to 20d.	
	Ditto	53		
	Tobago ...	14	19d.	clean and good.
		250 Bags.		

"Most of the cotton imported at Bristol goes through the hands of Mayler and Maxse. Customary payment for cotton at Bristol is by Bills at 1 month, 6 weeks, or 2 months. The last is thought an indulgence.

"Freight from Bristol to Liverpool 30s. per ton. Insurance 20s. to 30s. per cent, according to the season of the year, but the ships are very irregular in this conveyance."

"Prices of Cotton in some of the West India Islands, April, 1775, as per letter received from John Craven, dated St. Croix, April 24th, 1775 :—

At St. Croix 2s. per lb. 100lb. Dutch weight there are equal to 112lb. English.

	d.
Exchange $84\frac{1}{4}$ per cent.	$11\frac{3}{4}$
Duty on Exportation 15 per cent.	$1\frac{1}{2}$
Commission there, Freight, Insurance, &c., home...	3
Interest till in cash again	$\frac{3}{4}$
1lb avoirdupois cost home	17

St. Eustatia—Exchange 70 per cent.

	d.
22d. per lb., under the above circumstances	$15\frac{3}{4}$
2s. 3d. ditto ditto	$18\frac{3}{4}$
2s. 6d. St. Domingo Cotton (Interest 1d.)	$20\frac{1}{2}$ "

"Cotton imported at London from Christmas, 1769, to 1774, per a clerk at the Custom House :—

Year.	lbs. weight.	
1770	1,544,488	
1771	726,923	
1772	2,730,167	
1773	988,737	
1774	2,691,473	
1776	530,832, West India 1,253,637, Turkey	As per Bills of Entry."

"At Lancaster—Tare 4lb. per 112lb., draught 1lb. per bag.

West India Cotton imported	1771...3107	Bags and Pockets.
	1772...3803	
	1773...3821	
	1774...4293	
To December,	1775...4051	
	1776...3055	
	1777...4903	

Carriage by land to Manchester, 2s. 6d. per cwt.
Freight to Liverpool from Lancaster, 8d. per 112lbs.
Insurance $\frac{1}{2}$ per cent. This way 2s. 6d. also."

"Cotton imported at Lancaster from January, 1775, to January 1st, 1776 :—

	Bags.	lbs.
From Jamaica ...	479 ...	96,999
Grenada ...	854 ...	225,483
St. Vincent...	330 ...	88,533
Dominica ...	813 ...	232,470
St. Kitts ...	568 ...	145,257
Antigua ...	55 ...	13,322
Barbadoes ...	672 ...	100,337
	3,771	902,431"

" West India Cotton imported at Liverpool—

1770	5,820	Bags and Pockets.
1771	4,897	
1772	7,496	
1773	4,633	
1774	5,276	
1775	4,525	
1776	6,566	West India.
	1,547	Turkey.

Direct from West Indies	4,411
Havre de Grace	81
Nantes	5
Cadiz	6
Rotterdam	22
	4,525

Turkey Cotton imported into Liverpool in 1775—

From Leghorn	507	bales.
,, Rotterdam	588	,,
,, Amsterdam	288	,,
,, Marseilles	1,356	,,
Turkey Cotton imported at Liverpool ...	2,739	bales."

Ordinary Meeting, March 21st, 1871.

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

Mr. John Hopkinson, D.Sc., was elected an Ordinary Member of the Society.

"On the Mechanical Equivalence of Heat," by the Rev. H. Highton, M.A.

The following is an abstract of the arguments as given in the paper and brought out in the subsequent discussion.

1. The author apologised for having mentioned other names in connection with great discoveries which were undoubtedly due primarily to Dr. Joule, and spoke of the very great value of Dr. Joule's experiments, even when he did not agree with the deductions drawn from them.

2. The subject is of extreme importance both for the interpretation of physical phenomena and for determining what limits are assigned by the stern laws of Nature to the exercise of man's mechanical and scientific skill.

3. No doubt Dr. Joule has ascertained the heat ordinarily derived from the destruction of energy, by means of friction with various substances; but it has been assumed, *in defiance of facts*, that the numerical relations which connect heat and energy in the case of friction hold good when energy and heat produce or destroy each other by any other means.

4. In the case of friction itself, energy is not transformed simply into heat, but partly into heat and partly into another kind of energy, which is involved in the expansion of the solids or liquids acted on.

5. No doubt the coincidence between the mechanical equivalent of heat, found by Dr. Joule from friction, and that by M. Favre from working a magnetic engine, seems very striking; but

A. The value of Favre's experiment disappears on examination. It was but a single experiment, either never repeated, or never repeated with the same results; in a very delicate experiment there was only the difference of 300 units out of 18,000; and even the permanent enlargement which always takes place in magnets which are in use might account for these; and

B. Numerous and long-continued experiments by M. Soret show results entirely discordant with the single one of M. Favre.

6. It seems incredible, that with the imperfectly constructed engine used by Joule and Scoresby, they should at the very first trial have succeeded in utilizing 2-3rds of the magnetism evolved, or capable of being evolved, by their battery; and Dr. Joule now tells us that according to his latest calculations of the mechanical equivalence of heat, they utilized 6-7ths of the power of the battery. The only conclusion we can arrive at is, that the real power of the battery, and therefore of a grain of zinc, must have been much greater than he calculated.

7. For consider the disadvantages under which the engine acted:

A. The temporary and permanent magnets were never nearer than ¼ of an inch apart. Though Dr. Joule assures us this does not affect the *power* of the engine, it certainly produces a waste of zinc, as the near approach of the magnets creates counter-currents which check materially the consumption of zinc.

B. The copper wire was not tested for conductivity; a subject little thought of at that time, and it is found that a very small impurity in copper wire will very, very, largely diminish the power of an electro-magnet.

C. The iron was not tested for specific capacity for magnetism; yet this is a most important point which is even now but little appreciated. It is found practically that, if two electro-magnets be made from the very same piece of iron, most carefully prepared, with the very same length of the same wire, without the slightest assignable cause, one will sometimes have three times the power of the other. Hence I conclude that the maximum energy capable of being evolved by a grain of zinc must be very much greater than that assigned to it by Dr. Joule.

7. Dr. Hopkinson's argument, in his paper lately read to this Society, virtually amounted to this—that a well constructed magnetic engine will get no more duty from a grain of zinc than an ill-constructed one; and consequently, I presume, that magnets might be weakened to any extent, and removed to ever so great a distance from one another, without necessarily affecting the efficiency of the engine.

8. Dr. Hopkinson has in his criticism strangely substituted $(a—b)$ for $(b.)$ In Joule and Scoresby's paper, the consumption of zinc is expressed not by $(a—b)$ but by (b); and consequently the duty of a grain of zinc not by $\frac{W}{a-b}$ but by $\frac{W}{b}$; and when the magnets are stronger and approach nearer to each other, even if W be not increased, (b) is diminished.

9. My argument was this, that since the accepted theory of the mechanical equivalence of heat is *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, then the doctrine of the mechanical equivalence of heat cannot be true; we might with equal justice call it a mechanical equivalence of cold. It is no reply to say that such facts are simple deductions from the laws of thermodynamics. This would

only show that the laws of thermodynamics are inconsistent with the doctrine of the mechanical equivalence of heat.

10. The argument from the fire syringe I withdraw, as inconclusive. But I think my case was sufficiently established without it.

11. Joule and Scoresby in their paper incorrectly assume that if the quantities of electricity in the current at different times be represented by (a) and (b), the heat varies as a^2 to b^2. This is only true where the resistance is the same. In the case before us the working of the engine introduces a fresh element in resistance.

12. Again, by assuming that (a—b) represents diminution of quantity of the current, *and* the diminution in the zinc consumed, *and* the heat converted into useful work, they involve the supposition either that less zinc produced equal heat, or that heat was changed into useful work which was never produced at all, and therefore could not be absorbed. In fact there was no *proof* that any heat was absorbed at all.

13. It is said that in electro-plating, electro-magnetic engines, worked by steam, are found more economical than batteries. This is in cases where a battery of many cells would be required; which is always wasteful, as a large number of equivalents of zinc must be consumed to deposit one equivalent of silver or other metal.

14. Besides, there is a far greater advantage in changing work into electricity, than electricity into work. In the former case all, or nearly all, the work is effective; in the latter, a very small portion of the electricity has hitherto been utilised.

Dr. Hopkinson said that most of Mr. Highton's objections to the mechanical equivalent of heat appear to arise from a mistake as to what is meant by the term. The nature of this mistake may be best seen in the case of a perfect heat engine, of which t_1 and t_0 are the absolute temperatures of the source and refrigerator. Then from every unit of heat leaving the source we obtain $\frac{t_1 - t_0}{t_1}J$ units of work. Now this a quantity variable with t_1 and t_0; it would be similar to most of Mr. Highton's arguments to infer that from a given quantity of heat a variable quantity of work could be obtained. But, of course, the case really is, that, of the unit of heat leaving the source, $\frac{t_0}{t_1}$ is lost in the refrigerator, whilst $\frac{t_1 - t_0}{t_1}$ disappears *as heat* and is converted into the work done, and the principle of the equivalence of heat and work asserts that J is constant. It will be seen that this is the mistake Mr. Highton makes in his paper in the Journal of Science (end of article 6). He seems there to imagine it stated, that the work done is equivalent to the whole heat thrown into the gas, and he fails to perceive that a certain portion is used to raise the temperature of the air or turpentine.

This will make my criticism of his paper in the *Chemical News* clearer. Mr. Highton argued against the mechanical equivalent, and what I pointed out was, that the chemical energy, which was converted into mechanical effect and *not* used to heat the wire, was proportional to $a—b$, that therefore, in order to prove that there was no mechanical equivalent, Mr. Highton must show $\frac{W}{a - b}$ is variable. I do not assert that a badly constructed engine will get as much heat from fuel as a good one, but merely that the work done and the heat, which has disappeared as heat and been converted into work, are in a constant ratio.

Now as regards Mr. Highton's argument from the case of elastic wires—that the wire will be cooled when stretched follows from the two laws of thermodynamics, a proof may be seen in Tait's Thermodynamics, p. 105. Mr. Highton replies, "Quite true; but this only shows that one of the laws of thermodynamics is inconsistent with the doctrine of the mechanical equivalence of heat." Now the first law of thermodynamics asserts nothing else than that there is a mechanical equivalent, constant in all cases; whilst the second law, as usually stated, involves the first law, and involves nothing else but Carnot's axiom and the principle that in conduction heat flows from the hot to the cold body, both of which no one will doubt. Mr. Highton's reply is very similar to stating that one of Kepler's laws is inconsistent with the planets moving in ellipses. What Mr. Highton proposes as a paradox is then a necessary consequence of the principle he attacks.

Though the doctrine of the mechanical equivalent of heat finds its firmest basis in the immortal experiments of Dr. Joule, the fact, that assuming it we can explain many phenomena, is a valuable supplementary proof.

"Examples of the Performance of the Electro-Magnetic Engine," by J. P. Joule, D.C.L., F.R.S., &c., V.P.

Some experiments and conclusions I arrived at a quarter of a century ago having been recently criticised, I have thought it might be useful to place the subject of work in connexion with electro-magnetism in a different and I hope clearer form than that in which I have hitherto placed it. The numbers given below are derived from recent experiments.

Suppose an electro-magnetic engine to be furnished with fixed permanent steel magnets, and a bar of iron made to revolve between the poles of the steel magnets by reversing the current in its coil of wire. Such an arrangement is perhaps the most efficient, as it is the most simple form of the apparatus. In considering it, we will first suppose the battery to consist of 5 large Daniell's cells in series, so large that their resistance may be neglected. We will also suppose that the coil of wire on the revolving bar is made of a copper wire 389 feet long, and $\frac{1}{16}$ of an inch diameter, or offering a resistance equal to one BA unit. Then, on connecting the terminals of this wire with the battery, and keeping the engine still, the current through the wire will be such as, with a horizontal force of earth's magnetism 3·678, would be able to deflect the small needle of a galvanometer furnished with a single circle of one foot diameter, to the angle of 54° 23′. Also this current going through the above wire for one hour will evolve heat that could raise 110·66 lbs. of water 1°, a quantity equal to 85430 ft. lbs. of work. In the meantime the zinc consumed in the battery will be 535·25 grains. Hence the work due to each grain of zinc is 159·6 ft. lbs., and heat ·20674 of a unit.

I. In the condition of the engine being kept still we have therefore, current being 1·396, as shown by a deflection of 54° 23′,

1. Heat evolved per hour by the wire 110·66 units.
2. Consumption of zinc per hour 535·25 grains.
3. Heat due to 535·25 grains, 110·66 units.
4. Therefore the work per hour will be $(110{\cdot}66 - 110{\cdot}66)772 = 0$.
5. And the work per grain of zinc will be $\frac{0}{535{\cdot}25} = 0$.

II. If the engine be now started and kept by a proper load to a velocity which reduces the current to $\frac{2}{3}$, or ·9307, indicated by deflection 42° 57′ we shall have

1. Heat evolved per hour by the wire $110{\cdot}66 \times \left\{\frac{2}{3}\right\}^2 = 49{\cdot}18$ units.
2. Consumption of zinc per hour $535{\cdot}25 \times \frac{2}{3} = 356{\cdot}83$ grains.
3. Heat due to 356·83 grains, $110{\cdot}66 \times \frac{2}{3} = 73{\cdot}77$ units.
4. Therefore the work per hour will be $(73{\cdot}77 - 49{\cdot}18)772$ $= 18983$ ft. lbs.
5. And the work per grain of zinc will be $\frac{18983}{356{\cdot}83} = 53{\cdot}2$ or $\frac{1}{3}$ of the maximum.

III. If the load be lessened until the current is reduced to $\frac{1}{2}$ of the original amount, or to ·698, we shall have

1. Heat evolved per hour by the wire $110{\cdot}66 \times \left(\frac{1}{2}\right)^2 = 27{\cdot}665$ units.

2. Consumption of zinc per hour $535{\cdot}25 \times \frac{1}{2} = 267{\cdot}62$ grains.

3. Heat due to 267·62 grains $110{\cdot}66 \times \frac{1}{2} = 55{\cdot}33$.

4. Therefore the work per hour will be $(55{\cdot}33 - 27{\cdot}665)772 = 21357$.

5. And the work per grain of zinc will be $\frac{21357}{267{\cdot}62} = 79{\cdot}8$ or $\frac{1}{2}$ of the maximum duty.

IV. If the load be still further reduced and velocity increased so as to bring down the current to $\frac{1}{3}$ of what it was when the engine was still, or to ·4653, shown by a deflection of the galvanometer of 24° 57′ we shall have

1. Heat evolved per hour by the wire $110{\cdot}66 \times \left(\frac{1}{3}\right)^2 = 12{\cdot}294$ units.

2. Consumption of zinc per hour $535{\cdot}25 \times \frac{1}{3} = 178{\cdot}42$ grains.

3. Heat due to 178·42 grains $110{\cdot}66 \times \frac{1}{3} = 36{\cdot}89$ units.

4. Therefore the work per hour will be $(36{\cdot}89 - 12{\cdot}294)772 = 18988$ ft. lbs.

5. And the work per grain of zinc will be $\frac{18988}{178{\cdot}42} = 106{\cdot}4$, or $\frac{2}{3}$ of the maximum duty.

V. Remove the load still further until the velocity increases so much that the current is brought down to $\frac{1}{100}$ of its quantity when the engine is still. Then we shall have

1. Heat evolved per hour by the wire $110{\cdot}66 \times \left(\frac{1}{100}\right)^2 = {\cdot}011066$ of a unit.

2. Consumption of zinc per hour $535{\cdot}25 \times \frac{1}{100} = 5{\cdot}3525$ grains.

3. Heat due to 5·3525 grains of zinc $110{\cdot}66 \times \frac{1}{100} = 1{\cdot}1066$ units.

4. Therefore the work per hour will be $(1{\cdot}1066 - {\cdot}011066)772 = 845{\cdot}73$ ft. lbs.

5. And the work per grain of zinc will be $\frac{845{\cdot}73}{5{\cdot}352} = 158$ or $\frac{99}{100}$ of the maximum duty.

When the velocity increases so that the current vanishes the duty = 159·6.

I. Let us now improve the engine by giving it a coil of 4 times the conductivity, which will be done by using a copper wire 389 feet long and $\frac{1}{8}$th of an inch diameter, the same battery being used as before. Then when the engine is kept still we shall have a current $1{\cdot}396 \times 4 = 5{\cdot}584$, shown by a deflection of 79° 51′. Then we shall have

1. Heat evolved per hour by the wire $110{\cdot}66 \times \frac{4^2}{4} = 442{\cdot}64$ units.
2. Consumption of zinc per hour $535{\cdot}25 \times 4 = 2141$ grains.
3. Heat due to 2141 grains 442·64 units.
4. Therefore the work per hour will be $(442{\cdot}64 - 442{\cdot}64)772 = 0$.
5. And the work per grain of zinc will be $\frac{0}{2141} = 0$

II. Start the engine with such a load as shall reduce the current to $\frac{2}{3}$, or to 3·7227 (74° 58′), then we shall have

1. Heat evolved per hour by the wire $442{\cdot}64 \times \left(\frac{2}{3}\right)^2 = 196{\cdot}73$ units.
2. Consumption of zinc per hour $2141 \times \frac{2}{3} = 1427{\cdot}3$ grains.
3. Heat due to 1427·3 grains $442{\cdot}64 \times \frac{2}{3} = 295{\cdot}09$ units.
4. Therefore the work per hour will be $(295{\cdot}09 - 196{\cdot}73)772 = 75934$.
5. And the work per grain of zinc will be $\frac{75934}{1427{\cdot}3} = 53{\cdot}2$ or $\frac{1}{3}$ of the maximum duty.

III. Lessen the load so that the velocity of the engine is increased until the current is reduced to one half its original amount, or 2·792 shown on the galvanometer by a deflection of 70° 18′. Then we shall have,

1. Heat evolved per hour by the wire $442{\cdot}64 \times \left(\frac{1}{2}\right)^2 = 110{\cdot}66$ units.
2. Consumption of zinc per hour $2141 \times \frac{1}{2} = 1070{\cdot}5$ grains.
3. Heat due to 1070·5 grains, $442{\cdot}64 \times \frac{1}{2} = 221{\cdot}32$ units.
4. Therefore the work per hour will be $(221{\cdot}32 - 110{\cdot}66)772 = 85430$ ft. lbs.
5. And the work per grain of zinc will be $\frac{85429}{1070{\cdot}5} = 79{\cdot}8$ or $\frac{1}{2}$ the maximum duty.

IV. Let the load be further reduced until the velocity reduces the current to $\frac{1}{3}$, or to 1·8613 shown by a deflection of 61° 45′. Then we shall have

1. Heat evolved per hour by the wire $442·64 \times \left(\frac{1}{3}\right)^2 = 49·182$ units.
2. Consumption of zinc per hour $2141 \times \frac{1}{3} = 713·66$ grains.
3. Heat due to 713·66 grains of zinc $442·64 \times \frac{1}{3} = 147·55$ units.
4. Therefore the work per hour will be $(147·55 - 49·182)772 =$ 75940 ft. lbs.
5. And the work per grain of zinc will be $\frac{75940}{713·66} = 106·4$ or $\frac{2}{3}$ of the maximum.

V. Let the load be still further reduced until, with the increased velocity, the current becomes reduced to $\frac{1}{100}$, or to ·05584 showing a deflection of 3° 12′. Then we shall have

1. Heat evolved per hour by the wire $442·64 \times \left(\frac{1}{100}\right)^2 = ·044264$ of a unit.
2. Consumption of zinc per hour $2141 \times \frac{1}{100} = 21·41$ grains.
3. Heat due to 21·41 grains of zinc $442·64 \times \frac{1}{100} = 4·4264$ units.
4. Therefore the work per hour will be $(4·4264 - ·04426)772 =$ 3383 ft. lbs.
5. And the work per grain of zinc will be $\frac{3383}{21·41} = 158$ or $\frac{99}{100}$ of the maximum duty.

Now suppose that we still further improve our engine by making the stationary magnets twice as powerful. In this case all the figures will remain exactly the same as before, the only difference being that the engine will only require to go at half the velocity in order to reduce the current to the same fraction of its first quantity. The attraction will be doubled, but the velocity being halved no change will take place in the amount of work given out.

In all cases the maximum amount of work per hour is obtained when the engine is going at such a velocity as reduces the current to one half of its amount when the engine is held stationary; and in this case the duty per grain of zinc is one half of the theoretical maximum.

The same principles apply equally well when, instead of employing the machine as an engine evolving work, we do work on it by forcibly reversing the direction of its motion. Suppose for instance we urge it with this reverse velocity until the quantity of current is quadrupled or becomes 22·336 indicated by a deflection of 87° 26′. Then we shall have

1. Heat evolved per hour by the wire $442{\cdot}64 \times 4^2 = 7082{\cdot}2$ units.
2. Consumption of zinc per hour $2141 \times 4 = 8564$ grains.
3. Heat due to 8564 grains of zinc $442{\cdot}64 \times 4 = 1770{\cdot}56$ units.
4. Therefore the work per hour will be $(1770{\cdot}56 - 7082{\cdot}2)772 = -4100432$ ft. lbs.
5. And the work per grain of zinc will be $\frac{-4100432}{8564} = -478{\cdot}8$ or -3 times the maximum working duty.

The principal reason why there has been greater scope for the improvement of the steam engine than for the electro-magnetic engine arises from the circumstance that in the formula $\frac{a-b}{a}$, applied to the steam engine by Thomson, in which a and b are the highest and lowest temperatures, these values are limited by practical difficulties. For a cannot easily be taken above $459° + 374° = 833°$ from absolute zero, since that temperature gives 12·425 atmospheres of pressure, nor can b be readily taken at less than the atmospheric temperature or $459° + 60° = 519°$. Also there is much difficulty in preventing the escape of heat; whereas the insulation of electricity presents no difficulty.

I had arrived at the theory of the electro-magnetic engine in 1840, in which year I published a paper in the 4th Vol. of Sturgeon's Annals, demonstrating that there is "no variation in economy, whatever the arrangement of the conducting metal, or whatever the size of the battery." The experiments of that paper indicate 36 foot lbs. as the maximum duty for a grain of zinc in a Wollaston battery. Multiplying this by 4 to bring it to the intensity of a Daniell's battery, we obtain 144 foot lbs. Here, as in the experiments in the paper on Mechanical Powers of Electro-Magnetism, Steam, and Horses, the actual duty is less than the theoretic; which is owing partly to the pulsatory nature of the current, and partly also to induced currents giving out heat in the substance of the iron cores of the electro-magnets; although these last were obviated as far as possible by using annealed tubes with slits down their sides.

Ordinary Meeting, April 4th, 1871.

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

Mr. W. Mellor and Mr. S. C. Trapp were appointed Auditors of the Treasurer's Accounts.

The President said that Mr. B. H. Green had presented to the Society another of the books of the late Mr. George Walker, from which he desired to give a few extracts. The following relates to the production of cotton:—"At Liverpool, May 8th, 1784, met with Mr. Cock, lately come from Barbadoes, and who had resided in that island as factor for the last eight years. He says the crop of cotton in that island this year is greater than ever was known, and that the quality is very good; not less than 8,000 bags from 1½ to 2 cwt. each will this year be produced there. The usual time for sowing the Cotton seed is in the month of July, and Cotton is ready to pick off said trees the next Christmas, say in five or six months. These trees would continue to produce Cotton annually for several years, but the planter finds it most advantageous to plant fresh seeds every year. The trees are pulled up by the roots when the cotton picking is ended, and which is in April or May in each year. These shrubs serve for fuel. Betwixt the rows of cotton trees there are vegetables which come to perfection long before the cotton is gathered. This picking or gathering is performed every day as the pods open, and continues from Christmas to March.

"The best Cotton is produced on the windward part of the Island. That produced to the leeward is called *Syke's Cotton,* and is inferior in colour and staple, nor is it so well cleared from seeds and dirt as that to windward. There is a species of Cotton in Barbadoes called *Vine Cotton* from the stems resembling those of vines, being long and slender. This plant produces but few pods in proportion to their common Cotton trees, therefore it is not much attended to. The Cotton is very white, long, and silky, something like Demerara or the finest Withywood Jamaica Cotton. Some of this Cotton is packed in small bags entire, but often mixed with common Barbadoes. G. W. bought 4 Bags this day at $17\frac{3}{4}$d. and the best Barbadoes was offered at $15\frac{3}{4}$d. at same time.

"The usual quantity of Cotton produced on each acre of land in Barbadoes is about 300 lbs. This year 8 acres have produced 3000 lbs., which shews that there is a plentiful harvest. It generally happens that the produce is of the best quality when the crop is plentiful. When land is bought in Barbadoes the usual price is £50 per acre, and £50 per head for every slave kept on that estate. The seller always disposes of the slaves along with the plantation.

"Mr. Cock says the Islands of Guadaloupe, Martinique, and Grenada will produce great quantites of Cotton this season, but the crop will be very small on Tobago, there being there a general blight. The current prices in Barbadoes is 15d. Exchange 135. In the French Islands 205 to 215. Exchange 182.

"Lancaster, June, 1784.—On enquiry made here it appeared that the current prices at Guadaloupe and Martinique were 170 to 180 Louies for 100 French Weight and the Exchange 180 per Cent."

COTTON WOOL.—MEMORANDUMS RESPECTING IT.

AVERAGE PRICES OF COTTON.

		Turkey Smirna.	Surat.	Carthagena.	Leeward Islands.	French.	Barbadoes.	Tobago.	Domingo.	Jamaica.	M.P.	Brasil Maranm.	Demerara.	Surinam.	Cayenne.	Pernambuco.	Georgia.	
		d.	d.	d.	d.	d.	d,	d.	d.	d.	d.	d.	d.	d.	d.	d.	d.	
1771	Liverpool....				9½	10½			13		12							In the year 1744 and 1745 Smirna Cotton sold at 7d. per lb. G. W.
1770	Manchester..				10½	11½			14	13								
1772	London......	9			11½	12			13	12½		12						
1773	,,																	
1774	,,																	
1775	,,																	
1776	,,																	
1777	Manchester..	19½				18			21									
1778	,,	13				17			18½									
1779	,,	13				16			18									
1780	,,	15½				19			21									
1781	,,	21				27			30	In Nov. 1781, T.&W. sold Barb. 3s. 7d. Cr.								
1782	,,	24				36			40									
1783	,,	16				20			21½									
1784	,,	13				17			19									
1785	,,	14				21			23									
1786	,,	14½				22	23	23½	23½			2/6	2/2	2/2	2/3	3/3		
1794	,,	10½	11½		15½	17	17		17				18½d			22d.		In June.
1795	,,	14½				22½	22½		23			2/4	2/3½			2/4		,,
1796	,,	17½				2/-	2/-		23½			2/2	2/0½	2/3	2/4			,,
1797	,,		19									2/3	2/2			2/5		,,
1798	,,	23	20				2/5					2/8		2/8			21	
1799	,,		18				3/9 2/2	}				4/4 } 2/10 }	4/2 Oct			 Oct	2/6½ 20	,, ,,
1800	,,			2/-			2/8		2/9			2/8½				3/1		,,
1801	,,					2/6	2/7		2/6			2/10	2/8			3/-	22½	,,
1802	,,											2/3				2/10	21	January.

COTTON IMPORTED.

Year.	London. Wst India	Turkey.	Liver pool. Wst India	Turkey.	Lancaster Wst India	Bristol. Wst India	Hull. Scind.	Glasgow. Wst India
1788	25055	6560	24469	485	6196	1510	...	5012
1789	38227	9690	46671	673	9295	2000	1680	5878
1790	34874	12575	51060	851	8842	2000	3066	12478
1791	41266	4235	68400	...	10000	2000	5033	12531
1792	36390	18928	66390	6186				
1793	26432	11168	22114	2770				
1794	39702	16953	36669	835				
1795	44799	2051	55917	32				
1796	49475	4271	63914	...				
1797	25552	5922	58352	...				
1798	35637	7824	69520	151				
1799	66172	12640	93014	186				
1809	} 215405 from North America, Brazils, and West Indies, from Jan. 1st to August 21st, 1809.—See particulars below.							

Liverpool, August 28th, 1809.—William Peers to John Aspinwalls.

Cotton imported at Liverpool from 1st Jan. to Aug. 21st.

From New Orleans	4773	99767 Bags.
Different American States	94994	
Brazils	68460	
Demerara	14210	
Barbadoes	5053	
Jamaica	7379	
West India Islands	20536	—Total, 215,405 Bags.

Cotton imported into London, Liverpool, and Glasgow, during the year 1807.

British West Indies	28969	Bags.
Colonies conquered from the Dutch	43651	
Portugal	18981	
East Indies	11409	(See *Month Review*
United States of America	171267	for August, 1812.)
All other ports	8390	
	282,667	Bags.

Add Bristol and Lancaster.

"Arsenic in Pyrites and Various Products," by H. A. Smith, communicated by Professor Roscoe, F.R.S.

The difference existing between the amount of Arsenic in Pyrites in published analyses and that found in practical working led the author to make the following series of experiments, the results of which are arranged in three tables.

Table I. Part I. Gives the analysis of various specimens of Pyrites; these are extracted from Richardson and Watt's Technology in Part II. The author's analyses are given of a few specimens of the same species of ore.

Table II. Gives the amount of arsenic in a certain kind of Pyrites; in the sulphuric acid manufactured from it; and in the various products in the manufacture of which the sulphuric acid had been used; also the amount of arsenic in the chamber and flue deposits.

Table III. Is calculated from Table II., giving the amount in a manner more useful for practical purposes.

TABLE I.

Pyrites.	Arsenic per Cent.	Pyrites.	Number of Analyses made.	Arsenic per Cent.
Spanish	0·21 to 0·31	Spanish { Mason's	10	1·7453.
		Spanish { Tharsis.	10	1·6517.
Belgian.........	Trace.	Belgian	8	0·9437.
Westphalian...	Trace.	Westphalian	8	1·8783.
Norwegian ...	None.	Norwegian { Hard	8	1·6490.
		Norwegian { Soft..	8	1·7085.

TABLE II.

Substance in which Arsenic is found.	Number of Analyses	Arsenic per cent.
Pyrites before burning	8	1·649
,, after ,, ...	4	0·465
Sulphuric Acid	15	1·051
Deposit in flue leading from Pyrites kiln to Lead chamber	4	46·360
Deposit on bottom of Lead chamber	5	1·857
Hydrochloric Acid	8	0·691
Sulphate of Soda	15	0·029
Soda waste	6	0·442
Carbonate of Soda	12	——
Recovered Sulphur (Morid's process) before purification began	4	0·700
Morid's process, latest methods	2	0·000

The Pyrites used for analysis in Table II. was the hard Norwegian, and this, as may be seen in Table I. comes next in order in freedom from arsenic to the Belgian. The Belgian itself was put aside owing to the fact that it made too much "smalls" in breaking for the kilns, which became an expense as well as inconvenience.

TABLE III.

	Tons As.
100 tons hard Norwegian Pyrites (Table I) contain before burning	1·649
,, ,, after burning	0·465
100 tons hard Norwegian Pyrites make 140·875 tons H_2SO_4 containing	1·481
140·875 tons Sulphuric Acid make 104·9 tons HCl containing....................................... ...	0·724
140·875 tons Sulphuric Acid make 204·12 tons Na_2SO_4 containing	0·059

This Table gives the amounts of arsenic on a practical scale so that the total impurity may be seen at once.

We see by this Table how the arsenic persistently adheres to the products in the manufacture of which acid made from Pyrites has been used, and how it becomes distributed through the acid and soda in various stages.

The arsenic also has an effect on the Nitric Acid supplied to the lead chamber, being converted from arsenious to arsenic acid, and thereby causing a slight loss of the Nitric Acid Gas. In the deposit in the chamber (mentioned in table II.) were found crystals of Arsenic Acid, as a proof of this result.

Mr. Boyd Dawkins, F.R.S., exhibited a collection of human bones obtained from a cave at Terlhi Chwareu, a place about six miles from Llangollen, and from a chambered tomb at Cefn, near St. Asaph. The corpses to which they belonged had been buried in the sitting posture, as in most of the Neolillni interments. The examination of the skulls proved that the cranial capacity of the ancient dwellers in Denbighshire at that time was not below the average at the present day. The angle at which the nasals articulated with the frontals showed that their noses were of the turned-up order, and in no sense aquiline. Their stature, however, ranged from 5 feet to 5 feet 4 or 6 inches. Some of the leg bones from *both* interments exhibited the peculiar antero-posterior flatness of shin which Prof. Busk terms platycnemic, and which M. Broca believes to be a race-character, while others were of the more usual form. The flatness however differed from that observed in the interments of France and Gibraltar, in that it was due to the anterior extension of the bone, and *not* to its posterior extension. The skulls differ most materially from both those of Gibraltar and of France. It follows therefore that M. Broca's estimate of the value of platycnemism as a race-character is far too high, since it is found to obtain in skeletons of races offering most important cranial differences, and since it is not found in all the individuals of the same race at Cefn and Terlhi Chwareu. This is the first case of its being noticed in any skeleton in Great Britain.

The relation of these interments to history is unknown, and there is no clue to the race of men by whom they were made. Besides the Teutonic and Celtic and Iberian races which have successively occupied Britain, there were most probably other races of which the very names have perished. Till we can be certain that this is not the case, it will be impossible to assign remains of this kind to any given race by an appeal to cranial and skeletal characters. A flint flake in the cave corroborates to a certain extent the Neolillni character of the interments, which were undoubtedly made by men of the same race. The chambered tomb was of a class common to France, Britain, and Scandinavia, termed by Dr. Thurnam 'gallery-graves,' and by Professor Nilsson 'gangraben.'

"Description of some Experiments on the Method of propelling Balloons, illustrated by a Model," by Professor Osborne Reynolds, M.A.

"Notes on the Drift of the Eastern Parts of the Counties of Chester and Lancaster," by E. W. Binney, F.R.S., F.G.S., President.

*** Abstracts of these papers will appear in the next number of Proceedings.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 27th, 1871.

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

"Notes on *Polygonum minus* and its allies," by Mr. G. E. Hunt.

The following remarks are written in reference to the discussion at this Society in November last, as to the claims of Polygonum mite, Schrank, to rank as a native of Cheshire, and in support of which it was stated that "so long ago as 1828, Mr. W. Wilson, of Warrington, sent the plant from a Cheshire locality under the erroneous name of P. minus to the late Sir W. J. Hooker, in whose Herbarium at Kew the specimens still are."

Through the kindness of Mr. Baker of Kew I have been since furnished with the perianths and fruit of the original specimen referred to above, and have compared them carefully with P. minus and mite from various stations both in Britain and from the Continent. The comparison quite satisfied me that the Kew specimen from Cheshire could not be associated with P. mite, but was correctly referred by Mr. Wilson to P. minus, Huds. I forwarded specimens to Mr. Baker for his opinion, stating my own views as expressed above, and received his reply as follows, in a letter dated 31st January, 1871 — "I believe now that I have laid the nuts side by side and compared them carefully, that you are quite right about the Polygonum." I may further add that all the specimens also of more recent collection from Lancashire and Cheshire seen by me belong to P. minus, Huds.

Polygonum minus, Huds., has a lax perfectly erect somewhat interrupted spike of small flowers, with small pitchy black smooth shining nuts. Syme (in *Eng. Botany*) mentions the presence of very minute glands at the base of the ochreæ and perianths, and I find these in the Cheshire specimens. Leaves linear lanceolate, smooth margin, ciliated. Stems, in all the specimens I have seen, more or less diffuse or ascending, but never erect. Syme describes the stem as commonly geniculate and rooting at the base, then erect and ascending. Babington describes it as usually procumbent, diffuse.

Polygonum mite, Schrank, is a more robust plant than P. minus, and has a lax perfectly erect, somewhat interrupted spike of rather large perianths, with dark, slightly rough, but shining nuts, twice the size of those of P. minus; leaves elliptical, smooth margin, ciliated. Syme describes the stem as erect, Babington as 1–3 feet high. I have never seen living specimens, but in dead ones which I possess there seem to be, as in P. minus, very minute glands at the base of the ochreæ and perianths. After careful comparison of the ochreæ of this species and P. minus I can find no difference to be relied on.

Syme in English Botany quotes without any expression of doubt Polyg. dubium, Stein, as a synonym of P. mite, and refers to Gren. and Godron. P. dubium, Stein, seems to me to be a well marked variety or sub-species, differing from P. mite in its longer, denser, and *pendulous* spike, and *lanceolate* leaves. Its aspect is that of P. Hydropiper, which species however is at once separated from all its allies by the presence of numerous large and conspicuous glands on the perianth.

Grenier and Godron on the other hand quote Polygonum mite, Schrank, as a synonym of a hybrid plant, viz., P. Hydropiperi—dubium G. and G., and add to this hybrid as another synonym P. hydropiperoides, Mich., Fl. Am. boreal. P. hydropiperoides, Mich., is described by Dr. Asa Gray in his Manual of Botany of United States as "common, especially southward," and he adds as a synonym to this plant, "P. mite, Persoon, *not of Schrank*," and of the two plants supposed by G. and G. to be parents to this, one, viz., P. Hydropiper, is marked in Gray's U. S. Flora as "Naturalized from Europe," and the other parent, viz., P. dubium, Stein, is altogether absent. P. hydropiperoides is nearly related to P. mite, Schrank, but is distinguished by its rough or appressed pubescent leaves.

Polygonum Persicaria, L. is in all the specimens I have seen readily separated from P. mite by its shorter, dense, oblong, uninterrupted spikes; leaves roughish with sparing appressed pubescence; shorter, wider, and more strongly fringed ochreæ, and the nuts as pitchy black, smooth and shining as those of P. minus; in size and form however the nuts exactly resemble those of P. mite.

Accompanying, for sake of comparison, are tracings of leaves of P. minus, P. mite, and P. dubium, also perianths and ripe nuts of the same.

Mr. HARDY remarked that Mr. Hunt's paper was an able résumé of the characters of the allied species of Polygonum, but so far as he could perceive it added nothing to our knowledge on the subject. With respect to the more immediate purport of the paper, viz., the disputed identity of the Mere Mere and Jackson's Boat plants, with the P. minus of Hudson, it would appear from Mr. Hunt's remarks that besides Mr. Baker, two at least of our oldest and most able botanists had failed to differentiate P. minus, and P. mite, when specimens were before them. In support of what was stated at the previous meeting of the Society, as quoted by Mr. Hunt, Mr. Hardy read the following extract from an article in the old *Phytologist* (vol. 2, p. 332), by Mr. H. C. Watson, "*On the Polygonum mite of Schrank and allied species*":—"*Cheshire specimens (of P. mite) are in the Herbarium of Sir W. J. Hooker, sent by Mr. William Wilson, under the name of P. minus (1828); I have also European specimens of the same species, sent with the names of laxiflorum (Weihe), dubium (Braun), Braunii (Bluff and Fing.), and mite (Persoon).*" Mr. Hardy declined to accept Mr. Hunt's dictum that the relative size of the nut furnished the only good character by which to separate the two plants, believing that the size of the flower and the habit of growth, when occurring side by side, as these specimens did, ought not to be passed over; the leaves, too, of the Mere Mere specimen in particular were actually more broadly lanceolate than those of the Oxford and Surrey specimens traced by Mr. Hunt; and both the nuts and flowers larger than any of the other selected minus exhibited by Mr. Hunt, and doubtless correctly named. The presence or absence of glands was, he believed, an important character, but it was requisite, for the observation of these, that the specimens should be freshly gathered.

Mr. Hunt's localities for P. mite in Britain are all southern, but Mr. Baker, in his "North Yorkshire," gives no less than four localities for P. mite; two in the immediate neighbourhood of the city of York, and one as far north as Thirsk.

Mr. Hardy referred incidentally to the notes by the Hon. J. Warren in the January No. of the *Journal of Botany,* on the Mere Mere P. nodosum, and also on the Cheshire Epilobium called obscurum, and stated that Mr. Warren seemed to be going over old ground as regarded these two plants, the former of which he believed to be the seedling form of P. amphibium (terrestre) and the latter identical with the plants published by Mr. Baker in his "Plantæ Criticæ," and North Yorkshire Fasciculus, under the name of E. ligulatum (Baker).

March 27th, 1871.

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

"On some Logs of Oak found in the Irwell Valley Gravels," by JOHN PLANT, F.G.S.

The author described the valley of the river Irwell as being irregular and flat, except in such places where the red sandstone hemmed in the river between high banks, with the river forming several horseshoe curves, both above and below Manchester. The flat meadows are covered with layers of fine sandy loam, without pebbles or large foreign matters; it averages six feet deep, and in heavy floods—especially the one of November, 1866—these flat lands are flooded and a fresh deposit of silt takes place. This silty loam rests upon gravels and sands, but sometimes upon the red rock which forms the basin of the valley from Kearsley Paper Mills on the N.W. to below Carrington where the Irwell joins the Mersey.

These gravels and sands are evidently of estuarine origin, are current bedded with bands of strong ferruginous cement running in them. The pebbles are of moderate size, rarely exceeding 5 inches in diameter, and are smoothed and flattened; sixty per cent are from the coal measure sandstones, the remainder being igneous or metamorphic. There are plenty of Wastdale and Eskdale granites, with a few of the whiter granites of Creefell and Dalbeatie, but no Shap granites; black dolomites, greenstones and basalts, all doubtless derived from the boulder clays which cap the high land bounding the valley. There are no records of bones of extinct mammalia or flint weapons having been found in these sands or gravels. Occasionally a black deposit is found at the top of the gravel, with numbers of the common hazel nuts, and within the gravel fine logs of oak timber have not unfrequently been found. At the present moment three of these logs have been found lying quite near each other, and another was found a few years ago about 300 yards to the south of these. The three logs were under 6 feet of loam and 2 feet of very clean red gravel; they were denuded of bark and the smaller branches all gone, but they were perfectly sound, purplish black and very heavy; only one was exhumed and it measured 25 feet long and 2 feet diameter at the bole. All the holes from which branches were torn were filled with clean gravel.

It is quite probable that they have been originally washed out of beds of peat from the high moorland and brought down by floods to their present position, but it must be referred back to the times when estuarine waters occupied the low lands of Lancashire and Cheshire to the very base of the hills very far to the East of Manchester.

Mr. Plant exhibited and described the finding of a large flint-core in the alluvial deposit near Ordsall Lane Railway Station. The river deposits partake of the same features as are described above, and the flint-core was found at the bottom of a bed of loam nearly five feet from the surface.

Mr. Plant also exhibited fine remains of coal period reptiles, a lower jaw of "*Anthracosaurus*" 15 inches long, dermal plates of *Loxomma*, with a portion of a jaw having five erect teeth, and a scale of a new fish *Megalichthys coccosteus*, and stated that at a future meeting he would exhibit and describe a number of other new reptiles and fishes in his collections from the Manchester coal field.

Annual Meeting, April 18th, 1871.

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

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

The Council have the satisfaction to report that the past year has been one of steady progress for the Society. On the 31st of March, 1870, the general balance of the Treasurer's account was £268 1s. 2d., and on the 31st of March last it had increased to £287 19s. 1½d.

The number of ordinary members on the roll of the Society on the 1st of April, 1870, was 161, and 14 new members have been since elected. The losses during the year have been — deaths, 1; resignations, 3; and defaulters 2. The number on the roll on the 1st of April instant was therefore 169.

In the last annual Report it was stated that in consequence of the rapid increase of the Library of the Society it would be necessary to provide additional accommodation. This has since been done, a new bookcase having been fitted up in the Council room in place of a smaller one which has been removed into the tea room.

The Council refer with pleasure to the fact that the principal result of the recent solar eclipse expedition is due to the energy, intelligence, and skill of a member of the Society, Mr. Alfred Brothers, F.R.A.S., whose beautiful photograph of the solar corona is now generally regarded as having settled the long disputed question as to the real nature of this remarkable phenomenon.

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

October 4th, 1870.—"On Convertent Functions," by Sir James Cockle, F.R.S.

"On the Victoria Cave Exploration," by W. Boyd Dawkins, F.R.S.

October 10th, 1870.—"On the Variations of Abraxas Grossulariata," by Mr. Joseph Sidebotham.

October 11th, 1870.—"On a large Meteor seen at Keswick, August 15th, 1870," by G. V. Vernon, F.R.A.S.

"Observation of the Occultation of Saturn, September 30th, 1870," by Mr. James Fellows.

October 18th, 1870.—"On Sun-spot Curves," by Professor Belfour Stewart, F.R.S.

"On recent Spectroscopic Investigations of the Solar Atmosphere," by Mr. Lockyer, F.R.S.

"Account of an Examination of Offa's Dyke," by W. Boyd Dawkins, F.R.S.

"On the Action of Sulphuric Acid on Diallyl," by W. R. Jekyll, Dalton Chemical Scholar in Owens College. Communicated by Professor Roscoe, F.R.S.

November 1st, 1870.—On the Changes in the Magnetic dip during the Aurora of October 25th, 1870," by Dr. J. P. Joule, F.R.S.

"On the Aurora Borealis of October 25th, 1870," by T. T. Wilkinson, F.R.A.S.

"On the Position of the Centre of the Corona of the Aurora of October 25th, 1870," by J. Baxendell, F.R.A.S.

"On a Method of Taking Casts of Objects of Natural History," by W. Boyd Dawkins, F.R.S.

"Notes on Glacier Moraines in Cumberland and Westmorland," by W. Brockbank, F.G.S.

November 7th, 1870.—"The Hawthorns of the Manchester Flora," by Mr. Charles Bailey.

November 15th, 1870.—"On Improvements in Machines for Cutting and Paring Heavy Articles of Machinery," by W. B. Johnson, C.E.

"On Two Singular Accumulations of Boulder Stones on the Sea Beach at Seascales and Drigg," by E. W. Binney, F.R.S., F.G.S., President.

"On the Temperature Equilibrium of an Enclosure containing a Body in Visible Motion," by Professor Balfour Stewart, LL.D., F.R.S.

November 29th, 1870.—"The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena," by Professor Osborne Reynolds, M.A.

"On Iso-di-Naphthyl," by Watson Smith, F.C.S.

"Notes on the Botany of Mere, Cheshire," by Mr. G. E. Hunt.

December 13th, 1870.—"Some Observations upon Railway Accidents, and Suggestions for preventing their Frequent Occurrence," by W. B. Johnson, C.E.

"Contributions towards a knowledge of Anthophila (Hymenoptera Aculeata) in the Mersey Province," by Mr. F. O. Ruspini. Communicated by H. A. Hurst, Esq.

December 27th, 1870.—"Observation of the Eclipse of the Sun, December 22nd, 1870," by J. B. Dancer, F.R.A.S.

"Notes on some of the High Level Drifts in the Counties of Chester, Derby, and Lancaster," by E. W. Binney, F.R.S., F.G.S., President of the Society.

January 9th, 1871.—"On *Carex flava*, L., and its allies of the Manchester Flora," by Mr. Charles Bailey.

January 10th, 1871.—"On the Postal System and Cotton Trade a century since." Extracted from a memorandum book of Mr. George Walker, one of the original members of the Society, by E. W. Binney, F.R.S., F.G.S., President.

"On the Drift Deposits near Burnley," by T. T. Wilkinson, F.R.A.S.

"Notes on the Effects of Cold upon the Strength of Iron," by W. Brockbank, F.G.S.

"On the Properties of Iron and Steel as applied to the Rolling Stock of Railways," by Sir William Fairbairn, Bart., LL.D., F.R.S., &c.

"On the Alleged Action of Cold in rendering Iron and Steel brittle," by J. P. Joule, D.C.L., F.R.S., &c., Vice-President.

"On the Effect of Cold on the Strength of Iron," by Peter Spence, F.C.S., &c.

January 24th, 1871.—"On the Effect of Cold on the Strength of Iron," by Mr. William H. Johnson.

"Experiments on the Oxidation of Iron," by Professor F. Crace Calvert, Ph.D., F.R.S., &c.

January 30th, 1871.—"On Fossilization," by W. Boyd Dawkins, F.R.S.

"On the Structure of Some Specimens of Stigmaria," by Professor W. C. Williamson, F.R.S.

"On the Cultivation of Madder in Derbyshire," by Joseph Sidebotham, F.R.A.S.

February 7th, 1871.—"On the Organisation of an Undescribed Verticillate Strobilus from the Lower Coal Measures of Lancashire," by Professor W. C. Williamson, F.R.S.

"The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena, Part II.," by Professor Osborne Reynolds, M.A.

"Further Experiments on the Effects of Cold upon Cast Iron," by Peter Spence, F.C.S., &c.

February 21st, 1871.—"The Overthrow of the Science of Electro-Dynamics," by John Hopkinson, D.Sc.

"Remarks on Mr. Spence's Experiments on the Effects of Cold on the Strength of Cast Iron," by Joseph Baxendell, F.R.A.S.

"Further Observations on the Strength of Garden Nails," by J. P. Joule, D.C.L., F.R.S., &c.

"On the Action of Sulphurous Acid on Phosphates," by Dr. B. W. Gerland. Communicated by Dr. R. Angus Smith, F.R.S., &c.

February 27th, 1871.—"Notes on *Polygonum minus* and its allies," by Mr. G. E. Hunt.

March 7th, 1871.—"Further Observations on the Strength of Garden Nails," by J. P. Joule, LL.D., F.R.S., V.P.

"On Anthraflavic Acid, a Yellow colouring Matter accompanying Artificial Alizarine," by Edward Schunck, Ph.D., F.R.S., V.P.

"On the Cotton Trade a Century ago, being further extracts from the memorandum book of Mr. George Walker, one of the original Members of the Society," by E. W. Binney, F.R.S., F.G.S., President.

March 21*st*, 1871.—"On the Mechanical Equivalence of Heat," by the Rev. H. Highton, M.A.

"On Mr. Highton's objections to the Mechanical Equivalent of Heat," by John Hopkinson, D.Sc.

"Examples of the Performance of the Electro-Magnetic Engine," by J. P. Joulo, D.C.L., F.R.S., &c., V.P.

March 27*th*, 1871.—"On some Logs of Oak found in the Irwell Valley Gravels," by John Plant, F.G.S.

April 4*th*, 1871.—"On the Production of Cotton a Century ago." Extracted from a memorandum book of Mr. George Walker, one of the original Members of the Society, by E. W. Binney, F.R.S., F.G.S., President.

"Arsenic in Pyrites and various Products," by H. A. Smith. Communicated by Professor Roscoe, F.R.S.

"On Human Bones obtained from a cave near Llangollen, and from a chambered tomb at Cefn, near St. Asaph," by W. Boyd Dawkins, F.R.S.

"Description of some Experiments on the Method of propelling Balloons, illustrated by a Model," by Professor Osborne Reynolds, M.A.

"Notes on the Drift of the Eastern Parts of the Counties of Chester and Lancaster," by E. W. Binney, F.R.S., F.G.S., President.

The printing of the fourth volume of the Society's third series of Memoirs has been completed, and a new volume has been commenced. Some of the papers in the above list have already been passed for printing in this volume.

During the session now ending an important alteration has been made in the terms of admission of Sectional Associates, the annual subscription having been reduced from one pound to ten shillings, except for those Associates who wish to make use of the Society's Library, who will still continue to pay a subscription of one pound per annum.

The Librarian reports that he has not been able to send any books to be bound in the course of the past year. The Society continues to receive the publications of those societies to whom our own "Memoirs" and "Proceedings" are sent, but the recent war between France and Germany has to some extent interrupted the transmission of the works of many continental societies.

The number of Societies, &c., now in relation with the Society is as follows:—

In England	86
In Scotland	12
In Ireland	10
In British India	7
In Australia and Tasmania	4
In Canada	5
In the United States	28
In France and Algeria	59
In Germany	54
In Belgium	5
In Holland and Luxembourg	16
In Switzerland	9
In Denmark	2
In Sweden	5
In Norway	4
In Italy	14
In Austria and Hungary	14
In Russia	8
In Spain	1
In Portugal	2
In Batavia	2
In the Brazils and Chili	2
Total	349

Showing an increase of 38 in the number at the corresponding period last year.

On the motion of Dr. S. Crompton, seconded by Mr. S. Ogden, the Annual Report was unanimously adopted.

On the motion of Mr. S. Clement Trapp, seconded by the Rev. Brooke Herford, it was resolved unanimously—That the system of electing Sectional Associates be continued during the ensuing session.

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

President.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.

Vice-Presidents.

JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S., &c.
EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, Ph.D., F.R.S., F.C.S.
Rev. WILLIAM GASKELL, M.A.

Secretaries.

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

Treasurer.

THOMAS CARRICK.

Librarian.

CHARLES BAILEY.

Other Members of the Council.

PETER SPENCE, F.C.S., M.S.A.
WILLIAM LEESON DICKINSON.
HENRY WILDE.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
OSBORNE REYNOLDS, M.A.
WILLIAM BOYD DAWKINS, M.A., F.R.S.

THOMAS CARRICK, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER,
FROM MARCH 31ST, 1870, TO MARCH 31ST, 1871.

1871.			£ s. d.	£ s. d.
April 1.	To Balance in the Bank of Heywood Brothers and Co.			366 16 2
	To *Members' Contributions:*			
	Arrears 1870–71		17 17 0	
	Members on the Roll April 1st, 1870, 163 at 42s.	£342 6 0		
	Deduct Compounders 4, Ditto Members deceased 0, Ditto in Arrear March 31st, 1871 5	18 18 0		
			323 8 0	
	13 Members elected in 1870, April to December	27 6 0		
	1 Ditto ditto 1871, Jan. to April	1 1 0		
		28 7 0		
	Less Arrears	7 7 0	21 0 0	
	1 Admission Fee Arrear	2 2 0		
	4 Admission Fees	29 8 0		
		31 10 0		
	Less Arrears	8 8 0	23 2 0	
	7 Associates of Microscopical Section at 10s.		3 10 0	
	1 Ditto Arrear at 10s.		0 10 0	
	1 Associate of Physical and Mathematical Section at 10s. 6d.		0 10 6	389 17 6
	To *Sale of Publications:*			
	Proceedings		0 10 4	
	Memoirs		0 15 6	1 5 10
	To *Sundry Income:*			
	Sectional Contributions:			
	Microscopical		2 2 0	
	Physical and Mathematical		2 2 0	
	Interest allowed by Bankers		5 3 7	9 7 7
				£767 7 1

Composition Fund	£98 15 0
General Balance	287 19 1½
	£386 14 1½

1871.		£ s. d.	£ s. d.
March 31.	By *Charges on Property:*		
	Chief Rent	12 13 0	
	Fire Insurance	3 7 6	
	Inhabited House Duty	6 7 6	
	Property Tax	2 16 8	25 4 8
	By *House Expenditure:*		
	Water, Gas, Candles, and Coal	15 5 4½	
	Cleaning and Petty Expenses	2 14 10	
	Repairs, Painting, &c.	62 6 2½	
	Tea and Coffee at Meetings	16 8 7	96 15 0
	By *Administrative Charges:*		
	Wages of Keeper of Rooms	52 0 0	
	Attendance on Sections	4 4 0	
	Postages and Parcels	16 14 9½	
	Stationery, Printing Circulars, and Receipt Stamps.	10 4 6	83 3 3½
	By *Publishing:*		
	Memoirs, Printing, Engraving, &c.	55 8 0	
	Printing Proceedings	53 8 6	
	Editor of Memoirs and Proceedings	50 0 0	158 16 6
	By *Library:*		
	Periodicals, Binding Books, &c.	15 12 6	
	Subscription to Palæontological Society, 1870	1 1 0	16 13 6
	Total Disbursements		380 12 11½
	By Balance in Bank of Heywood Brothers and Co.	379 13 3	
	In the hands of the Treasurer	7 0 10½	386 14 1½
			£767 7 1

12th April, 1871.
THOMAS CARRICK, TREASURER.

Audited and found correct, April 14th, 1871.
WILLIAM MELLOR.
S. CLEMENT TRAPP.

The following paper was read at the Ordinary Meeting of the Society, held on the 4th of April, 1871 :—

"Notes on Drift of the Eastern Parts of the Counties of Chester and Lancaster," by E. W. BINNEY, F.R.S., F.G.S., President.

Having in a previous paper during this session given a short description of the higher drift found in these counties, we will proceed to consider the thick surface covering of the general drift, which nearly hides from our view the underlying strata, except where they are exposed in river courses, or in canal or railway cuttings. This generally reaches to an elevation of about 700 feet above the sea, and does not alter much in its appearance, whether it is seen at Blackpool, Ormskirk, or Liverpool, or at Burnley, Rochdale, Glossop, or Macclesfield, except being usually more divided as it is found inland, and approaches the sides of the *Pennine* chain. It consists of beds of till, clay, sand, and gravel. We commence with it at Crewe, and follow it by Sandbach, Macclesfield, Poynton, to Stockport, and thence up the valleys of the Goyt and the Etherow, and past Hyde, Dukinfield, Ashton-under-Lyne, Oldham, Rochdale, Heywood, Middleton, Bury, up into Rossendale, by Bolton, Chorley, and Preston, running up to Blackburn, Burnley, and Colne, on the one side, and by Kirkham and Poulton-le-Fylde, to Fleetwood, on the other.

It has been treated on by various authors, a list of whose works are given. The deposits were tracked along the line above named; and betwixt that line and the Irish Sea, the only direct allusion to them was on the line of Railway from Liverpool through Newton, Manchester, Middleton, and Rochdale, to Todmorden, with sections of sinkings and borings on each side of it. This is at the best merely an outline, but it is probably better than attempting to lay down the deposits over a great extent of country, without possessing sufficient data for a map of such changeable deposits as those of the drift are, it being considered more desirable to give an incomplete rather than a made up description. Certainly it is not intended to pretend to lay down the drift deposits over 600 square miles of country, and to predict what beds lie under. Some districts will be more particularly described, because materials are at hand for doing it. The county around Poynton, and High Lane, Stockport, and Brinnington, between Manchester and Oldham, and Manchester and Middleton and Rochdale, near Bury, around Clifton, Swinton, Astley, Leigh, and Ashton-in-Mackerfield. In addition detached sections proved by boring or sinking will be given, and the height above the sea of the localities where it can be ascertained.

In memoirs published in the Society's Transactions,* the order of superposition of the drift beds at Manchester was given by me in the following descending order, namely:—(1) valley gravel; (2) forest sand and gravel containing beds of till and clay; (3) till or boulder clay; (4) lower gravel and sand. In a paper published by the Manchester Geological Society,† "after noticing at length the great value to all classes of society of a correct knowledge of those superficial deposits which were formerly termed diluvium but are now better known by the name of drift, the author describes the whole of the counties from the Irish Sea to the foot of the *Pennine* chain as being more or less covered with different portions of foreign drift so that the underlying strata are only visible in steep escarpments, the great lines of drainage, or in artificial sections. In some places it reaches to heights of 1000 to 1200 feet above the level of

* Vols. VIII. and X. (Second Series).

† Proceedings of the Society for 1842-3, p. 6.

the Irish Sea. The flat surfaces and hollows of underlying rocks appear to have afforded lodgment for it in much higher places than sloping sides of rock at a lower level. Near the shores of the Irish Sea it is very simple, being composed of brown till covered with a thin deposit of fine forest sand, as seen near Ormskirk. At Manchester it is composed of lower gravel, till, and upper sand and gravel, while at Heywood and Poynton, near the base of the *Pennine* chain, the beds of the last named sand and gravel are parted by several beds of loam and clay."

In a Paper published in the Society's Memoirs by Mr., now Professor, Hull, F.R.S.,* that geologist classes the drift and recent deposits of the basin of the Mersey and its tributaries, as follows:—

Recent—1. *Valley gravel and river terraces.*

2. *Upper boulder clay or till,* Bolton, Halshaw Moor, Clifton Moss, Moston, Oldham, Newton Heath, Denton, Cheadle Hulme, &c.

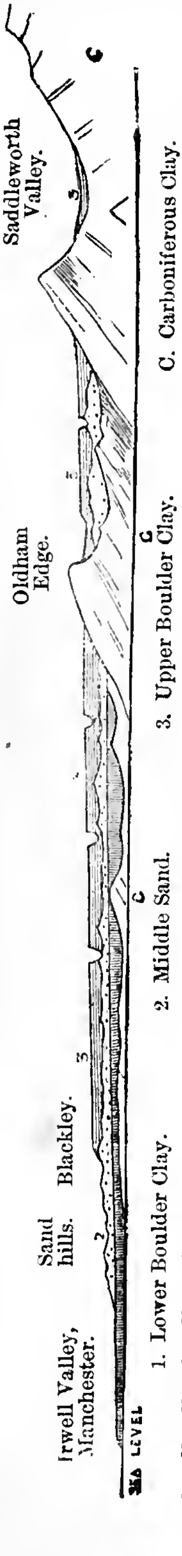
3. *Middle sand and gravel,* Bolton, Pendlebury, Prestwich, Kersal Moor, Heywood, Middleton, Blackley, Gorton, Stockport, Poynton, Wilmslow, Prestbury, Macclesfield, Crewe, &c.

4. *Lower boulder clay or till,* Monton, Salford, Manchester, Heaton Norris, &c.

At page 455 of the same paper the author says, "The middle sand is, unfortunately for its consistency of character, not always free from bands of loam or clay. One of these, which is largely used for brick making near Prestwich, Heywood, and Rochdale, occurs about the centre of the mass, and divides the sand into two main beds, the upper of which frequently occurs in detached hillocks. This bed, however, is of very local occurrence, and thins out southward." At page 458 he gives a general section to shew the arrangement of the drift deposits between Manchester and

* Note, Vol. 2. (Third series), page 431.

Oldham, illustrated by a wood-cut showing the two beds of till and the intervening sand and gravel.



A B C C D

Fig. 3.—Section to show the General Arrangement of the Drift deposits.

At page 451 he states, "We have nowhere been able to discover the lower sand No. 4 *in situ* during our examination."

Professor Hull's classification has been adopted by Mr. De Rance, F.G.S., and other authors connected with the geological survey. In order to test the value of this classification of the drift betwixt Manchester and Oldham, a distance of between six and seven miles, the sinkings and borings of the district have been collected, several of which have been supplied to me by Mr. Bailey, of Honeywell-lane, Oldham. By practical men the term "marl" is used for till, and when the clay is full of pebbles it is called strong, or stony marl. Of course the sinkings are more to be relied on than the borings, but it is surprising how skilled borers by long experience are able to detect small variations in the structure of deposits.

Professor Hull's general section is here given, and the letters A, B, C, and D, are marked upon it to designate the points where the actual sinkings and borings have been made. The line is not a straight one, but makes a considerable curve to the north by Blackley, and my sections are not all on the line, but some to the east and others to the west of it.

A

Strata found in sinking the shaft at St. George's Colliery, Rochdale Road, Manchester. Height above the sea, 183 feet.

	ft.	in.
Till	45	0
Sand and Gravel	10	6
Resting on Red Clays..		
	55	6

B

Sinking at Crumpsall Workhouse. 250 feet.

	ft.	in.
Till [Upper Boulder Clay of Hull]	5	0
Sand	16	0
Quick Sand	26	0
Loam	4	0
Till	30	0
Clay in laminæ	5	0
Till	4	0
Hard Clay	0	6
Sand	2	0
Gravel	7	6
Till	25	0
Sand with small stones resting on Trias Rock		
	125	0

C

Sinking at New Pit, Moston. 325 feet.

	ft.	in.
Soil and Clay	4	0
Marl	21	0
Sand	5	9
Marl	4	0
Sand	0	6
Marl and Loam	9	0
Loam	19	0
Dry Sand	12	0
Quick Sand	2	0
Strong Marl	23	3
Loam	8	6
Strong Marl	9	0
Loam	2	0
Quick Sand	15	6
Strong Marl	21	6
Clay	1	0
Gravel	16	0
Resting on Coal Measures.		
	174	0

C

Mr. Wood's Bore above Medlock Vale. 250 feet.

	ft.	in.
Soil	1	0
Till	21	0
Sand	6	0
Till	13	0
Sand	2	6
Till	14	0
Sand	7	0
Till	2	0
Sand	2	0
Till	27	6
Brown Metals		
	97	0

C

Boring at Mr. Walmsley's, near Failsworth Pole. 327 feet.

	ft.	in.
Soil	1	2
Clay	3	0
Soft Marl	12	4
Loamy Sand	1	0
Marl	3	0
Sand	0	5
Marl	9	2
Marl with sand beds	3	0
Sand	0	9
Marl	42	0
Sand	1	6
Hard Sand	17	10
Loamy Marl	0	10
Marl	10	5
Loamy Marl	7	7
Black Sand	3	4
Loamy Marl	4	6
Red Marl resting on red sandstone	5	4
	127	0

C

Sinking at Bower Colliery, (No. 2 Pit), near Hollinwood. 350 feet.

	ft.	in.
Marl	19	0
Quick Sand	1	6
Solid Marl	64	0
Sand and Gravel	3	0
	87	6

C

Boring at Lymeditch, Failsworth. About 350 feet.

	ft.	in.
Marl	16	6
Loamy Sand	0	3
Wet Sand	1	0
Soft Marl	1	5
Wet Sand	1	7
Marl	4	3
Dry Loam	2	10
Soft Marl	5	0
Marl	4	0
Loamy Sand	2	0
Soft Marl	1	0
Loamy Sand	1	6
Quick Sand	1	0
Marl	3	0
Marl and Sand	0	9
Loam	1	1
Strong Gravel	2	2
Strong Marl	40	9
Loamy Marl	7	11
Soft Smooth Marl	6	2
Wet Sand	1	0
Quick Sand	2	3
Loamy Marl	1	8
Quick Sand	2	6
Resting on Coal Measures		
	112	7

D

Sinking at Park Colliery, near Oldham. 550 feet.

	ft.	in.
Soil and Clay	6	0
Strong Blue Marl	24	0
Sand and Gravel	7	0
Strong Marl with small gravel beds	63	0
Sand and Gravel resting on rock	6	0
	106	0

D

Bore near the Middleton Junction Railway Station. 350 feet.

	ft.	in.
Soil	1	6
Gravel	2	0
Loamy Sand	18	0
Marl	9	0
Dry Sand	17	6
Wet Sand	10	6
Marl	5	2
Wet Sand	21	9
Wet Grey Sand	9	0
Loamy Marl	12	7
Wet Sand	5	9
Marl	0	3
Wet Sand	5	0
Marl	0	2
Wet Sand	5	8
Marl resting on Coal Measures	2	8
	126	6

D

Sinking at Hartford Colliery, Werneth. 550 feet.

	ft.	in.
Soil and Clay	6	0
Marl	27	0
	33	0

D

Sinking at Honeywell Lane Pit. 580 feet.

	ft.	in.
Soil and Clay	6	0
Strong Marl	48	0
	54	0

The district near Chamber Hall lying betwixt Hartford and Park collieries is almost free from drift, and the Chamber Hall rock can be seen cropping out to the surface with very little cover upon it.

The sinking at St. George's Colliery may be a little to the east of Mr. Hull's line of section, but the beds there are about the same as those which are met with in the brick-yards of Cheetham; and from the last-named place by Crumpsall Workhouse to Moston Colliery, leaving Mr. Wood's section above Medlock Vale, and Mr. Walmsley's at Failsworth to the east, thence by Bower Colliery, leaving Lymeditch to the east, up to Park sinking, leaving Middleton Junction and Hartford to the west, and Honeywell-lane to the east, are nearly upon his line.

To me it appears that the drift beds found between Manchester and Oldham cannot well be classed under the triple division which Professor Hull has adopted, although it would doubtless simplify matters if it could be done. However that may be, we have nothing left but to take the deposits as we find them in sinkings and borings, which certainly are not always to be relied on, still they are better to trust to than leisurely walking over the ground and making ideal sections.

The drift deposits are so variable, and our knowledge of them inland so limited, that at present any classification should be regarded as provisional, and the intercalation of beds of sand and gravel in the till, instead of two or more beds of till with the sands and gravels all packed between them, will probably be more convenient, as it will enable us to include the lower sand and gravel which, although often absent, is sometimes found under the till.

In other districts numerous sinkings and borings in the drift are given for the purpose of showing the nature of the deposits, similar to what has been done between Manchester and Oldham, and these, on the whole, appear to show that the thick bed of till has a tendency to divide into several beds, parted by sands and gravels, generally to the north and east. This is especially seen near Manchester, where the middle sand and gravel at its junction with the thick bed of till is not observed overlying it, but cropping out from under it.

At the Annual Meeting of the Society, held on the 18th of April, 1871,

Dr. Joule, F.R.S., drew attention to the remarkable atmospheric phenomenon which had been seen by several persons in Derbyshire and elsewhere, on the evening of Good Friday, April 7th, and stated that he had witnessed a similar appearance near Glasgow, on the day before it was observed in this neighbourhood. The perpendicular ray extended upwards from the sun to an altitude of 30°, and was very clearly defined. It was observed from half an hour before, until after the sun had set. The phenomenon was also witnessed, at the same time, by Professor J. Thomson, who was sailing on the Firth of Clyde.

PHYSICAL AND MATHEMATICAL SECTION.

March 28th, 1871.

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

Mr. Brothers, F.R.A.S., after some preliminary remarks as to the chief objects of the English Eclipse Expedition to Sicily, in December last, exhibited on the screen a series of photographs illustrating, in the first instance, the means adopted for obtaining photographs of the eclipse. A series of pictures was then shown illustrating the corona as photographed during the eclipses in 1860, 1868, and 1869, and the whole of the pictures taken during the totality in 1870 at Syracuse. These were shown in comparison with the pictures taken in Spain by the American observers, and also with sketches taken by members of the English expedition in Spain. These illustrations showed in a remarkable manner the advantages of photography in depicting phenomena such as are seen during eclipses of the sun—the strict identity of the positions of the rifts or dark spaces in the corona being shown most perfectly. The identity of those rifts was also shown by comparison with a drawing made by Professor Watson at Carlentini, in Sicily.

During the evening Dr. Roscoe, F.R.S., explained the importance of spectrum analysis in investigating the solar phenomena, and described the preparations he had made for viewing the eclipse from his station on Mount Etna, about 5,000 feet above the sea level.

The photographs and other illustrations were exhibited by means of Mr. T. Harrison's powerful electric apparatus.

Annual Meeting, April 25th, 1871.

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

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

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

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

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

Treasurer.
THOMAS CARRICK.

"Performance of the Electro-Magnetic Engine," by the Rev. H. Highton, M.A.

1. I am sure that Dr. Joule is a sufficient lover of truth not to feel offended if I submit his last explanations on this subject to the test of facts and experiments.

Not having myself the means of trying experiments with sufficient accuracy to venture to publish results, I will make use only of M. Favre's and Dr. Joule's own experiments.

2. The whole basis of Dr. Joule's new reasoning is, that when a magnetic engine works rapidly, less heat is evolved and more absorbed in it. Taking his own figures as a basis, I find he calculates that when the current is reduced to one-half what it was when the engine was at rest, the heat evolved *per hour* would be one quarter, and *per equivalent of zinc* consumed one half. Now let us take the measurement by M. Favre's calorimeter ("Comptes Rendus," vol. XLV.) When the engine was at rest, the calorimeter in which it was placed showed, per equivalent of zinc consumed, 2,219 heat-units. When it worked slowly, raising a

weight, it evolved 2,947 units, and when it worked rapidly, raising no weight, it showed 4,769 units. In that case, therefore, the effect was exactly the reverse of what Dr. Joule supposes.

3. I have taken Dr. Joule's own experiments described in the "Philosophical Magazine," vol. XXVIII., and have calculated, on the data he gives, what weight ought to have been raised in each experiment. I subjoin a table of what should have been raised and what actually was raised :—

Exp.	Foot pounds calculated.	Actual.
I.	19,377	21,100
II.	18,628	17,820
III.	11,533	8,800
IV.	11,232	9,000
V.	17,416	10,031
VI.	18,033	12,673

In order that any mistake I may have made may be detected, I subjoin the calculations themselves in an appendix. The numbers are calculated on the supposition that the resistance of the battery could be neglected. If Dr. Joule in those experiments acted on the erroneous principle, into which he was misled by Jacobi, that the resistance of the wire should be equal to that of the battery, each of the above calculated numbers should be reduced by one half, which in every case would make the work actually done considerably more than the calculation, in some cases more than double.

4. Let me next observe that if, instead of a wire 389 feet long and $\frac{1}{16}$th of an inch in diameter, Dr. Joule had taken one which was half the length and half the section, all his figures would apply equally well; or, again, a wire of German silver a quarter the length and with four times the resistance; or, to take an extreme case, a wetted cord enclosed in an insulating tube $\frac{1}{100}$th part the length, and with 100 times the resistance.

5. Dr. Joule when he demonstrates that there is no variation in economy, whatever the arrangement of the conducting metal, or whatever the size of the battery, demonstrates too much; for it would follow from this, that no diminution in the size of the battery would affect the result injuriously. Why then should he prescribe a battery of such a size that the resistance may be neglected? Take, again, his own figures in the 4th vol. of "Sturgeon's Annals." By reducing his battery from 40 pairs to 10 he increased his economic duty from 65·00 to 97·00 or 50 per cent.

6. But in reality Dr. Joule's views in 1840 were more correct than they are now; for he was then not so far committed to the theory of the mechanical equivalence of heat. In 1840 he allowed that an economy could be effected by increasing the quantity of wire. Let us see whether he *was* right then, or whether he *is* so now. In his experiments in 1845 he used twice as much wire in his first two experiments as in his four last. What was the consequence? In his first two experiments the average of foot-pounds per hour was 19,460, and duty per grain of zinc 98·35. In the four last experiments the average per hour 10,125, and duty per grain of zinc 51·8, thus clearly showing that doubling the quantity of wire nearly doubled both the actual work done, and the economical duty.

7. Again, a comparison of his two first experiments would tend to prove that reducing the intensity below one half reduces not only the work done per hour, but the duty also, instead of increasing it. For the full current being 2,232, a reduction to 920 produced 21,100 foot-pounds of work per hour, and a duty of 102·9; whereas a reduction to 850 (by increasing the turns from 140 to 180 per minute) produced only 17,820 foot-pounds of work per hour, and a duty of only 93·8, or nearly 10 per cent. less.

8. Dr. Joule's accurate and most valuable experiments are still the great storehouse of facts on this subject. What I contend is, that they disprove, not prove, his own theory.

APPENDIX.

Calculations, according to Joule's present theory, for his experiments in 1845.

N.B. The calculations are only close approximations, and not perfectly accurate.

FIRST EXPERIMENT.

I. When the engine was at rest, current in B A units, 1,296.

1. Heat evolved per hour by the wire = 102·89 units.
2. Consumption of zinc per hour, 497·7 grains.
3. Heat due to 497·7 grains, 102·89 units.
4. Work per hour (102·89 – 102·89)772 = 0.
5. Work per grain of zinc = 0.

II. The engine was then started, and kept to a velocity which reduced the current to ·5338.

1. Heat evolved per hour by the wire, $102{\cdot}89 \times \left(\frac{{\cdot}5338}{1{\cdot}296}\right)^2 = 17{\cdot}15$ units.
2. Consumption of zinc per hour, $497{\cdot}7 \times \frac{{\cdot}5338}{1{\cdot}296}$ = about 205 grains.
3. Heat due to 205 grains, $102{\cdot}89 \times \frac{{\cdot}5338}{1{\cdot}296} = 42{\cdot}25$ units.
4. Work per hour $(42{\cdot}25 - 17{\cdot}15) \times 772 = 19.377$ ft. lbs.

SECOND EXPERIMENT.

III. The current is reduced to ·495.

1. Heat evolved per hour by the wire, $102{\cdot}89 \times \left(\frac{{\cdot}495}{1{\cdot}296}\right)^2 = 15{\cdot}15$.
2. Consumption of zinc per hour, $49{\cdot}77 \times \frac{{\cdot}495}{1{\cdot}296}$ = about 190 grains.
3. Heat due to 190 grains, $102{\cdot}89 \times \frac{{\cdot}495}{1{\cdot}296} = 39{\cdot}28$.
4. Work per hour $(39{\cdot}28 - 15{\cdot}15) \times 772 = 18{,}628$ ft. lbs.

THIRD EXPERIMENT.

IV. When the engine was at rest, current ·806.

1. Heat evolved per hour, 63·88 units.
2. Consumption of zinc per hour, 309 grains.
3. Heat due to 309 grains, 63·88 units.
4. Work, 0.

V. The current is now reduced by the velocity to ·495.

1. Heat evolved per hour by the wire, $63·88 \times \left(\frac{·495}{·806}\right)^2 = 24·34$ units.

2. Consumption of zinc per hour, $309 \times \frac{·495}{·806}$ = about 190 grains.

3. Heat due to 309 grains = 39·28 units.

4. Work per hour (39·28 – 24·34)772 = 11,533 ft. lbs.

FOURTH EXPERIMENT.

VI. The current was now reduced to ·3934.

1. Heat evolved by the wire, $63·88 \times \left(\frac{·3934}{·806}\right)^1 = 15·25$ units.

2. Consumption of zinc per hour, $309 \times \frac{·3934}{·806} = 151$ grains.

3. Heat due to zinc = 31·21 units.

4. Work per hour (31·21 – 15·25)772 = 11·232 ft. lbs.

FIFTH EXPERIMENT.

VII. When the engine was at rest the current was 1·211.

1. Heat evolved per hour by wire, 96·07 units.

2. Consumption of zinc per hour, 465·5.

3. Work, 0.

VIII. Current reduced to ·757.

1. Heat evolved by the wire, $96·07 \times \left(\frac{·757}{1·211}\right)^2 = 37·6$.

2. Consumption of zinc per hour = 291 grains.

3. Heat due to 291 grains = 60·16.

4. Work per hour (60·16 – 37·6) × 772 = 17,416 ft. lbs.

SIXTH EXPERIMENT.

IX. When the engine was at rest, current 1·182.

1. Heat evolved per hour by the wire, 94·15 units.

2. Consumption of zinc per hour, 453·4 grains.

3. Heat due to 453·4 grains, 94·15.

4. Work, 0.

X. When the current was reduced to ·58.

1. Heat evolved by the wire, $94·15 \times \left(\frac{·581}{1·182}\right)^2 = 22·74$.

2. Consumption of zinc per hour = 223 grains.

3. Heat due to 223 grains = 46·103 units.

4. Work per hour should be (46·103 – 22·74)772 = 18,033 ft. lbs.

Dr. Joule said in noticing Mr. Highton's remarks,—I would refer him to my paper on the calorific effects of magneto-electricity and the mechanical value of heat, published in the *Phil. Mag.* for 1843, vol. 23. He will there find it stated, pp. 274, 351, as the result of my experiments, that the heat evolved by the wire of my revolving electro-magnet varied with the square of the current passing through it, and was not affected by the resistance presented by magneto-electric induction in consequence of the working of the electro-magnetic engine. This fact is the basis of my reasoning in that paper, and the neglect of it has involved Mr. Highton in error, as may be seen in his reply to Mr. Apjohn's most lucid exposition of the true theory in the *Chemical News*, vol. 23, p. 105.

The correctness of Jacobi's formula for the proper arrangement of the wires of a battery to produce the maximum magnetic effect is not necessarily connected with the subject of economy of work. Nevertheless I may refer Mr. Highton to my experiments on this subject in Sturgeon's Annals for 1839, by which I showed that the attraction of electro-magnets for one another is proportional to the square of the current between very wide limits.

Mr. Highton seems to forget, when commenting upon the large amount of duty obtained in the first two experiments of my paper with Scoresby, that in them the theoretical duty was also greater than in the others, owing to the current being worked down to a lower intensity. Another reason, explained in the paper was, that in those two experiments, recently mixed, and therefore hot solutions were used in the battery, the potential of which was thereby increased.

Mr. Highton is in error if he supposes that my paper in the Proceedings for March 21 contains a new theory, or that I have abandoned any of my original views. My object in that paper was simply to place the true theory in a form in which it might be easily understood by those who have not worked on the subject.

"On a Diurnal Inequality in the Direction and Velocity of the Wind, apparently connected with the daily changes of Magnetic Declination," by JOSEPH BAXENDELL, F.R.A.S.

In a Paper with the above title read before the Section on the 5th January, 1867, I gave the results of a discussion of the Anemograph observations made at the Radcliffe observatory, Oxford, during the eight years 1859—66, and showed that the differences between the bi-hourly directions and velocities of the wind indicated the operation of a force acting in a direction from magnetic west to east during the hours of from about 7 a.m. to 9 p.m., and producing its maximum effect from 1 to 2 p.m. The Anemograph results for 1867, have since been published in the volume of "Radcliffe Observations" for that year; and a copy of the unpublished results for 1868, having been kindly forwarded to me by the Rev. R. Main, F.R.S., the Radcliffe Observer, I have combined the results of these two years with those of the previous eight years, and thus obtained the following mean results for the entire period of ten years, 1859-68:—

h	Mean Direction. °	′	Mean Bi-horary Velocity.	h	Mean Direction. °	′	Mean Bi-horary Velocity.
0	222	30	15·88	12	214	14	12·38
2	223	33	16·02	14	214	32	12·27
4	221	37	15·00	16	214	47	12·28
6	218	23	13·53	18	216	28	12·26
8	214	36	12·78	20	217	9	13·01
10	213	32	12·58	22	219	54	14·62

These results, like those for the eight years, 1859-66, show that during the night little or no change takes place in the direction and velocity of the wind; about 7 a.m. both the angle of direction and the velocity begin to increase, and attaining their maxima a little before 2 p.m. they afterwards gradually diminish until about 9 p.m., when they again resume their night values.

The rectangular co-ordinates A and B of these directions and velocities, taken in the direction of the meridian, and at right angles to it, and the differences between the individual and the mean values of A and B are shown in the following table:—

h.	A	Difference from Mean.	B	Difference from Mean.
0	11·71	+1·04	10·73	+2·41
2	11·61	+0·94	11·04	+2·72
4	11·21	+0·54	9·96	+1·64
6	10·60	—0·07	8·40	+0·08
8	10·52	—0·15	7·26	—1·06
10	10·49	—0·18	6·95	—1·37
12	10·24	—0·43	6·97	—1·35
14	10·11	—0·56	6·96	—1·36
16	10·09	—0·58	7·01	—1·31
18	9·86	—0·81	7·29	—1·03
20	10·37	—0·30	7·86	—0·46
22	11·22	+0·55	9·38	+1·06

Now if the view I took in my former paper is correct, and the mean daily movement of the wind is due to two forces,—one constant, or nearly so, both in direction and intensity, and the other constant in direction, but variable in intensity, and acting during only a portion of the day, then the sums of the differences in columns 3 and 5 of the above table, taken without reference to sign, will be the rectangular co-ordinates of the angle of direction, and total amount of movement caused by the action of the variable force. These sums are:—

A differences = 6·15.

B differences = 15·85.

And the resulting angle of direction = 248° 48′, and the total movement = 17·00 miles.

The mean magnetic declination at Greenwich during the period of 1859-68 was 20° 46′ W.; and as the declination is about 40′ greater at Oxford than at Greenwich, the mean value for this period at Oxford would be 21° 46′ W., or the angle of magnetic west would be 248° 34′. The calculated angle of direction of the disturbing force differs therefore only 14′ from that of magnetic west.

Deducting the sums of the A and B differences from the sums of the A and B values, we have the co-ordinates of the direction and movement of the wind due to the action of the constant force. These are 121·88 and 83·96, and the bi-horary values = 10·156 and 6·997. The amounts for a period of 10 hours will therefore be 50·78 and 34·98; but the amounts actually observed during the 10 hours, from 9 p.m. to 7 a.m. are 50·79 and 35·18, and we are therefore entitled to conclude that the disturbing force is almost, if not altogether, inoperative during this interval.

The rate of increase and decrease of the variable force will be seen from the following numbers:—

h.		
20	+0·21	+0·86
22	1·06	2·38
0	1·55	3·73
2	1·45	4·04
4	1·05	2·96
6	0·44	1·40
8	0·36	0·26

An examination of these numbers shows that the intensity of the force increases most rapidly about the time when the north pole of the magnetic needle is moving most rapidly to the westward. It is at its maximum when the needle is at its greatest elongation west; and its greatest rate of decrease occurs at the time when the needle is moving most rapidly to the eastward. These coincidences and the fact that the force acts in the direction of a perpendicular to the magnetic meridian, seem to indicate very clearly that it is directly connected with the forces which cause the daily changes of magnetic declination.

In my paper "On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface," read before the Society, on the 8th of March, 1864, I suggested that changes in the magnetic condition of the earth might produce corresponding changes in the directions of the great currents of the atmosphere, and as the changes in some of the magnetic elements take place in a period corresponding with that of solar spot frequency, it occurred to me to examine whether the mean direction of the wind at Oxford, in different years, had any relation to the number of spots observed on the sun's disk. The mean direction in each year of the series was

1859	S. 53½° W.	1864	S. 20° W.
1860	70½	1865	16
1861	51	1866	12½
1862	51½	1867	24
1863	41	1868	38

It appears therefore that the greatest angle of direction occurred in 1860, which was a year of maximum solar spot frequency; and the least angle in 1866 when solar spots were least numerous. I had noticed this remarkable coincidence when I wrote my paper in 1868, but did not mention it in the paper, as I thought it better to wait for the publication of the observations for another year or two, to see whether the angle of direction of the wind would increase after the period of solar spot frequency had passed its minimum. The above numbers show that an increase has taken place, and I therefore now feel justified in drawing attention to the subject as one of considerable importance in its bearing upon meteorological science, and also upon that of terrestrial magnetism.

In conclusion, I may mention as a noteworthy fact that although observations of the direction and velocity of the wind have, for many years, been made at various observatories in this country, and elsewhere, with self-recording instruments, I have not yet been able to meet with any, save these made at Oxford, the results of which are published in a form available for the purposes of an investigation similar to the one which forms the subject of this paper. This is however only one of many instances that might be adduced to show how important it has become to effect a reform of the unsatisfactory system which has so long and so generally been pursued in publishing the results of meteorological observations.

"On the Rainfall at Old Trafford, Manchester, during the year 1870," by G. V. Vernon, F.R.A.S., F.M.S.

The rainfall for 1870 was remarkable for its very irregular distribution over the year. The total amount collected was 5·988in. below the average of the last seventy-seven years: it fell upon 155 days, or upon 55 days less than in 1869.

During the first, second, and third quarters of 1870, the rainfall was below the average of the corresponding periods of the last seventy-seven years: whilst the fourth quarter was in excess, but by no means making up for the previous deficiency.

January, March, April, and October, had a rainfall in excess of the average, the amount in October being very exceptional.

February, May, June, July, August, September, November, and December had a rainfall below the average.

The rainfall of October was very remarkable, no such an amount having fallen in this district between 1794 and 1870, the largest amount registered being 7·793in. in 1843.

In every year in which the rainfall has been above five inches in October, since 1794, excepting 1804, the rainfall for the year has considerably exceeded 30·000in., and it appears to be very unusual to have a dry year with a very excessive rainfall in October.

OLD TRAFFORD, MANCHESTER.

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

Quarterly Periods. 1869. Days.	Quarterly Periods. 1870. Days.	1870.	Fall In Inches.	Average of 77 years.	Difference.	No. of Days Rain-fall in 1870.	Quarterly Periods. 77 Years. Inches.	Quarterly Periods. 1870. Inches.	Difference.
		Jan. ...	3·131	2·505	+0·626	18			
56	41	Feb. ...	0·856	2·397	—1·541	14	7·199	6·365	—0·834
		March.	2·378	2·297	+0·081	9			
		April...	2·217	2·031	+0·186	10			
44	35	May ...	0·746	2·316	—1·570	9	7·030	4·753	—2·277
		June...	1·790	2·683	—0·893	16			
		July ...	0·809	3·515	—2·706	10			
48	32	August	1·652	3·534	—1·882	8	10·312	5·118	—5·194
		Sep. ...	2·657	3·263	—0·606	14			
		Oct. ...	8·363	3·877	+4·486	23			
62	47	Nov....	2·420	3·814	—1·394	12	10·998	13·315	+2·317
		Dec....	2·532	3·307	—0·775	12			
210	155		29.551	35.539	—5.988	155	35·539	29·551	—5·988

"On the Rainfall at Old Trafford, Manchester, and Comparison with the Average of Twenty Years and Seventy-seven Years," by G. V. VERNON, F.R.A.S., F.M.S.

In a paper published in Volume I. of the third series of the Society's Memoirs, I gave the rainfall for Old Trafford, for the years 1850 to 1860, and I beg now to submit to the society a continuation of the same down to the end of 1870, making a period of ten years.

The period 1850 to 1860 was unfortunately very incomplete, owing to the month of August being deficient in the first six years of the period. The series I now submit is complete throughout the period of ten years.

In the first place I annex a comparative statement of the various periods.

MONTH.	1850–1860	1861–1870	1850–1870	1794–1870
January	2·778	2·659	2·719	2·505
February	1·899	2·412	2·156	2·397
March	1·925	2·356	2·131	2·297
April	1·848	1·983	1·915	2·031
May	1·808	2·112	1·953	2·316
June	3·310	2·286	2·798	2·683
July	2·869	2·547	2·713	3·515
August*	4·806	2·913	3·454	3·534
September	2·745	4·014	3·349	3·263
October	3·280	4·191	3·785	3·877
November	2·611	3·245	2·928	3·814
December	2·786	3·347	3·053	3·307
SUMS	32·665	34·165	32·954	35·539

* 1850 to 1860, 4 years only.

This table evidently points out the fact that the period 1850 to 1860 was drier than the period 1860 to 1870, and would have shown it much more so if the observations for August had been complete during the earlier period, as the average of the four years 1857, 1858, 1859 and 1860 is quite an abnormal value; the rainfall in this month being excessive in each of these years.

During the last ten years the smallest amount of rain occurred in 1865, the amount being 29·389in. falling upon

164 days; the largest amount occurred the following year 1866, the amount being 43·169 falling upon 214 days.

From the longer period of observations, 1794 to 1870, it seems to be quite well established that in this district the minimum rainfall occurs in April, and the maximum in October, and but for the departure from symmetry in September we should have a simple curve with one maximum and one minimum.

In October, 1870, the largest fall of rain for the month occurred of any October between 1794 and 1870, and during this entire period there were only two months approaching so large a fall, viz., July, 1828, 11·280ins., and August, 1799, 8·740ins. (Society's Memoirs, second series, Vol. XV.)

The summer months of 1868, 1869, and 1870 were excessively dry. The four months of May, June, July, and August had a total rainfall of

1868, 4·194ins.; 1869, 7·612ins.; 1870, 4·997ins.

The average for seventy-seven years being 12·048ins., showing a very great falling off, especially in 1868 and 1870.

In trying to trace some law of periodicity in this long series of rainfall observations I have not been able to find any defined period.

Going back to 1786, it would appear from Mr. Walker's rainfall returns, 1786—1793, that the heaviest rainfalls did not occur at the period of lowest annual mean temperature, but a few years later; but the irregularities are so great that some other method must be adopted to reduce the irregularities of the temperature and rain curves for the year: perhaps taking 5 yearly means might enable a better comparison to be made, that is, the year itself and the two preceding and two following ones in each case.

RAINFALL AT OLD TRAFFORD, MANCHESTER.

Year.	January.			February.			March.		
	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.
	In.	In.	In.	In.	In.	In.	In.	In.	In.
1861	0·388	—2·331	—2·117	2·522	+0·366	+0·125	4·143	+2·012	+1·846
1862	1·896	—0·823	—0·609	0·958	—1·198	—1·439	3·669	+1·538	+1·372
1863	4·425	+1·706	+1·920	0·941	—1·215	—1·456	0·804	—1·327	—1·493
1864	1·684	—1·035	—0·821	4·027	+1·871	+1·630	2·011	—0·120	—0·286
1865	3·112	+0·393	+0·607	2·357	+0·201	—0·040	1·674	—0·457	—0·623
1866	3·252	+0·533	+0·747	2·983	+0·827	+0·586	2·168	+0·037	—0·129
1867	3·270	+0·551	+0·765	2·930	+0·774	+0·533	1·446	—0·685	—0·851
1868	2·746	+0·027	+0·241	2·114	—0·042	—0·283	3·999	+1·868	+1·702
1869	2·686	—0·033	+0·181	4·436	+2·280	+2·039	1·270	+0·039	—1·027
1870	3·131	+0·412	+0·626	0·856	—1·300	—1·541	2·378	+0·247	+0·081
Mns	2·659	2·719	2·505	2·412	2·156	2·397	2·356	2·131	2·297

Year.	April.			May.			June.		
	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years,
	In.	In.	In.	In.	In.	In.	In.	In.	In.
1861	2·426	+0·511	+0·395	0·734	—1·219	—1·582	2·412	—0·386	— 0·271
1862	2·717	+0·802	+0·686	4·470	+2·517	+2·154	3·072	+0·274	+0·389
1863	1·391	—0·524	—0·640	1·724	—0·229	—0·592	4·631	+1·833	+1·948
1864	1·602	—0·313	—0·429	3·175	+1·222	+0·859	2·945	+0·147	+0·262
1865	1·082	—0·833	—0·949	3·187	+1·134	+0·871	0·957	—1·841	—1·726
1866	0·299	—1·616	—1·732	1·540	—0·413	—0·776	3·975	+1·177	+1 292
1867	4·323	+2·408	+2·292	1·950	—0·003	—0·366	1·591	—1·207	—1·092
1868	1·676	—0·239	—0·355	0·872	—1·081	—1·544	0·368	—2·430	—2·315
1869	2·096	+0·181	+0·065	2·726	+0·773	+0·410	1·122	—1·676	—1.561
1870	2·217	+0·302	+0·186	0·746	—1·207	—1·570	1·790	—1·008	—0·893
M'ns.	1·983	1·915	2·031	2·112	1·953	2·316	2·286	2·798	2·683

Year.	July.			August.			September.		
	Rain-fall.	Differ. of 21 years.	Differ of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.
	In.	In.	In.	In.	In.	In.	In.	In.	In.
1861	3·646	+0·933	+0·131	2·232	—1·222	—1·302	4·050	+0·701	+0·787
1862	4·527	+1·814	+1·012	2·350	—1·104	—1·184	4·998	+1·649	+1·735
1863	1·630	—1·083	—1·885	5·027	+1·573	+1·493	5·559	+2·210	+2·296
1864	1·687	—1·026	—1·828	2·367	—1·087	—1·167	4·009	+0·660	+0·746
1865	2·996	+0·283	—0·519	3·840	+0·386	+0·306	0·666	—2·683	—2·597
1866	4·309	+1·596	+0·794	5·119	+1·665	+1·585	7·128	+3·779	+3·865
1867	4·284	+1·571	+0·769	1·410	—2·044	—2·124	2·990	—0·359	—0·273
1868	0·454	—2·259	—3·061	2·500	—0·954	—1·034	1·760	—1·589	—1·503
1869	1·131	—1·582	—2·384	2·633	—0·821	—0·901	6·320	+2·971	+3·057
1870	0·809	—1·904	—2·706	1·652	—1·802	—1·882	2·657	—0·692	—0·606
M'ns.	2·547	2·713	3·515	2·913	3·454	3·534	4·014	3·349	3·263

Year.	October.			November.			December.		
	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.	Rain-fall.	Differ. of 21 years.	Differ. of 77 years.
	In.	In.	In.	In.	In.	In.	In.	In.	In.
1861	1·230	—2·555	—2·647	3·878	+0·950	+0·064	2·066	—0·987	—1·211
1862	5·035	+1·250	+1·158	1·685	—1·243	—2·129	3·221	+0·168	—0·086
1863	6·242	+2·457	+2·365	2·902	—0·026	—0·912	3·064	+0·011	+0·057
1864	1·908	—1·877	—1·969	3·255	+0·627	—0·559	1·970	—1·083	—1·337
1865	5·005	+1·220	+1·128	2·770	—0·158	—1·044	0·743	—2·310	—2·564
1866	2·521	—1·264	—1·356	5·721	+2·793	+1·907	4·154	+1·101	+0·847
1867	3·979	+0·194	+0·102	2·431	—0·497	—1·383	3·975	+0·922	+0·668
1868	4·505	+0·720	+0·628	3·108	+0·180	—0·706	8·123	+5·070	+4·816
1869	3·119	—0·666	—0·758	4·284	+1·356	+0·470	3·623	+0·570	+0·316
1870	8·363	+4·578	+4·486	2·420	—0·508	—1·394	2·532	—0·521	—0·775
M'ns.	4·191	3·785	3·877	3·245	2·928	3·814	3·347	3·053	3·307

Yearly Fall.

	In.	Days.		In.	Days.
1861	29·727	199	1866	43·169	214
1862	38·598	218	1867	34·579	188
1863	38·340	215	1868	32·225	188
1864	30·640	171	1869	35·446	197
1865	29·389	164	1870	29·551	155

"Results of Rain-Gauge Observations made at Eccles, near Manchester, during the year 1870," by Thomas Mackereth, F.R.A.S., F.M.S.

It will be seen from the table I present below that the rainfall of the past year was in some respects very similar to that of the two previous years, viz., that the excesses happened in the coldest months of the year, and long droughts in the hottest months. The individual characteristic of this year's fall is the quantity collected in the month of October. This is the largest amount I have measured in any month during the last ten years. It will be seen that it reached 8·900 inches. The total fall of the year is 30·404 inches, or 3·975 inches below the average of the last ten years. The driest month of the year was July, in which 0·775 inch of rain was measured. The following table shows the results obtained from a rain-gauge with a 10in. round receiver, placed 3 feet above the ground.

Quarterly Periods.		1870.	Fall in Inches.	Average of 10 years.	Differences.	Quarterly Periods.	
Average of 10 years.	1870.					Average of 10 years.	1870.
Days.	Days.						
51	44	January	3·127	2·681	+0·446	7·534	6·547
		February	0·949	2·292	—1·343		
		March	2·471	2·561	—0·090		
43	38	April	2·049	1·968	+0·081	6·417	4·866
		May	0·899	2·052	—1·153		
		June	1·918	2·397	—0·479		
48	41	July	0·775	2·550	—1·775	9·647	4·949
		August	1·828	3·109	—1·281		
		September	2·346	3·988	—1·642		
56	55	October	8·900	4·181	+4·719	10·781	14·042
		November	2·757	3·345	—0·588		
		December	2·385	3·255	—0·870		
198	178		30·404	34·379	—3·975		

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 coldest months give a greater rainfall during the night than the day. This is somewhat reversed in the spring and summer months, June and August showing an exception.

1870.	Rainfall from 8 a.m. to 8 p.m.	Rainfall from 8 p.m. to 8 a.m.	Difference between Night and Day Fall.
January	1·476	1.810	+0·334
February	0·325	0·593	+0·268
March	1·354	1.114	—0·240
April	1·584	0·472	—1·112
May	0·797	0·086	—0·711
June	0·753	1·136	+0·383
July	0·380	0·353	—0·027
August	0·343	1·246	+0·903
September	1·456	0·889	—0·567
October	4·488	4·406	—0·082
November	1·416	1·289	—0·127
December	0·906	1·465	+0·559
Sums	15·278	14·859	—0·419

I have measured the rainfall for the day and night a little over three years, and therefore present below the average day and night fall for three years. This table shows that, without exception, the coldest months of the year have a greater rainfall during the night than the day, and the exceptions to the reverse of this during the warmer months are, as in last year, June and August. Of course a three years' average is scarcely sufficient to point to a rule. This must be left to be shown by the results of future observations.

AVERAGE OF THREE YEARS, FROM 1868 TO 1870.

	Rainfall from 8 a.m. to 8 p.m.	Rainfall from 8 p.m. to 8 a.m.	Difference between Night and Day Fall.
	Inches.	Inches.	Inches.
January	1·414	1·523	+0·109
February	0·864	1·481	+0·617
March	1·226	1·260	+0·034
April	1·075	0·829	−0·246
May	1·179	0·394	−0·785
June	0·530	0·714	+0·184
July	0·424	0·315	−0·109
August	0·983	1·594	+0·611
September	1·731	1·731	0·000
October	2·804	2·839	+0·035
November	1·453	1·772	+0·319
December	1·945	2·596	+0·651
Sums	15·628	17·048	+1·420

MICROSCOPICAL AND NATURAL HISTORY SECTION.

April 24th, 1871.

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

Mr. CHARLES BAILEY exhibited some seedling sycamores having abnormal cotyledons. He said it was by no means rare to meet with occasional sports in the form and number of the cotyledons of the Sycamore, but most of the seedlings which had come up this season in his garden presented some aberration from the normal type. The most frequent deviation was that in which the extremity of one of the cotyledons bifurcated; in a lesser number this division extended more than half way down the cotyledon; while in some of the specimens exhibited the division was complete, so that the seedling possessed three distinct cotyledons of equal dimensions. A less frequent, although not uncommon, form was one in which both cotyledons were bifid, the divisions occasionally extending more than half way down. Most of the seedlings seemed to be the production of one tree, and Mr. Bailey had not hitherto noticed any change in the plumule.

"On the Microscopical Examination of Dust blown into a Railway Carriage near Birmingham," by JOSEPH SIDEBOTHAM, F.R.A.S.

On the 24th May, 1870, while travelling by rail between Saltley and Camp Hill, I spread a paper on a seat of the carriage near the open window, and collected the dust that fell upon it. A rough examination of this with the twothirds power showed a large proportion of fragments of iron, and on applying a soft iron needle I found that many of them were highly magnetic. They were mostly long, thin, and straight, the largest being about $\frac{1}{150}$ of an inch, and, under the power used, had the appearance of a quantity of old nails. I then with a magnet separated the iron from the other particles.

The weight altogether of the dust collected was 5·7 grains, and the proportion of those particles composed wholly or in part of iron was 2·9 grains, or more than one half. The iron thus separated consisted chiefly of fused particles of dross or burned iron, like "clinkers," many were more or less spherical, like those brought to our notice by Mr. Dancer from the flue of a furnace, but none so smooth; they were all more or less covered with spikes and excrescences, some having long tails like the old "Prince Rupert's drops"; there were also many small angular particles like cast iron, having crystalline structure.

The other portion of the dust consisted largely of cinders, some very bright angular fragments of glass or quartz, a few bits of yellow metal, opaque white and spherical bodies, like those described by Mr. Dancer, grains of sand, a few bits of coal, &c.

After the examination of this dust, I could easily understand why it had produced such irritation; the number of angular, pointed, and spiked pieces of iron, and the scoriæ or clinkers, being quite sufficient to account for the unpleasant effect.

I think it probable that the magnetic strips of iron are laminæ from the rails and tires of the wheels, and the other iron particles portions of fused metal, either from the coal or from the furnace bars. The large proportion of iron found in the dust is probably owing to the metal being heavier than the ordinary dust, and accumulating in cuttings such as those between the two stations named.

If I had to travel much by railway through that district, I should like to wear magnetic railway spectacles, and a magnetic respirator in dry weather.

Mr. Charles Bailey, as bearing upon the subject introduced by Mr. Sidebotham, drew attention to some experiments which Mr. Charles Stodder, of Boston, U.S., had been making on the microscopic contents of the atmosphere of that city. Amongst other investigations he was led to examine a fine black dust from a beam in the polishing shop of the United States Armoury, at Springfield. He found it to contain a few vegetable fibres, some apparently organic fragments, and some broken crystals; but the great mass of it was made up of amorphous fragments of iron, of the 1-100 m.m. and upwards in size, as well as curved and irregular fibres and masses of iron with sharp jagged edges, from 5 to 15 m.m. in size; there were also some very minute perfect spheres, probably iron. In trying the effect of a magnet upon this dust, he found it removed it from a sheet of paper as completely as if it had been swept off with a brush, and he concluded that the non-metallic portions adhered to the iron particles by the thin layer of oil with which all the particles of dust were coated.

To prevent this dust passing into the atmosphere of cities, Mr. Stodder recommended a plan which had been put in practice many years ago in this country, but abandoned from the indifference of the workpeople, viz., the fixing of magnets in the immediate neighbourhood of grindstones and polishing wheels.

In the same report, Mr. Stodder alludes to the labours of two members of this Society—Dr. Angus Smith and Mr. J. B. Dancer—in examining the contents of the air, and points out an important matter considerably affecting the results of such investigations, viz., the method employed for filtering the air through water. The usual method has been to place a small quantity of pure water in a large bottle, and shake it in the air under investigations, repeating the operation with renewed volumes of air in the same water; but Mr. Stodder shows how impossible it is to intercept all the foreign particles in the atmosphere in this way, inasmuch as the smallest bubbles of air which pass through the water very much exceed in size the particles of matter which are sought for, and myriads must elude observation. A greater difficulty, however, is to obtain absolutely pure water for such experiments, and whether filtered or distilled water was used, a drop evaporated on a glass slide always left a deposit of scaly and granular particles. This result, as Mr. Stodder justly says, puts an end to this mode of investigation, and throws a cloud of suspicion on all reported researches in this line, when water was the medium used.

Mr. Bailey stated that the information communicated above had been extracted from an official document emanating from the "State Board of Health of Massachusetts," and he commended it to the Officers of Health and to the Corporations of Manchester and Salford, as an illustration of what is required in this neighbourhood. The document just issued gives a summary of the work done during the past year, and embraces reports upon Public Abattoirs; the Cattle Plague, and its effect on milk; an Outbreak of Typhoid Fever; the Overcrowding of Tenements and want of Clean Streets in Boston; Smallpox; Poisoning by Lead; Trichiniasis in Massachusetts; Health of the various Towns in the State; Homes for the People; Alcoholic Drinks; Mortality of the City of Boston; Ventilation of School-houses; Water Supply, and its Comparative Purity; Air, and some of its Impurities; Health of Children employed in the manufacture of Textile Fabrics; Effect of Sewing Machines on Health, &c.; and all this at a cost of under £600 the year.

Mr. Walter Morris read a paper "On the Adulteration of Food," principally with a view to its detection by the microscope. Adulteration was defined as being the fraudulent addition to any substance of another, for the sake of increased sale or profit. There are several modes of accomplishing this end; the first, and the most common, is by the addition of some article to increase the bulk or weight, as when starch is added to mustard, and cheaper flours to wheaten flour; the second by improving the appearance and apparent quality, so as to sell an inferior article at the price of a better, as in the case of the artificial colouring of pickles made of stale vegetables to resemble fresh. One of the commonest apologies for these practices is that the public prefer the adulterated article to the pure; that, for instance, pure mustard "will not sell." This allegation is, however, hardly a fair one, as the pure article is never offered; and, doubtless, if the pure article were used as freely as the ordinary mixture, it would be found unexpectedly pungent. But the fallacy of such apologies has been exposed by the example of pickles, which under this plea used to be invariably coloured with an artificial and frequently poisonous pigment. The public eye was thus educated to expect them of a bright green; yet, since some manufacturers have exposed the fraud and sent out pure pickles, the public have completely turned round, and avoid any which show an unnatural colour.

The adulteration of bread and flour with alum, to make them look whiter and of a superior quality, has to some extent diminished; but that substance is often replaced by the still worse sulphate of copper, or blue vitriol, which was recently detected in 16 out of 20 loaves tested. In this case the public has been led to suppose that the quality of bread is shown by its whiteness, whereas by taking out the bran a most valuable part of the grain, viz., its azotised or flesh-forming portion, is lost. Less dangerous admixtures are those of cheaper flours, such as barley, rice, and "cones" (the latter made from a species of wheat called revet), and even beans.

The adulteration of coffee with chicory, though so well understood, exists, especially in poorer neighbourhoods, to an extent hardly credible. Out of 47 samples, 18 were found pure, the lowest price of which was 1s. 4d. per lb.; of the the rest, most were half, and some were wholly, composed of chicory, which being worth about 6d. per lb., was thus sold at 1s. and 1s. 4d. The difference can be readily detected by the microscope, the cells of chicory being much larger, and the cell walls much thinner, than those of coffee.

Even chicory itself is much adulterated; out of 57 samples only about one-half were pure, the adulterants being roasted wheat, acorns, beans, carrots, and sawdust.

Tea is less subject to adulteration than many articles of food; such abominations as the celebrated Maloo mixture, consisting of old used leaves redried, willow leaves and twigs, and even iron filings, have been quickly detected and refused by the trade. The "facing," however, of green tea, with poisonous coloring matter, is both absurd and harmful: and it will probably be continued so long as the public are content to accept such a palpable imposture as "genuine green."

It is a matter of opinion whether cocoa as ordinarily sold is to be considered an adulterated, or a manufactured, article. It is seldom sold pure and alone; being usually mixed with starch and sugar — the term "pure cocoa" is therefore in most cases intended to mislead. Some kinds have lard or suet admixed, and to others red ochre is added to bring up the colour, rendered pale by an excessive quantity of starch. The relative quantities of these component parts in any sample of cocoa may be readily ascertained by the microscope; that of starch may be roughly seen by shaking up some of the cocoa with water in a test tube or tall bottle, breaking up the lumps and then allowing all to settle; when the starch will sink to the bottom and form a white layer beneath the cocoa. On warming the water, the fat will of course float on the top, and the sugar will be dissolved. The sugar crystals and fat are also shown by redrying the solution on a glass slide.

Sugar is mixed with inferior kinds of the same article, but not (as popularly believed) with sand; the chief impurities in raw sugar are cane fibre, accidental dirt, and the sugar mite or acarus. The latter exists in most raw sugars (out of 72 samples, 69 contained mites); but more abundantly in the moderately brown kinds than in the darker. The insect is barely visible to the naked eye. To obtain specimens, the sample should be dissolved in tepid water and well stirred, then allowed to stand a few minutes, and the acari will be found as minute particles floating on the top. The process of refining entirely removes these and the other impurities named.

Mustard is invariably adulterated with flour, which forms one half or three fourths of the article as usually sold. It may be readily detected by the microscope, mustard itself containing no starch whatever. Turmeric is often added to bring up the colour, after this wholesale admixture, and cayenne to give it strength.

Pepper may now be obtained pure of respectable dealers; but as regards the cheaper kinds, and in poor neighbourhoods, it is largely adulterated with meal or starch, gypsum, and dirt of any kind, to give bulk and weight. The starchy substances may be detected by the microscope, the earthy ones will be left as ash after burning, and their character may be ascertained by the polariscope. The particles of pepper itself are easily recognized by the characteristic stellate cells in the outer skin, and the hard angular ones of the inner part of the seed.

Many examples of the above and other kinds of adulteration, mounted for the microscope, were exhibited at the same time, for comparison with pure specimens.

Attention was drawn to the loss science has sustained by the death of the late Mr. WILLIAM WILSON, the eminent muscologist, and it was unanimously resolved that a letter of condolence be forwarded to Mrs. Wilson and her family.

Annual Meeting, May 8th, 1871.

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

"Observations on the Bilharzia hæmatobia (Cobbold), Distomum hæmatobium (Bilharz)," by HENRY SIMPSON, M.D.

The Bilharzia hæmatobia is an entozoon infesting the human body, and very prevalent in some hot climates. It occurs abundantly in Egypt and at the Cape of Good Hope, where it is the cause of the hæmaturia endemic in those countries.

It was discovered in 1851, by Dr. Bilharz, of Cairo, and found to inhabit the small blood vessels of the bladder, kidneys, &c., producing various abnormal changes in the mucous membranes. It slowly undermines the health, and leads eventually, in many cases, to a fatal result. It is very common in Egypt, for Griesinger and Bilharz found the worm in 117 out of 363 post mortem examinations.

The first cases met with in this country were related by Dr. John Harley, in 1864. These were from the Cape. In 1869 and 1870 he published papers supplementary to his first one.

As the opportunities for observations on this parasite are not abundant in this country, and as it is interesting both from a scientific as well as from a medical stand-point, I will briefly remark on some points suggested by a case recently under my care in the Manchester Infirmary, avoiding medical details as far as possible.

Through inflammatory changes leading to loss of substance, the ova become free in the bladder, and are washed away in the urinary secretion in immense numbers, along with blood discs and pus corpuscles. They are generally ovate in form, but vary somewhat in outline. At one end the shell is produced into a short spike, something like that on Von Moltke's helmet. Occasionally this is placed laterally. They vary in length from $\frac{1}{170}$th to $\frac{1}{200}$th of an inch, and in breadth from $\frac{1}{400}$th to $\frac{1}{550}$th of an inch. The shell is without any distinct operculum. The contents of the egg are seen in all stages of development, from scarcely distinguishable granules, enclosed in a vitilline membrane, to the perfectly formed ciliated embryo, exhibiting active movements of the body and rapid play of the cilia while still enclosed in the shell. Sometimes the head of the embryo lies towards the spiked end, sometimes to the plain end of the shell. Free, living embryos are often met with in the urine, and it is curious to watch the mode in which they escape from the shell.

The general shape of the embryo is elliptical; they are abundantly supplied with cilia, especially at the anterior extremity, and show distinct traces of a water vascular system. The development of this entozoon in all probability follows the same general plan as that of the other Trematode worms, or Flukes, which pass through several phases or alternations of generation; one or two intermediate hosts, generally mollusca or aquatic larvæ, being necessary before the adult fluke becomes parasitic in the body of the vertebrate animal destined to be its host.

The liver-fluke of the sheep, which is occasionally found in man, is the best known of these parasites. The adult worm we are now considering was called by Bilharz, Distomum hæmatobium, but more recent writers have placed it in a distinct genus, because each individual is male or female, and not hermaphrodite, as in the Distomata. Diesing called it Gynæcophorus hæmatobius, and Cobbold, Bilharzia hæmatobia, in honour of its discoverer. It is described by Küchenmeister, Leuckart, Cobbold, and others, in most cases from materials derived from Griesinger; so that I will only say that the male is about half an inch in length, having a short body and long tail. The female is rather longer, but much more slender, the anterior end being less than $\frac{1}{300}$th of an inch in thickness, and lower part about $\frac{1}{90}$th.

It has been supposed to be taken into the body in a larval condition, either with the water, or along with uncooked vegetables, as water-cress, on which small molluscs containing cercariæ may have been lodged. Another supposition is that while bathing the larvæ penetrate the skin. Opposed to this is the statement that, at the Cape, while the Colonists and the Coolies, who are not remarkable for a love of bathing, are subject to the disease produced by the Bilharzia, the Kaffirs, who bathe frequently—sometimes three or four times a day, are free from it.

The history of the case under my care gave no clue as to the mode in which the parasite obtained access to the body, further than this, that it was quite possible that it might have been taken either with the water or food, the former being occasionally drunk unfiltered.

The question of the introduction of the disease among us is a matter of much interest and importance. In these days of travel, cases will be imported more frequently than formerly, and the eggs of the parasite distributed in immense numbers. Dr. Cobbold mentions the case of a little girl from the Cape who has been recently under his care, and he estimates that 10,000 eggs must have escaped daily for many months. He concludes, from experiments as to the effects of reagents on the living embryos, that there is not likely to be any risk of its spread by means of sewage distribution, as they were killed by water in which decomposing matter of any kind had been introduced, and indeed required water almost absolutely pure for their development. The addition of a little salt to the water seemed, however, to act favourably. The conditions apparently required in these experiments are very unlikely to be met with in nature, and if they were necessary, the worm should, I think, have been extinct long ago in its native home.

With the exception of temperature, the other conditions for their development are probably present with us, and we do not as yet know that the former is essential. The truth seems to be that the circumstances necessary for their development are still unknown, and that it is premature to assume that sewage distribution will not increase the risk of its becoming acclimatized among us.

Specimens of nearly all the descriptions of Caoutchouc known to Commerce were exhibited by Mr. SPENCER H. BICKHAM, and a paper was read illustrative of the probable sources of supply, and the chief characteristics of each class.

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

Your Council have to report that during the past Session papers on the following subjects have been read at the meetings:—

1870.

Oct. 10.—"On Abraxas grossulariata," by JOSEPH SIDEBOTHAM, F.R.A.S.

Nov. 7.—"The Hawthorns of the Manchester Flora," by CHARLES BAILEY.

Nov. 7.—"On Limobius dissimilis," by JOSEPH SIDEBOTHAM, F.R.A.S.

" "Notes on the Botany of Mere," by G. E. HUNT.

" "On the occurrence of Myosurus minimus, near Northwich," by S. H. BICKHAM, Junr.

Dec. 5.—"Contributions towards a knowledge of Anthophila (Hymenoptera Aculeata) in the Mersey Province," by F. O. RUSPINI.

1871.

Jan. 9.—"On Carax flava L. and its Allies," by CHARLES BAILEY.

30.—"On Different Modes of Fossilization," by W. BOYD DAWKINS, F.R.S.

" "On Stigmaria," by Prof. W. C. WILLIAMSON, F.R.S.

" "Notes on the Cultivation of Madder in Derbyshire," by JOSEPH SIDEBOTHAM, F.R.A.S.

Feb. 27.—"Further Notes on the Polygonum from Mere, Cheshire," by G. E. HUNT.

Mar. 27.—"On some Logs of Oak found in the Irwell Valley Gravels," by JOHN PLANT, F.G.S.

April 24.—"On Abnormal Forms of Cotyledons of the Sycamore," by CHARLES BAILEY.

" "The Microscopic examination of Dust blown into a Railway Carriage near Birmingham," by JOSEPH SIDEBOTHAM, F.R.A.S.

" "On the Adulteration of Food," by WALTER MORRIS.

May 8.—"Observations on the Bilharzia hæmatobia," by HENRY SIMPSON, M.D.

" "On the Various Descriptions of Caoutchouc known to Commerce," by S. H. BICKHAM, Junr.

At the close of last Session, your Council were requested to endeavour to make arrangements with the Parent Society by which Associates could be admitted on more favourable terms. They have to report that the liberality of the Parent Society has enabled them materially to modify the regulations affecting the admission of this class of Members, and trust that, owing to the alteration, a great accession of strength will be gained.

Already seven new Associates have been elected, and the Section now consists of 37 Ordinary Members, one Corresponding Member, and 13 Associates.

The Treasurer's report is annexed, from which it will be seen that the finances of the Section are in a very satisfactory condition, there being a balance in hand of £34. 3s.

The election of Officers for the Session 1871-2 then took place, and the following gentlemen were elected:—

President.

JOSEPH BAXENDELL, F.R.A.S.

Vice-Presidents.

JOSEPH SIDEBOTHAM, F.R.A.S.
R. D. DARBISHIRE, B.A., F.G.S.
CHARLES BAILEY.

Treasurer.

HENRY ALEXANDER HURST.

Secretary.

SPENCER H. BICKHAM, JUN.

Of the Council.

JOHN B. DANCER, F.R.A.S.
W. C. WILLIAMSON, F.R.S.
A. G. LATHAM.
HENRY SIMPSON, M.D.
JOHN BARROW.
W. BOYD DAWKINS, F.R.S., F.G.S.
WALTER MORRIS.

List of Members.

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

List of Associates.

Bradbury, C. J.
Callender, A. W.
Hardy, John.
Hunt, G. E.
Hunt, John.
Labrey, B. B.
Linton, James.
Meyer, Adolph.
Peace, Thos. S.
Plant, John, F.G.S.
Ruspini, F. O.
Stirrup, Mark.
Waterhouse, J. Crewdson.

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

	£	s.	d.		£	s.	d.
1870.	£	s.	d.	1870.	£	s.	d.
To T. A. Pritchard, Diatoms	3	2	0	By Balance	20	4	9
,, Parent Society, for use of Rooms	2	2	0	,, Subscriptions	26	0	0
,, W. Roscoe for Teas	4	7	6	,, Interest allowed by Bank	0	10	3
,, J. E. Cornish, Microscopical Journal	0	16	0				
,, Chas. Simms and Co., Printing Circulars	2	4	6				
,, Balance carried down	34	3	0				
	£46	15	0		£46	15	0
				1871.			
				May 8. By Balance	£34	3	0

Examined and found correct,

(Signed) W. Boyd Dawkins,
Spencer H. Bickham, Jun.