PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

VOL. IV.

SESSION 1864–65.

MANCHESTER:
PRINTED BY THOS. SOWLER AND SONS, ST. ANN'S SQUARE.
LONDON: H. BAILLIERE, 219, REGENT STREET.
1865.

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, 1864.

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

Amongst the donations announced was a bust of Dr. Dalton, presented by Mrs. Samuel Fletcher.

On the motion of Dr. Roscoe, seconded by Dr. Joule, the best thanks of the Society were unanimously voted to Mrs. Fletcher, for her very valuable present.

The Chairman, in announcing the severe loss which the Society had sustained by the death of their late lamented Honorary Librarian, adverted to the very important services he had rendered. By Mr. Ekman's able management and indefatigable industry the library had become one of the most valuable in the country.

Thomas Windsor, Esq., M.R.C.S., was unanimously elected Honorary Librarian of the Society.

Several members stated their experience of the earthquake which occurred on the 26th ult. Mr. Baxendell described a remarkable derangement of the sidereal clock belonging to the corporation, which he had mounted at his house, which has suggested a new method of registering the occurrence of earthquake shocks.

A Paper was read entitled "Remarks on Mr. Dyer's Paper entitled 'Notes on Spinning Machines,' by Mr. Henry Brierly"; communicated by E. W. Binney, F.R.S.

Mr. Dyer, in the abstract of his paper (*Proceedings*, vol. iii. p. 265), commences by saying that "two distinct principles were embraced in the inventions of James Hargreaves and Richard Arkwright, which were afterwards combined by Samuel Crompton, to form the beautiful power-driven machine called the mule. Arkwright employed the throstle, or throated spindle, with arms or 'flyers' to conduct the threads on bobbins arranged in stationary frames; Hargreaves employed naked spindles arranged on a traversing frame or carriage, by which the threads were drawn out (about five feet) in horizontal lines, whilst being twisted, and were then taken up, or wound, on the spindles, to form 'cops,' whilst the carriage returned to the roller beam for another 'stretch.'"

As I understand Mr. Dyer, he attributes to Arkwright the invention of the *spindle and flyer*, and seems to consider that as the leading "*principle*" of Arkwright's invention, as distinguished from Hargreaves's invention, the main "*principle*" of which he mentions as consisting in the employment of *naked spindles* mounted on a *traversing frame* or *carriage*. Mr. Dyer is in error on both these points; and it is only fair to a very ingenious inventor who preceded both Hargreaves and Arkwright that these matters should be properly understood.

As to Hargreaves's "Jenny," there is no doubt of the originality of the invention, and that Hargreaves is entitled to the sole merit of whatever was valuable in that invention; but I shall show that as regards the inventions commonly attributed to Arkwright the case is far different. Before doing so, however, I must point out the mistake committed by Mr. Dyer as regards the nature of Hargreaves's inven-

tion. Hargreaves did not employ spindles mounted upon a *moving carriage,* nor was there any "*roller beam*" in his machine at all. Mr. Dyer seems to be here confounding the invention of Hargreaves with the subsequent invention of Crompton. An inspection of the specification of the patent taken out by Hargreaves in 1770 (No. 962) will show that in his "Jenny" the *spindles* revolve in *stationary bearings,* while instead of *rollers* for drawing out the material to the requisite fineness a "clasp" was used, which was arranged to move backwards and forwards in suitable framing. Thus, before beginning to spin a "stretch," the spinner had the "clasp" near to the spindles, with a certain length of roving between the clasp and each spindle. He then with one hand drew the clasp gently away from the spindles, thus elongating the portions of roving between them, while with the other hand he turned round certain apparatus by means of which the spindles were caused to revolve and twist the threads, and having thus drawn away the clasp for about five feet from the spindles he then returned it back towards them, at the same time guiding the threads by means of a "presser," or faller, and turning the spindles round so as to wind the spun threads upon *bobbins placed upon them,* and not upon the *spindles themselves.* This is a very different operation from that of the mule, in which *rollers* moving at different velocities draw the material to the required fineness, while the spindles *recede* from them in a *moving carriage,* at the same time revolving so as to twist the threads, which are afterwards wound upon the *spindles themselves* so as to form "cops." It is necessary to bear these distinctions in mind in following the progress of development which resulted in the production of the highly effective spinning machinery in use at the present day.

In coming to the consideration of the inventions usually attributed to Arkwright, it is only proper that due notice

should be bestowed upon those of a very ingenious man who preceded him, viz., Lewis Paul. The invention of "spinning by rollers," as it is familiarly termed, and for which Arkwright usually obtains the credit, most undoubtedly originated with Paul. It is only necessary to read the specification of the patent granted to Paul on the 24th June, 1738, to be satisfied on this point. It is true that there has been some dispute as to whether Paul himself really originated the invention described in that specification, a person of the name of Wyatt being mentioned by some as having communicated the invention to Paul, and another person of the name of Highs having laid some sort of claim to the invention. The claims of these parties, however, only rest upon some very vague assertions made by themselves or persons connected with them, such, for instance, as that contained in a letter written by Wyatt to Sir Leicester Holt, and quoted at page 124 of Baines's *History of the Cotton Manufacture*, in which Wyatt says, referring to the machine known as Paul's—"I am the person that was the principal agent in compileing the spinning engine," &c. Such assertions as these, however, cannot be taken as satisfactory proofs of the claims of these parties, and when in opposition to them we have the specification of a patent granted to Paul for this application, *in which specification the name of Wyatt actually appears* as that of a witness to the signature, &c., of the document by Paul, it is difficult for an unprejudiced person to come to any other conclusion than that Paul was really the inventor of the contrivance for which the patent was granted. However that may be, it is quite clear that this invention does not belong to Arkwright, whose first patent relating to spinning machinery was taken out in 1769, or above thirty years after the patent granted to Paul. And that Arkwright was aware of the existence of Paul's patent is beyond all doubt, and is proved by documents which are quoted in the work to which I have

already referred, viz., Baines's *History of the Cotton Manufacture,* as well as by other documents to which I need not now particularly allude.

In speaking of the "peculiar soft property" possessed by the yarn spun upon the mule, as distinguishing it from the yarn spun by the machine called the "throstle," Mr. Dyer is quite correct. I believe it is not so generally known as it should be that the "throstle," as the term is now generally understood, consists mainly of a combination of the leading features of Paul's invention of 1738, which I have already mentioned, with those of a later invention, for which he obtained a patent in 1758. Mr. Dyer mentions the *spindle and flyer* as *Arkwright's invention,* but they had been in use upwards of *one hundred and sixty years* before the date of Arkwright's inventions. This is proved by several of the works now in the library of the Patent Office, especially an Italian work, entitled "Novo Teatro di Machine et Edificii," &c., and a pamphlet published in 1681, entitled "Some Proposals for the Employment of the Poor." In Paul's specification of 1738 we have the application of rollers moving at different velocities for drawing the material, and in that of 1758 the *combination* of *rollers* (of which only one pair are shown in the specification of the latter patent) with the *spindle* and *flyer,* by means of which the thread was twisted and wound upon a bobbin. So far as I have been able to ascertain, this is the first instance in which we have the material passing through *rollers* in its way to a *spindle and flyer* by which it is twisted and wound upon a bobbin, and we have here, clearly, the origin of the "throstle." Paul's specification of 1758 describes these operations very minutely, but spinners acquainted with this class of machinery will at once perceive that the machine described in Paul's specification, though displaying great ingenuity, was defective in one important particular, which

was, that there was no arrangement by which the "traverse" of the bobbin, as it is now termed, could be performed. The manner in which the yarn was distributed over the bobbin was peculiar. The flyer was *tubular*, and only reached downwards to just within the head of the bobbin, and the latter was of *conical* form, the lower end being the smallest. The yarn was thus wound constantly at the *top* of the bobbin by the flyer, but as it accumulated there, the *conical form* of the bobbin caused the coils of yarn to *slip downwards*, and thus a kind of distribution of yarn over the bobbin was effected. This part of the arrangement was evidently very defective, and it is highly probable that the want of some more efficient method of distributing the yarn over the bobbin was one if not the sole cause of the want of success which undoubtedly attended the efforts of Paul to bring his machine into general use. The addition to the machine of the "traverse motion," as it is called, or the arrangement by which the bobbin is made to ascend and descend on the spindle, was unquestionably a vast improvement, and of this Arkwright may fairly have the credit. But even here Arkwright had been partly anticipated,* for in the specification of a patent granted to Comah Wood, in 1772, No. 1018, there is the description of a mode of causing the bobbin to be moved on the spindle for the purpose of winding the thread upon all parts of it in succession, this being done by a movable rail, capable of being shifted from time to time by hand. This, however, was only a slight approximation to what was required, and Arkwright, in applying the "heart motion,"† as it is now termed, may be said to have brought the throstle well nigh to the condition in which it is used at the present day.

* Arkwright's specification of 1769 does not describe any arrangement for this purpose.

† The date of this application appears to be uncertain.

Crompton's invention, in which he combined the drawing rollers of Paul with the use of spindles mounted in a movable carriage, and which was thence called the "mule," as partaking to some extent of the principles of both the jenny and the throstle, formed an immense improvement on all previous inventions. Of course the mule has been greatly improved since Crompton's time; but in speaking of Mr. Kennedy and Mr. Peter Ewart as being among the *most eminent* of those who have improved that machine, Mr. Dyer gives no reason for his opinion. I pass from this, however, to Mr. Dyer's observations on the self-acting mule.

Mr. Dyer says the "real difficulty" connected with the construction of a self-acting mule was chiefly confined to the winding of the threads on the cops; but, as I can testify from some twenty years' actual experience, this is by no means the greatest difficulty, especially as regards the construction of *large mules*. The backing off, or unwinding of the loose coils of yarn from the spindles, which are above the cops, so as to allow the depression of the "faller," or guiding wire, is connected with mechanical difficulties of a most formidable nature, and the reaction produced by the sudden stoppage and reversal of the motion of the spindles of a large mule, with their attendant pulleys and geering, &c., has been a source of trouble and annoyance which none but those who have experienced it can understand. Mr. Dyer mentions a Mr. Snodgrass as having made the first "*notable*" attempt to construct a self-acting mule; but beyond the attempt being a notable *failure*, I know of nothing to entitle it to that distinction. Mr. Eaton's invention of 1819, which Mr. Dyer mentions as next in succession to that of Mr. Snodgrass, was undoubtedly a very ingenious invention, and was used *by himself* for some little time. Mr. Eaton's specification shows that his knowledge of the subject was very accurate, but the *details* of the invention, *practically considered*, were in many

respects very defective. I will not here go into all these details, but will point out two defects, either of which were sufficient to prevent the successful application of the invention. One of these was the arrangement of the apparatus for effecting the "backing-off," (the difficulties attending which movement I have already noticed,) and the manner in which the "sector used by Mr. Eaton for this purpose was made to pass into geer with the wheel upon the" run "shaft," or shaft which turned the spindles, was most objectionable, as creating a constant tendency to derangement and breakage of both the sector and wheel; and that this *was* the result of the arrangement I have been informed by a most respectable engineer now living in Manchester, and who saw Mr. Eaton's machines during the short time they were in existence.

Another defect lay in the arrangement for effecting the winding of the yarn upon the spindles. For this purpose Mr. Eaton employed two conical pulleys, one of which gave motion to the other by means of a strap, the latter being traversed along the pulleys at intervals so as to vary the rapidity of motion of the spindles according to the varying size of the cops. It is obvious that in this case the liability of the strap to slip upon the pulleys must have led to great irregularity in the working of the machine, and in fact I do not believe that a *large machine* could be worked at all on such a system. These defects, along with others which might be mentioned, led to such trouble and annoyance in connection with the attempt to work these machines, that they were abandoned after a trial extending over a few months. In fact the attempts of Mr. Eaton to construct a self-acting mule, as well as those of Mr. De Jongh and of Mr. Ewart, to which Mr. Dyer also alludes, can only be looked upon as so many *experiments*, none of which were attended with any reasonable amount of practical success; all being abandoned after a fruitless struggle, exhausting the

patience both of the inventors and of those connected with them.

In speaking of Roberts's invention of 1825, Mr. Dyer seems either not to understand of what that invention consisted, or to greatly undervalue it. Although it is quite true that this invention has been greatly improved upon since the date of the patent granted for it, those who are practically conversant with these matters know well that it contained the germs of what was afterwards developed into one of the most successful and extensively used machines ever produced. In fact it may truly be said that although great improvements in the construction of the self-acting mule have been made since the date of Roberts's invention, and especially during the last few years, still, in all or nearly all the self-acting mules now used there are embodied some of those excellent mechanical arrangements of which he was undoubtedly the originator, and which must always be looked upon as imperishable monuments of that splendid mechanical genius with which Roberts was beyond all question endowed. For example, the arrangement of the "cam-shaft" for producing the changes of motion of the machine; the arrangement of the double fallers; the method of causing the faller to be depressed by the reverse movement of the apparatus which turns the spindles in backing-off; and the system of regulating the "winding-on" by the gradual approximation or otherwise of the faller and counter faller, are all contrivances displaying the highest mechanical skill and ingenuity, and have all been most extensively brought into use, and so continue to the present day, while on turning to the patent obtained by Roberts in 1830, for the employment of the well known radial arm and drum as a means of effecting the "winding-on," we find that scarcely a self-acting mule is now constructed which does not contain in some form or other a modification of this famous invention.

In speaking thus of Roberts's inventions, I consider that I am only doing justice to a highly meritorious engineer, whose ingenious contrivances, although undoubtedly attended with some defects, which have led to certain of his arrangements being superseded by others of a more perfect character, must yet be admitted as having furnished some of the brightest examples of the mechanical genius of the present age. Mr. Roberts was the inventor of the first practical self-acting mule.

Mr. Dyer is correct in attributing to the inventions of Mr. Smith, of Deanstone, "much novelty and some good properties," and in stating that Mr. Smith's and Mr. Roberts's mules were the chief competing mules for many years. I really, however, cannot understand Mr. Dyer when he comes to speak of *Mr. Potter's* inventions. The patent of 1836, to which Mr. Dyer evidently refers, was not taken out by Messrs. John and James Potter, but by *Mr. James Potter only;* that gentleman being, as I know personally, an extremely ingenious mechanic. Mr. Potter's mules have had no inconsiderable amount of success. Mules to the amount of many thousands of spindles have been made and sold under Mr. Potter's patents, as I know personally. A large firm of cotton spinners, with whom I was formerly connected, purchased and used a number of them, and I have no hesitation in saying that they possessed qualities which rendered them for *some purposes* preferable, *on the whole*, to either Roberts's or Smith's mules. Such, indeed, was the admitted merit of Mr. Potter's invention, that on the approach of the termination of the fourteen years for which his patent was originally granted, the Lords of the Privy Council granted a prolongation of the patent for five years.

I am much surprised at what Mr. Dyer says in the latter part of his paper, in which he states that it was not until after the expiration of the second patent of Mr. Roberts and that of Mr. Smith that a *really good working*

mule was constructed, "and which," says he, "appears to have been realised in the patent obtained, in 1847, by Mr. Matthew Curtis and Mr. Robert Lakin. Thus thirty years had elapsed from the time when Mr. Eaton gave the true principles for constructing a self-acting mule to that of their being carried into practical effect as above stated." Now, I am acquainted with the specification of the patent taken out by Mr. Curtis and Mr. Lakin, in 1847. The invention described therein consists almost wholly, so far as it relates to the mule, of improvements, or *alleged* improvements, in some of the details of Smith's and Roberts's machines, and there is no reason to give it the character of the first really good working mule ever constructed. I have the pleasure of knowing Mr. Curtis and others connected with his firm, and I should think if they were inclined to claim such a character for any of their inventions, it would certainly not be for the one Mr. Dyer mentions.

Nor is there any better foundation for the assertion that Mr. Eaton "gave the true principles for constructing a self-acting mule," &c. As I have already mentioned, Mr. Eaton's invention only survived a few months, and there is no single particular of Mr. Eaton's invention to be found in the self-acting mules now used, while, on the other hand, Roberts's inventions form the basis of the great bulk of the self-acting mules now made, *not excepting even those made by Messrs. Curtis and Co. themselves.* It was Mr. Roberts, and not Mr. Eaton, who "gave the true principles for constructing a self-acting mule."

The highest test of the efficiency of a self-acting mule is its capability of spinning the finer numbers of yarn, as, from the delicacy and accuracy of manipulation required in producing the very fine threads, it has been found very difficult to adapt the self-acting mule to that class of spinning. Mr. Roberts himself failed in this, as I have heard him admit in the wit-

ness box of a court of justice; but if this test were applied to the invention of Messrs. Curtis and Lakin, of 1847, it would be found *lower down in the scale than even Roberts's.* Many persons have, indeed, until a comparatively recent period, entertained the opinion that the spinning of fine yarns would never be successfully accomplished by the self-acting mule; and had I not already extended these observations to a much greater length than I at first intended, I could mention some interesting circumstances connected with the progress of this branch of invention. That would carry me, however, into a discussion with reference to inventors and inventions not mentioned in Mr. Dyer's paper, and I will therefore reserve what I have to say upon that part of the subject for some future occasion.

Mr. Dyer, in reply, said that he had supported his statements about the patented inventions noticed in his paper (the abstract of which only was commented upon by Mr. Brierly), by literal citations from the printed specifications. His Notes were entitled "Part the First, on the Mule Jenny," and that therefore he had only to make the distinction of this class of spinning machines from those of the throstle class, and his description of the latter went no further than to show how much of it Crompton took to make up his composite mules.

Ordinary Meeting, October 18th, 1864.

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

Mr. Brothers, F.R.A.S., exhibited a photograph of the moon, which he had made from a negative by Warren De la Rue, F.R.S. The original negative is one inch in diameter, and from this a positive two inches in diameter was first made. This was placed within the rays of a nine-inch condenser of the solar camera, and an enlarged negative on a plate 36in. by 24in. was produced. The print exhibited was on a single sheet of paper, and thus the disadvantage of joining several sheets together as in other large prints of the moon was avoided. Various effects from the same negative could be produced by providing either for the finer details of the strongly illuminated side of the moon, or for the more rugged parts at the side near and at the termination of parts in shade. During the conversation which followed, Mr. Brothers stated that the negatives were the property of Messrs. Smith and Beck. It was suggested by Mr. Baxendell that more accurate micrometrical measurements could be made from such a photograph than direct from the moon's surface in the telescope—in the one case the object being still, and in the other in constant motion; and Dr. Roscoe remarked that if a series of the several phazes of the moon on the same scale as the one shown were published, he

believed they would be of great value for educational and scientific purposes.

A Paper was read, entitled "On the Relation of Force to Matter and Mind." Part I. By the Rev. THOMAS P. KIRKMAN, M.A., F.R.S., Hon. Member.

There are at least three schools of thinkers among us, about the relation of force to matter. The first and most popular one conceives of matter as really existing and occupying space, whether it be or be not the seat or subject of force, so that if all force were withdrawn, matter would remain, quiescent and unexciting. The second school, not, it is to be hoped, a numerous one, affirms that matter exists of necessity, and is of necessity endued with force; that no power can divorce matter and force; that every form of chaos or order is but one of the combinations that eternal matter must assume by the eternal play and collision of eternal force; and that the notion of an Author and Preserver of the Universe is but the dream of ignorance. This is the school of the Materialists. A third and I think a growing school denies the existence of matter as distinct from force; this may be called the school of the Immaterialists. The Immaterialists are not the idealists of Berkeley's type, for the former affirm space, and an external world of force having a real existence in space, which the latter are far from affirming.

If any leading philosopher will boldly preach this Immaterialist gospel, and proclaim a crusade against matter, I am prepared to promise him one humble follower.

Of *force* no man is required to give either a demonstration or a definition. We cannot converse without agreeing in affirming ourselves in space, which is simply the affirmation

of existence or force in this external world, which is not ourselves. But I have a right to demand from a disputant both a definition of *matter* and a proof of its existence. It appears to me that this existence is either an unproved inference from the experience of resistance, or is a delusion attached to a word of our infancy. Why cannot I close my hand upon a brick? The answer of a child is, because the brick is there, inactive and inert there, a piece of stuff which is doing nothing but just lying there: this is the notion of sluggish quiescent matter. The thinker perceives that he is prevented from closing his hand by force perpetually in action there, and that the brick is what it is by virtue of intense cohesion, and other resistances that defeat his pressure and the force of his will. Thus he gets the compound notion of matter having its seat in space, and of force having its seat in the matter.

But what need is there of the matter? Why may not the forces have their seats in space? How can you construct a demonstration of the presence of this matter?

You appeal first to the evidence of our senses. But we are all agreed that our sight and hearing can teach us nothing about forces, resistances, and motions, except through the interpreter touch; that the telegrams which reach the mind by sight and hearing are rapidly translated into nothing but the memory of touch. Now touch teaches us absolutely nothing but *resistances*, except lessons of pleasure and pain that may be here left out of consideration. Thus the evidence of our senses comes to nothing more than the consciousness and the memory of *forces* which resist our own force of volition.

You say that this table is a geometrical locus of material

points in space, and at the same time a locus of forces having their seats in those material points. Let us suppose that the Author of the Universe, or some lower power competent to do it, were to remove all the matter from this locus, and leave a system of force exactly filling the locus of the removed matter, each force having its seat not in a point of matter but in a point of space: suppose that these pure force-points in pure space should present the same resistance to my hand or my tools, should receive the same vibrations from, and communicate the same vibrations to, all that surrounds the locus, as the force did before the matter was removed. How could our senses detect the absence of the vanished matter? The table would feel and look the same, sound, smell, cut, burn the same, as before. Then it appears that our senses help us to no evidence either of the presence or the absence of this mysterious matter.

Do you appeal next to the mathematician? You refer to his moments, moving forces, living forces, &c., in all which the *mass* is before us in his formulæ. It is true that the letter *m* is there; but to the mathematician it is in all cases simply a number, a constant obtained by experiment only, experiment which is independent of all hypothesis about the existence or non-existence of matter. Newton, in the opening of his *Principia*, says, *Virium causas vel sedes non expendo.* He neither troubled himself about the cause, nor about the seat of force. It was sufficient for him to have the exact positions in space of the centre and points of departure of his forces with their directions and the numerical constant, given by experiment, which determine their intensities. No mathematician cares anything about material seats for his forces: he throws the *matter* invariably away, he contents

himself with forces given in position in space, with the requisite numerical multiples, and he investigates simply the relations and mutual actions of pure *force-points*, such as centres of gravity and the like: then he contracts and predicts the facts of the universe.

The Materialist gains nothing by the evidence of the mathematician. But he returns to the charge, thus you have been talking nonsense about that table, in pretending that the matter can be removed while all the forces remain; for matter exists in ultimate atoms, each constituted by an indefinite force of cohesion of its parts. Wherefore, if the atoms are supposed to be removed, their internal forces are removed from the locus along with them.

So here are the dear little atoms—their fairy troops are brought into the field. There is some poetry in the superstitions of the credulous Materialist; but poetry is not quite science. I deny the existence of atoms, and demand proof of it. Experimental proof will be difficult in the case of such small commodities; can we expect it from analysis? Will there ever be a proof of an indivisible absolute minimum of matter? Do you expect a set of formulæ to force up, demonstrating the impossibility of the existence in the universe of a bit of matter of less than a certain definite weight and diameter? That may do for the Materialists, but not for the mathematicians.

If I must believe in matter, I must also believe, not that it is infinitely *divisible*—for that is absurd; but that it is, like the space which it occupies at this moment infinitely *divided;* so that every mathematical point, or mere volume of space, is occupied in a material locus by a mathematical point, or zero mass, of matter. The difficulty of affirming an

actual infinite division is no greater in the case of matter than in the case of space; and we are compelled to affirm this in space. Wherefore I did not talk nonsense about the table. If there is matter, its ultimate atoms can be nothing but zero masses, each occupying a zero vacuum of space, which masses, having no parts, can be constituted by no internal forces of cohesion.

In conclusion, I venture to defy the Materialist to deduce any experimental or logical demonstration of the existence of his matter the seat of force, or bring proof of it beyond his own unsupported assertion. In these notions there is nothing new; nor am I aware that their adoption can make any change in the habits or symbols of our scientific thought, except only with reference to the greatest topic in philosophy, the cause of the Universe. I think that I have cleared abundant room for this observation: that, while it is confessedly difficult for the Theist to prove, by a merely intellectual process, the existence of his God,—it is *not* a whit less difficult for the Materialist to prove, by any process whatever, the existence of his matter.

PHYSICAL AND MATHEMATICAL SECTION.

October 13th, 1864.

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

Mr. VERNON, F.R.A.S., read a letter from Mr. James Taylor, Secretary of the Oldham Corporation Gas and Water Works, enclosing the following returns of Rainfall at Oldham, Strines Dale, and Brushes Clough. Mr. Taylor states that "the gauge at Oldham is fixed at the Gas Works, about four hundred yards from the centre of the town. Strines Dale is one of our reservoirs about two miles north-east of Oldham. Brushes Clough is the site of another of our reservoirs about three miles north of Oldham."

RAINFALL AT OLDHAM.

Gauge—6 feet above ground, and 600 feet above the sea.

	1860.			1861.		
	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.
	Inches.		Inches.	Inches.		Inches.
January	2·55	25	21st—0·38	0·27	8	24th—0·06
February......	0·89	16	8th—0·21	1·99	17	8th—0·57
March	3·77	25	28th—0·50	4·46	28	2nd—0·70
April	1.37	11	8th—0·32	0·87	6	1st—0·36
May............	3·18	18	26th—0·50	1·02	8	25th—0·28
June	6·30	25	2nd—0·92	2·14	16	21st—0·36
July............	2·71	13	29th—0·50	3·95	27	11th—0·53
August	4·48	28	10th—0·53	2·63	20	22nd—0·55
September ...	2·52	20	16th—0·66	4·33	17	8th—0·72
October	4·47	24	10th—0·72	1·16	15	24th—0·23
November ...	2·51	17	21st—0·53	4·28	24	25th—1·04
December ...	1·63	16	6th—0·86	1·56	15	6th—0·51
	36·38	238		28·66	201	

	1862.			1863.		
	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.
	Inches.		Inches.	Inches.		Inches.
January	2·52	20	31st—0·49	5·81	21	1st—1·81
February......	0·73	10	1st—0·38	1·46	16	4th—0·31
March	3·95	18	24th—1·13	1·01	16	7th—0·32
April	3·53	15	2nd—0·40	1·57	17	5th—0·35
May	5·70	22	7th—1·08	1·94	16	11th—0·62
June	3·49	25	13th—0·44	4·41	22	10th—1·46
July	4·43	21	31st—0·94	1·76	8	21st—0·83
August	3·22	16	8th—0·94	4·96	24	31st—0·89
September ...	5·17	17	3rd—1·22	6·41	26	21st—1·03
October	5·78	20	12th—0·82	6·05	26	30th—1·17
November ...	1·68	15	9th—0·31	3·43	21	3rd—0·73
December ...	3·36	23	9th—0·78	3·22	21	2nd—0·72
	43·56	222		42·03	234	

RAINFALL AT STRINES DALE.

Gauge—6 feet above ground, and 800 feet above the sea.

	1859.			1860.		
	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.
	Inches.		Inches.	Inches.		Inches.
January	1·53	14	17th—0·52	2·81	26	19th—0·28
February......	2·06	19	26th—0·34	0·55	10	8th—0·16
March	3·45	22	14th—1·12	2·80	23	28th—0·49
April	2·79	18	25th—0·42	0·75	14	1st—0·21
May	0·48	3	7th—0·27	3·09	18	12th—0·47
June	2·44	16	5th—0·52	7·73	27	2nd—1·38
July	2·56	13	22nd—0·65	2·63	16	29th—0·48
August.........	6·91	17	7th—2·26	5·10	28	5th—0·72
September.....	5·62	26	30th—0·94	3·10	21	29th—0·48
October........	3·48	19	25th—0.91	4·47	24	15th—0·94
November.....	2·31	16	1st—0·51	3·35	22	21st—0·75
December......	3·05	17	4th—1·02	2·14	20	6th—1·07
	36·68	200		38·52	249	

	1861. Rain Fall.	1861. No. of Days' Rain.	1861. Maximum Daily Rain Fall in each Month.	1862. Rain Fall.	1862. No. of Days' Rain.	1862. Maximum Daily Rain Fall in each Month.
	Inches.		Inches.	Inches.		Inches.
January	0·84	16	16th—0·28	2·39	22	31st—0·38
February......	2·11	17	8th—0·59	0·78	13	1st—0·17
March	4·84	28	2nd—0·96	3·80	16	24th—1·13
April	1·08	9	1st—0·39	2·81	18	6th—0·47
May............	1·14	11	24th—0·29	5·41	23	7th—1·24
June	2·55	16	23rd—0·47	3·29	24	13th—0·47
July	3·93	27	21st—0·59	4·53	20	15th—0·44
August	2·16	21	22nd—0·45	2·81	18	7th—0·88
September ...	4·13	20	8th—0·75	4·74	16	3rd—1·26
October	1·70	17	11th—0·44	4·59	23	15th—0·58
November ...	4·32	23	25th—0·85	1·27	18	14th—0·23
December ...	1·81	17	6th—0·67	2·65	25	9th—0·76
	30·61	222		39·07	236	

	1863. Rain Fall.	1863. No. of Days' Rain.	1863. Maximum Daily Rain Fall in each Month.
	Inches.		Inches.
January	5·02	25	1st—1·54
February......	0·99	15	4th—0·18
March	1·16	18	7th—0·28
April	1·47	19	5th—0·36
May............	1·87	15	14th—0·33
June	4·32	22	10th—1·46
July............	1·87	13	21st—0·95
August	5·27	24	31st—0·86
September ...	5·78	24	21st—0·90
October	5·99	24	30th—1·08
November ...	2·60	22	3rd—0·40
December ...	2·53	22	8th—0·46
	38·87	243	

RAINFALL AT BRUSHES CLOUGH.

Gauge—6 feet above the ground, and 950 feet above the sea.

	1862.			1863.		
	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.	Rain Fall.	No. of Days' Rain.	Maximum Daily Rain Fall in each Month.
	Inches.		Inches.	Inches.		Inches.
January	3·19	19	31st—0·53	6·41	25	1st—1·56
February......	1·01	16	1st—0·19	2·12	19	1st—0·34
March	4·29	21	24th—1·12	1·43	19	7th—0·29
April	4·04	18	5th—0·75	1·93	19	5th—0·54
May	6·49	22	7th—1·41	2·80	18	11th—1·01
June............	4·31	27	12th—0·50	4·40	17	11th—1·02
July	5·69	23	31st—1·05	1·97	9	21st—0·95
August.........	3·33	17	7th—0·95	5·23	22	27th—0·58
September ...	5·45	19	3rd—1·39	7·31	27	21st—1·33
October	6·56	23	12th—0·83	7.40	25	30th—1·38
November ...	1·85	17	9th—0·42	3·39	22	3rd—0·75
December ...	4·66	26	9th—0·96	3·80	21	2nd—0·85
	50·87	248		48.19	243	

Mr. Baxendell read the following "Note on a New Star near the Greenwich Variable, No. 1773 of the 12-year Catalogue."

On the 13th of June, 1861, I made a diagram of the Variable Star No. 1773 of the Greenwich 12-year Catalogue, and three small companion stars as seen with Mr. Worthington's 13-inch Newtonian Reflector, and estimated the magnitudes of the companions to be $a=13{\cdot}2$, $b=13{\cdot}2$, and $c=13{\cdot}7$. No other stars were visible at that time within an area round 1773 having a radius equal to the distance of the companion a, the limit of visibility being the 14·5 magnitude; and the accuracy of the diagram was verified on several subsequent nights.

On the night of the 2nd of October instant, while on a visit with my friend Mr. B. D. Naylor, F.R.A.S., at Bowdon, Cheshire, I turned his newly-mounted achromatic equatorial of 6·2 inches aperture, by Cooke and Sons, of York, on 1773, and observed three small stars near it which I took to be the objects I had laid down in my diagram of June 13, 1861; but it afterwards occurred to me that the star *c* was not in the position shown in the diagram, and therefore on the night of the 6th instant, I directed Mr. Worthington's 13-inch reflector upon 1773, when I at once saw that the star I had taken to be *c* was in reality a new object, all the three stars seen on the 13th June, 1861, being in the positions shown in the diagram, though, strangely enough, *c* was about half a mag. less bright than I had estimated it to be at the time the diagram was made. The magnitude of the new star *d* was estimated to be 13·3. Using the 13-inch reflector again on the night of the 7th instant, careful estimations gave the magnitude of the four small stars $a=13{\cdot}2$, $b=13{\cdot}1$, $c=14{\cdot}1$, $d=13{\cdot}3$.

The sky, for a short time before midnight on the 9th instant, was unusually clear to the west, and the three companions *a*, *b*, and *d*, were seen with the 5-inch achromatic, and their magnitudes, determined photometrically, were found to be, $a=13{\cdot}1$, $b=12{\cdot}8$, $d=13{\cdot}2$.

The relative positions of 1773 and its companion stars are shown in the annexed diagram. The distance of the new star *d* from 1773 is about 48,″ and its angle of position about 315°.

The place of 1773 for 1865 is

	H.	M.	S.
R.A.	19	42	51·6
Dec.	26°	57′	8·6″ N.

From some observations made with the 5-inch achromatic in September, 1863, it appears that *b* was at that time at least *three-tenths* of a magnitude less bright than *a*, which was then estimated to be of the same magnitude as at present. The variability of 1773 was discovered at the Royal Observatory, Greenwich, in 1837, by Messrs. Rogerson and Glaisher; and as *d* was certainly invisible, or at least below the 14·5 magnitude in June, 1861, and *c* is now about half a magnitude less bright than it was at that time, while *b* on the other hand is a full half magnitude brighter than it was in September, 1863, we have here *four* variable stars within an area of little over one square minute, and it is to be remarked that the place of Anthelme's new star of 1670 precedes this singular and interesting group with a difference of right ascension of only about 48 seconds.

Ordinary Meeting, November 1st, 1864

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

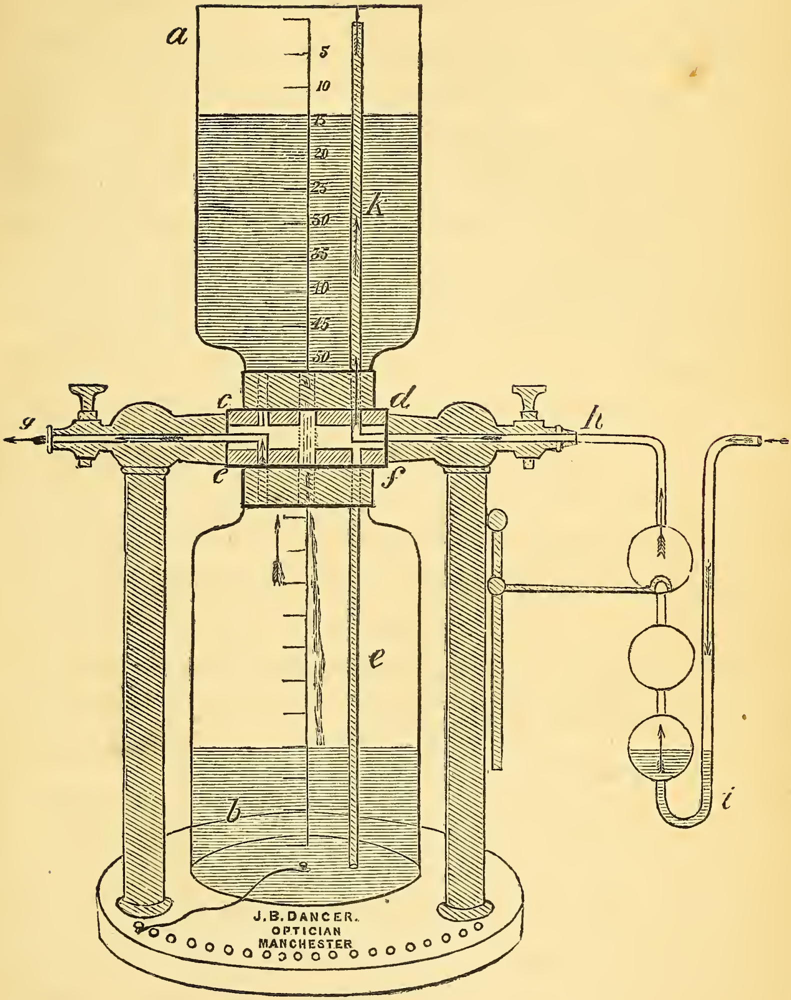
The following gentlemen were elected Ordinary Members of the Society:—Mr. William Cort Wright, F.C.S.; Mr. George Heppel, M.A.; and Mr. William Mather.

Mr. Sidebotham said that he had noticed the common statement that Beech Trees were never damaged by lightning. He had been induced to collect facts on the subject, and had found that out of 28 instances, the trees struck were—Oak, 9; Poplar, 7; Ash, 4; Willow, 3; Horse Chestnut, 1; Chestnut, 1; Walnut, 1; Thorn, 1; Elm, 1, respectively.

Mr. Binney remarked that strokes of lightning were generally determined by the nature of the subsoil, and that in certain localities thunderstorms were very destructive, while in others they were comparatively harmless, and damage by lightning hardly ever occurred. The Beech, it was well known, was generally found growing upon dry, sandy soils, which were bad conductors of electricity, and which therefore acted as protectors against destructive lightning discharges.

The PRESIDENT gave an account of an aspirator which had been contrived for him by Mr. J. B. Dancer, of Cross-street, to be used in the analysis of mixed gases, &c. A drawing accompanies this description. Two jars graduated into parts of a cubic foot or according to pleasure, are mounted in brass, and placed mouth to mouth on an axis *g, h,* or the white portion enclosed by *c, d, e, f.* The upper jar is filled with water and the taps opened; then the water flows from *a* to *b,* the air or gas entering by *h,* and passing previously through any solution that may be used, as at *i.* The gas enters *a* by the tube *k,* the air in *b* goes out by *c.* When *a* is emptied, it is simply turned round by the hand, and *b* the filled bottle stands uppermost. The shaded part of *c, d, e, f,* revolves with the jars, and the openings are so made as to form continuations of the openings in the axis *g, h.* The same conditions exist, no matter which jar is uppermost. This apparatus is very convenient in a laboratory, and has an advantage over other aspirators in measuring the gas. The measurements on the jars are made at a definite pressure of water, and this ought of course to be maintained when the numbers are read off. The apparatus is certainly very elegant, and is an ornamental as well as useful addition to a chemist's work table. It may be called Dancer's Aspirator, or the Swivel Aspirator.

Dr. Boswell Reid describes one somewhat resembling this, but instead of having the swivel movement, it was necessary to lift the whole apparatus and to invert it. It was in reality two aspirators; one emptied itself into the other.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J.B. DANCER.
OPTICIAN
MANCHESTER

Dr. JOULE described the process he employed to harden steel wires for magnetic needles. The wire was held stretched between the ends of two iron rods bent into a semicircular shape. The free ends of the iron rods could be placed in connexion with a voltaic battery by means of mercury cups. Underneath the steel wire a trough of mercury was placed. When the ends of the iron rods dip into the cups the current passes through the wire, heating it to any required extent. When these ends are lifted the current is cut off, while at the same instant the heated wire is immersed in the trough of mercury.

CORRIGENDA.

Mr. KIRKMAN begs the reader to make the following corrections in the last No. of these PROCEEDINGS :—

Page 14, line 11, for 'unexciting,' read *unresisting.*
„ 15, line 1, for 'existence,' read *resistance.*
„ „ line 22, for 'forces,' read *forms.*
„ 16, line 27, for 'centre,' read *centres.*
„ 17, line 2, for 'multiples,' read *multipliers.*
„ „ line 4, for 'then he contracts,' read *thus he constructs.*
„ „ line 21, for 'force,' read *turn.*
„ „ line 3 from bottom, for 'mere,' read *zero.*
„ 18, line 5, for 'vacuum,' read *volume.*
„ „ line 8, for 'deduce,' read *adduce.*
„ „ line 10, for 'bring,' read *any.*

Ordinary Meeting, November 15, 1864.

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

The following letter from Mr. James Nasmyth, C.E., addressed to Mr. Joseph Sidebotham, and dated November 8th, was read:—

I had intended to have sent the Manchester Literary and Philosophical Society "a Paper," embodying some ideas I entertain in regard to the vast antiquity of the features and details of the lunar surface, but in attempting to put my views on this subject into the formal shape of "a Paper," somehow or other my pen won't say what I want it to express, so I am fain to get out of the difficulty by sending you an abstract of my views on this subject in the form of a letter.

The views I entertain on the subject in question are these, namely, that as a direct consequence of the small mass of the moon, and its comparatively large surface, it must have parted with its original cosmical heat with much greater rapidity than in the case of the earth, and consequently the moon must have assumed a final condition of surface structure ages before the earth had ceased from its original molten condition. And as the moon had in all reasonable probability never possessed an atmosphere or water envelope (it certainly has none such now), while the earth has both, the action of the earth's atmosphere, and especially that of its ocean when it existed in the first instance as a vast vapour envelope, ere the earth had cooled down so as to permit the

ocean taking up its final position *as an ocean,* this mighty vapour envelope must have retarded the escape into space of the cosmical heat of the earth millions of ages after the moon had assumed its final condition as to temperature.

Therefore it is from such considerations I am led to the conclusion that the surface features and details of the moon present to us a sight of objects the antiquity of which is so vast as to be utterly beyond the power of language to express, and scarcely less so for the mind to conceive.

But yet at the same time such considerations appear to me to enhance so vastly the deep interest which ever attends the examination and contemplation of the moon's wonderful surface, that I would earnestly urge those who agree with the soundness of these views to bear them in mind next time they have an opportunity to behold the marvellous details of the lunar surface, as I am fain to think that in doing so the interest of what is there revealed to them will be rendered vastly more impressive.

The President read a paper "On the Composition of the Atmosphere." He believed that his inquiry proved that the oxygen test was a very valuable one, as indicating the condition of the atmosphere. The oxygen was diminished in many cases, and indeed in all cases where the air was known to be inferior. He said the objection to such air may perhaps be found not so much in the absence of oxygen as in the gases which take its place. That place was not wholly supplied by carbonic acid. He believed it needful to examine the composition to the second decimal place in the case of oxygen, and to the third or even fourth in the case of carbonic acid, as extremely small amounts of some gases affect us.

Hitherto we have had the composition of the air given in numbers varying a tenth per cent—specimens have generally been taken from rooms on streets or open places indiscriminately. It is the author's wish to show that variations are dependent on the conditions of soil, situation, wind, &c., and that the oxygen and carbonic acid together may with very minute analysis guide us in our sanitary inquiries. The paper cannot easily be given in abstract, further than by adding a table of the analyses showing the average numbers obtained in various places:—

Analyses of Atmospheres varying in Oxygen.

	Oxygen per cent. Avge. Nos.
N.E. sea shore and open heath (Scotland)	20·999
Tops of hills (Scotland)	20·98
In a suburb of Manchester in wet weather	20·98
Ditto ditto ditto	20·96
Front of street $\frac{3}{4}$ths of mile from Exchange, Manchester	20·945
At the back part of the house	20·936
Low parts of Perth	20·935
Swampy places (favourable weather)	20·922 to 20·95
In fog and frost in Manchester	20·91
In sitting room which felt close, but not excessively so	20·89
In a small room with petroleum lamp, well ventilated	20·84
Ditto after six hours	20·83
Pit of Theatre, 11 30 p.m.	20·74
Gallery, 10 30 p.m.	20·36
In large cavities in mines	20·77
In currents	20·65
Under shafts	20·424
In sumps	20·14
When candles go out about	18·5
The worst specimen yet examined in the mine	18·27
Very difficult to remain in for many minutes	17·2

Analyses of Atmospheres varying in Carbonic Acid.

	Avge. of Carbonic Acid per cent.
Manchester streets, usual	0·0403
During fogs	0·0679
About middens	0·0774
Average	0·0442
Fogs excepted	0·0424
Fogs and middens excepted	0·0403
Where the fields begin	0·0369
In close buildings	0·1604
Minimum of suburbs	0·0291
Over North Scotland (towns excepted)	0·0336
Candle goes out	1·8 to 2·5000
Lowest found in mines	2·5000
Lowest entered	4·0000

The greater part of this is given in Dr. Angus Smith's Report "*On the Air of Mines.*"—Appendix to Report of the Royal Mines Commission, 1864.

MICROSCOPICAL SECTION.

First Ordinary Meeting, Session 1864-5.
17th October, 1864.

JOSEPH SIDEBOTHAM, Esq., President of the Section, in the Chair.

The PRESIDENT stated that he regretted to have to inform the members of the total failure of the efforts made during the last summer to provide them with fresh cotton pods for the purpose of investigating into the structure of the cotton fibre. This was partly owing to ravages of the common greenhouse pest, the red spider, and partly unaccounted for as the plants had flowered but not fruited. He called the attention of the meeting to the compact form of microscope made by Mr. Dancer to facilitate seaside and other investigations, where portability, combined with means of using the higher powers, was the chief desideratum. A specimen was on the table, and he and other members could bear testimony to its advantages.

He also called the attention of the members to the many beautiful forms of insects and vegetable life which were frequently neglected as being too small to be examined by the unaided eye, and yet too large for the ordinary powers of the microscope. He assured the members the use of the present three or two inch object glasses would reveal to them many objects of surpassing beauty, which had hitherto only been studied in detail.

With regard to the use of such powers as the $\frac{1}{16}$th or $\frac{1}{25}$th, he thought they seemed to have reached the limits of

the available power of microscopic object glasses, as it appears impossible to separate or define lines more numerous than ninety thousand in an inch, on account either of the decomposition of light or some other cause. It therefore seems beyond our power ever to discover more of the ultimate composition of matter by aid of the microscope, even were we not prevented by the material composition of our lenses and organs of vision.

We have, however, penetrated to the very confines of organic life, if not beyond, inasmuch as no organisms appear to exist smaller than those we can already see.

It is, moreover, a curious fact that the smaller creatures are composed of fewer elements than the larger ones, and that the number of elementary bodies composing them decrease in number as the organisms themselves decrease in size. It becomes therefore a matter for speculation whether the reason of this may not be, that the ultimate atoms of some elementary bodies are larger than others, and that these, from their size, cannot be used in the composition of the more minute forms of organic bodies, and that smaller organisms than those about $\frac{1}{75000}$th of an inch do not exist, because the ultimate atoms of all solid bodies are too large to be economically used in their formation. The telescope appeared to have infinite fields of distance to explore, but it would seem the microscope had nearly reached the limits of its possible power.

Mr. J. B. Dancer, F.R.A.S., then read a paper "On a contrivance for regulating the amount of light transmitted from the source of illumination to the mirror of the microscope."

When viewing certain objects by transmitted light, and particularly with oblique illumination, a very slight alteration in the quantity and direction of the light produces a marked difference in the appearance of the object, especially in Diatomaceæ, where a proper management of the light shows lines or markings invisible under ordinary direct illumination.

The apparatus now exhibited is one easily made at a trifling cost, and consists of a circular disc of blackened tin or cardboard ten or twelve inches in diameter, with a number of perforations of various shapes and sizes—circular, cross-shaped, wedge-shaped, &c.—the centres of which are about 3½ inches from the centre on which the disc, placed perpendicularly, rotates. The form of perforations found generally most useful are parallel slits—slits at right angles to each other—wedge-shaped and circular openings.

The object under view must be well illuminated in the direction required, and then the disc, supported by a pillar, is placed between the source of light and the concave mirror, when a few trials will determine the best form of aperture.

The markings of Pleurosigma fasciola, angulatum, &c., may be seen by its aid under powers which would not show them with any arrangement of achromatic condensers, and it also has the good property of shading all but the amount of light required from the lower portion of the microscopic stage and stand.

The disc might be attached to the lamp, but it appears to work better on a stand, and is susceptible of various modifications which will readily suggest themselves to the microscopist.

Mr. W. H. Heys exhibited specimens of leaves of the vegetable marrow, showing reticulated markings somewhat similar to those of Symphytum, and presented a slide to the cabinet.

Mr. Sidebotham stated that in sweeping over herbage for Coleoptera and other insects, he had found some very curious seeds, to one of which, that of Sanicula Europea, he thought attention had not hitherto been drawn, though well deserving of it. Those of Henbane and Daucus were also most singular.

Mr. Linton exhibited the elegant tufted stigmas of Poterium sanguisorba, and the very singular calyx of the gum Cistus, which might almost be mistaken for the skin and scales of a fish.

Ordinary Meeting, November 29th, 1864.

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

Mr. E. C. Buxton was elected an Ordinary Member of the Society.

Dr. Joule exhibited a magnetic needle for showing rapid and minute alterations of declination. It consisted of a piece of hardened and polished watch spring, an inch long and one-tenth of an inch broad, suspended vertically by a filament of silk. The steel was magnetised in the direction of its breadth. He remarked that Professor Thomson had long insisted upon the advantages which would attend the use of very small bars in most magnetical investigations, and had employed excessively minute needles in his galvanometers with great success. Dr. Joule stated his intention to fit up his needle so as to be observed by light reflected from its polished surface, or otherwise by viewing a glass pointer, attached to the bottom of the steel, through a microscope. He believed that by the latter plan he should be able to observe deflections as small as 1″ of arc.

A paper was read "On Differential Equations," by His Honour Chief Justice COCKLE, M.A. F.R.A.S., F.C.P.S., &c., President of the Philosophical Society of Queensland; communicated by the Rev. ROBERT HARLEY, F.R.S., &c.

Importing an arbitrary constant C into a result which Mr. Robert Rawson, an Honorary Member of the Manchester Literary and Philosophical Society, in a letter dated Portsmouth, June 14th, 1862, communicated to me, we may say that the differential resolvent of a (trinomial) cubic, whereof the coefficient of the second term vanishes and that of the third is constant, is of the form

$$\frac{d^2y}{dx^2}-\frac{f''(x)}{f'(x)}\frac{dy}{dx}-C\left\{f'(x)\right\}^2 y=0\text{.........}(1)$$

Put

$$f'(x)=\frac{df(x)}{dx}=X, \quad C=K^2,$$

then, (1) divided by X^2 becomes

$$\frac{1}{X^2}\cdot\frac{d^2y}{dx^2}-\frac{X'}{X^3}\cdot\frac{dy}{dx}-K^2y=0 \text{ ....}(2)$$

Now, if

$$L=\pm K, \ M=\mp K,$$

then

$$\left(\frac{1}{X}\cdot\frac{d}{dx}+L\right)\left(\frac{1}{X}\cdot\frac{d}{dx}+M\right)y=0\text{.........}(3)$$

is the symbolical decomposition of (1) or (2). When L and M are any constants whatever, (3), when developed, gives rise to a linear differential equation of the second order reducible to an equation with constant coefficients by changing the independent variable from x to t, where

$$t=\int\frac{dx}{X^{-1}}=\int X dx$$

In deducing from (3) its development, the order of the symbolical factors is indifferent, but the two particular integrals of the development are, I think, obtainable by reversing in (3) the order of the symbolical factors. The differential resolvent of every such trinomial cubic as that discussed by Mr. Rawson is soluble by a change of the independent variable, and belongs moreover to a comparatively simple form of equation soluble by such change.

The theory of Transcendental Solution has led me to the following proposition (theorem):—

If an irreducible algebraical equation of the degree n have a homogeneous linear differential coresolvent of the order m, then any root whatever of the algebraical equation can be expressed as a linear and homogeneous function of any other m of its root.

The general demonstration would not be much more difficult than or very different from the particular demonstration of the case $m=2$. The converse of this theorem, I believe, is true.

In such case let a and b be the particular integrals of the differential resolvent which (since $m=2$) is by hypothesis of the second order only. Let α, β, and γ be any three of the roots of the algebraical equation. Then, since among the values that can be assigned, by means of the arbitrary constants, to the general integral, the roots of the algebraical equation are included, we have three such relations as

$$\mathrm{A}a + \mathrm{D}b = \alpha,$$

$$\mathrm{B}a + \mathrm{E}b = \beta,$$

$$\mathrm{C}a + \mathrm{F}b = \gamma,$$

wherein A, B, .. , F are constants. Multiplying the first, second, and last of these equations into the arbitrary multipliers λ, μ, and ν, and adding the results, we have

$$(\lambda A + \mu B + \nu C)a + (\lambda D + \mu E + \nu F)b = \lambda\alpha + \mu\beta + \nu\gamma \ldots\ldots (4)$$

Hence if the ratios of any two of the quantities λ, μ, ν to the third be so assigned as to satisfy the equations

$$\lambda A + \mu B + \nu C = 0,$$

$$\lambda D + \mu E + \nu F = 0,$$

then the sinister of (4) will vanish independently of a and b, and the homogeneous linear relation

$$\lambda\alpha + \mu\beta + \nu\gamma = 0 \ldots\ldots\ldots (5)$$

will subsist among the roots α, β, and γ of the algebraical equation. When $n=3$ we (since the differential resolvent is homogeneous) have without reference to, but consistently with, the theorem,

$$\alpha + \beta + \gamma = 0 \ldots\ldots\ldots (6)$$

Combining the above theorem with one given by Abel and Galois, we conclude that:—

If an algebraical equation have a differential resolvent of the second order, the algebraical equation is resoluble algebraically.

Before closing I would add that, as it seems to me, it would be more consonant with the notation and practice of the rule of three, and, therefore, with convenience and the analogies of arithmetic, if by the ratio $p:q$ there were universally understood (not the fraction $p \div q$, but) the fraction $q \div p$.

Mr. R. D. Darbishire, F.G.S., read a paper entitled "Notes on Marine Shells found in Stratified Drift at Macclesfield," and exhibited a series of specimens.

The specimens were chiefly collected by Mr. W. J. Sainter and Mr. Lowe, of Macclesfield, from sand and gravel exposed in the formation of the new Cemetery on the north side of the town, at an elevation of between 500 and 600 feet above the level of the sea. Unfortunately the buying of specimens had caused the intrusion of many spurious fragments, casting suspicion on several that might after all prove to be genuine.

The beds in question were exposed on a south-easterly face, but are now defaced by ballast tips; consist of fine (running) sand, fine and coarse shingle, and very coarse gravel with large pebbles unscratched; and, while stratified, in general horizontally, exhibit in their great irregularities of extension, level and false bedding, characteristically marine aspect, as of a sea bottom under the influence of tidal and other varying currents.

Below appears the "lower boulder clay" of the Ordnance geologists.

The shells are nowhere numerous. Mr. Lowe speaks of finding some in layers. Unfortunately the shells from particular beds have not been distinguished.

In the list specimens obviously spurious have not been noticed. The following species had been identified:—

Pholas crispata.
Pholas candida.
Mya truncata.
Mya arenaria.
Psammobia ferroensis.
Donax anatinus.
Tellina solidula.
Mactra solida.
Lutraria elliptica.
Cytherea chione.
Venus striatula.
Artemis lincta.
Cyprina islandica.
Astarte elliptica.
Astarte arctica.
Cardium echinatum.
Cardium aculeatum (?)
Cardium rusticum.
Cardium edule.
Cardium Norvegicum.
Mytilus edulis.
Modiola modiolus.
Nucula sp.
Arca lactea.
Pectunculus sp.
Pecten opercularis.
Ostrea edulis.
Patella vulgata.
Dentalium entale.
Dentalium Tarentinum.
Trochus cinerarius.
Littorina littorea.
Littorina rudis.
Littorina littoralis.
Turritella communis.
Aporrhais pes pelicani.
Natica nitida.
Natica monilifera.
Murex erinaceus.
Purpura lapillus.
Nassa reticulata.
Nassa incrassata.
Buccinum undatum.
Fusus gracilis.
Fusus antiquus.
Trophon clathratum.
Mangelia turricula.
Mangelia rufa.
Mangelia nebula.
Cypræa Europæa.

Total, 50 species.

Mr. Darbishire compared this list with those of the Moel Tryfaen drift (Caernarvon), and the Kelsey Hill (Hull) fossils, and with several lists of recent faunas of the British

Eastern and Western seas, and of seas North and South of the British Isles.

The present list was specially remarkable for including *Cytherea chione, Cardium rusticum, Cardium aculeatum* (?), and *Arca lactea;* all of them shells reaching their highest northern range in the extreme south or west of England and Ireland, a circumstance believed to be new to the history of the so-called "Drift."

The Macclesfield list rendered probable the deposit of those beds from the westward, after the period had commenced during which the physical conditions of the Western Sea have differed as they now do from those of the Eastern.

A depression of 600 feet would leave only a few islands where Ireland is, would allow of a great extension of the tidal current now narrowed in St. George's Channel, and would probably carry the warmer influences which are now checked on the West of Ireland to the shores of the Derbyshire hills. Are there any traces of the sea bottom or shores on which the shells of the drift seas *lived?*

Mr. Darbishire further mentioned his identification in a bed of gravel discovered by Mr. Prestwich, F.G.S., at about 1,200 feet above the sea, on the east side of Macclesfield, of nine species of shells, *including Cytherea chione.*

Mr. Binney, F.G.S., remarked on the extent of the list, the great distance eastward from the present shores of the deposit, the elevation of Mr. Prestwich's patch, and the peculiarly temperate aspect of the group of shells. This drift could not be called "arctic." He also referred to the difficulty of recognising distinct "upper" and "lower" boulder

clays; and illustrated the partial removal of the drift gravels and the underlying till over a large tract of western country during its elevation, and the re-distribution of portions of the same beds as eroded by water courses after the land had risen above the sea.

Dr. ALCOCK had examined the fossils produced, and especially the southern forms, and confirmed the identification of species.

Ordinary Meeting, December 13th, 1864.

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

Mr. Edward Sonstadt was elected an Ordinary Member of the Society.

Mr. Binney, F.R.S., exhibited some spores of plants found in the splint coal of Methill, Fifeshire. He said that many years ago he read a paper on some similar fossils before the Geological Society of London, and it was printed in the Journal of the Society for May, 1849. Those specimens were from a nodule found in the King Coal seam at Wigan. They also were met with in the Wigan Four Feet Coal in greater abundance. Since that time Professor Balfour, F.R.S. of Edinburgh, had described, in a paper printed in vol. xx of the Transactions of the Royal Society of Edinburgh, some similar fossils from the Fordel splint coal. The specimens exhibited were small lenticular bodies of a chestnut colour, about a line in diameter. They occurred in countless numbers, indeed forming a very considerable portion of the seam of coal itself. He stated that he had found them in equal abundance in the Eight Feet, Main, Wood, and Pirnie Well seams, but always in the splint or bone part of the coal. Dr. Hooker had proved that similar spores belonged to the Lepidodendron. The thick coating of the spore has doubtless afforded some protection to it as well as the peculiar process of bituminisation to which splint coal has been subjected, and different from that which soft or cherry coal has undergone. He said that when we considered the great abundance of these small fossils in all splint coals, and the immense

number of the roots of Sigillaria found in the floors of such seams of coal, it was almost certain that they had some connection with that plant. This tended to confirm M. Adolphe Brongniart's opinion, expressed many years ago, that Sigillaria and Lepidodendron were plants very nearly allied to each other. Mr. Binney also exhibited some larger spores in a nodule of clay ironstone given to him by Mr. Ward, of Longton, and found in the Derbyshire coal field. These were in a most beautiful state of preservation, and exhibited the tesselated character of the outside of the spore.

A paper was read by Mr. William Brockbank, "On the Discovery of the Bones of the Mammoth (Elephas primigenius) in a Fissure of the Carboniferous Limestone at Waterhouses, near Leek."

A considerable number of bones were found at Waterhouses some weeks since, but through ignorance of their real character they became dispersed without attracting attention, a good many having been used to manure the land by a neighbouring farmer.

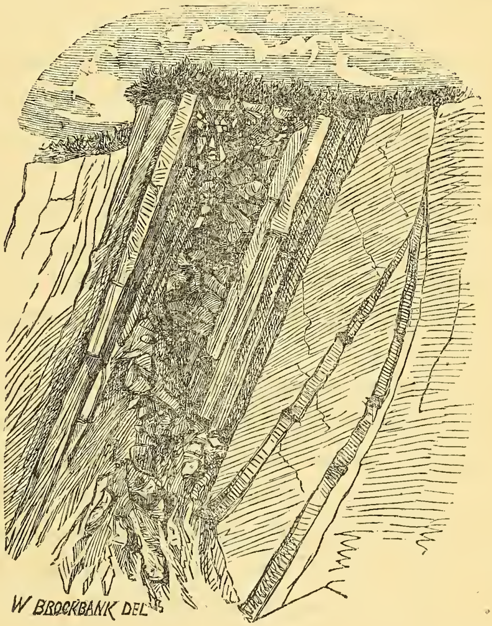
A few of these bones reached the author through Mr. Smith, of Cheddleton Mills, and were at once identified as belonging to the skeleton of an elephant. A further search was determined upon, and the author, accompanied by Messrs. Thomas Wardle of Leek, and J. Walsh and W. Smith of Manchester, visited Waterhouses on the 9th instant, and succeeded in finding a large number of bones.

Mr. Wardle, and Mr. Green of the Geological Survey, again visited the place on the 12th instant, and found very decided fragments of teeth. A further search is being made at the present time.

A large number of bones were submitted to the Society, all of which were considered to be those of the Elephas primigenius, amongst which were one humerus nearly complete. and part of the second; parts of the pelvis and scapula; one

ulna; several carpal and metacarpal bones; the head of the tibia; several fragments of tusks and two fine fragments of teeth, showing very clearly the peculiar narrow transverse plates and ridges of the dentine and enamel, by which the teeth of this elephant are distinguished.

The fissure in which these bones were found occurs in the upper beds of the carboniferous limestone, and has been exposed by the workings of a quarry. The limestone strata at Waterhouses are much dislocated, and fissures frequent, the



OSSEOUS FISSURE AT WATERHOUSES.

river Hamps disappearing in dry weather through fissures very near to the above quarry, and other streams in the immediate neighbourhood were described as sinking in the same manner. The face of the limestone in the quarry is nearly parallel to the general direction of the valley, or nearly east and west, and the fissure follows the dip and direction of

the strata, being nearly vertical, or inclining about 10° to the north. It is about six feet in width from face to face of the solid rock, and is filled up with angular blocks of limestone, cemented together at the sides of the fissure into a solid breccia, the stones being coated with stalagmite, whilst the centre is filled in with angular rubble and damp ochreous clay. The whole had evidently been filled in from above.

The bones were recovered in good condition from the breccia on the dryer side of the fissure, but those occurring amongst the damp clay and rubble were so friable that it was quite impossible to save them. Large numbers af ivory flakes were found, which proved to be the remains of the teeth, and one large fragment of tooth was obtained which was decomposing into these flakes.

At the furthest point reached, a very interesting group of bones was discovered, viz., a humerus in the socket of the scapula, with the head of another humerus resting upon it at the other end, and two cervical vertebræ were found near the scapula. These were the only bones found in their relative positions.

It was conjectured that the mammoth had fallen into the narrow fissure before it was filled in, its huge bulk preventing its reaching the bottom, so that it remained jammed in until by natural decay it fell to the bottom, bit by bit. By this supposition the absence of the head was accounted for, as it would probably fall off first, and would roll lower down the chasm. This surmise is confirmed by the fact that Mr. Wardle found several fragments of the teeth on his second visit, fifteen or twenty feet below the point where the bones occurred.

The author had not been able to find any record of the occurrence of the remains of the mammoth in any work on the geology of Derbyshire or Staffordshire; and Mr. Wardle, who has recently published an interesting account of the geology of the neighbourhood of Leek, believed it to be an entirely

new discovery. A considerable part of the fissure remains to be explored, and a further search is being prosecuted by Mr. Wardle and the author.

Dr. White considered it probable that among the bones exhibited there were the remains of more than one animal.

Mr. Binney said that Mr. Brockbank was mistaken in supposing that no remains of the elephant had hitherto been found in Derbyshire. They had been met with in two localities in the county of Derby and one in the county of Chester. The late Mr. White Watson, at page 58 of his "Delineation of the Strata of Derbyshire," says:—"About the year 1663 a large cavern was discovered in sinking for lead ore upon a hill at Balleye, within two miles of Wirksworth; in which a large skeleton was found, which in the original account of its discovery is said to be 'that of a man, that his brain pan would have held two bushels of corn, and that it was so big they could not get it out of the mine without breaking it.' Several of its teeth were distributed in the neighbourhood, one of which, with the author's account of the discovery, is in the writer's possession. The tooth is ivory, and when compared with the *dentes molares* of an elephant, no difference can be found; from this circumstance it is evident that the skeleton found could not have been that of a man or giant, so called by the miners, who are ever prone to the marvellous, but must be indisputably that of an elephant, and its capacious brain pan a corresponding proof; for after the miners had conferred on it the appellation of the giant's tooth, the brain pan must naturally follow the proportions of its bulky owner. The fangs, though perfect at the time of the discovery, are now broken, and no change appears to have taken place from its original substance. Several of these teeth were brought out, but the skeleton left behind, which, it is to be lamented, cannot now be viewed, that part of the mine having run in, rendering it impracticable without much trouble and expense." About twenty-five years since the late

Mr. James Meadows, of the Ashton Canal Company, presented to the Manchester Geological Society a portion of the tusk of an elephant which he had found in a limestone fissure at Doveholes, near Chapel-en-le-Frith. This specimen is now in the museum of the Society in Peter-street. The late Mr. F. Looney, F.G.S., in his List of Organic Remains, published with Mr. Elias Hall's Geological Map, in 1836, in alluding to the fossils found in the "superficial gravel," says: "Part of a molar tooth of the Asiatic elephant was found at Adlington, near Macclesfield, and is now in the possession of William Clayton, Esq." With these three exceptions he had never heard of the remains of the elephant having been met with in the counties of Derby and Chester, and he felt much obliged to Mr. Brockbank for taking the trouble to collect the fine series of remains on the table, and exhibit them to the members of the Society.

MICROSCOPICAL SECTION.

November 21st, 1864.

Joseph Sidebotham, Esq., President of the Section, in the Chair.

Mr. Watson exhibited and presented to the Section a dozen slides showing the battledore scales of several species of Polyommatus or Lycæna; he also illustrated the subject by drawings of the scales made by Mr. Sidebotham. He showed that the scale of Bæticus has manifestly no relation to the others, though this insect is placed at the head of the list of the genus Polyommatus by Boisdudal, H. Schaeffer, Dr. O. Standinger, and W. F. Kirby. Many lepidopterists have thought that it is a Thecla, and he proposed to examine various species of the genus Thecla and to report the result.

A letter from Captain Mitchell, of Madras, was read. He stated that he had examined fresh but full grown cotton treated with Mr. O'Neill's strong and weak solutions of copper, and had failed to detect any spiral fibres. He communicated some observations on a Lepidopterous larva, probably that described by Mr. Saunders in vol. iii. of the Transactions of the Entomological Society, which attacks the cotton pod. Wherever it was found, the cotton was entirely destroyed.

The Secretary read Mr. Edwards's directions for collecting Diatomaceæ.

November 28th, 1864.

JOSEPH SIDEBOTHAM, Esq., President of the Section, in the Chair.

Specimens Exhibited.

Physcomitrium sphœricum, by Mr. G. E. Hunt, found sparingly in three places on the borders of Mere Mere, this being only the second occasion on which it has been gathered in Britain. The first was thirty years ago, when it was detected in the same place by Mr. Wilson in the autumn of 1834.

Four species of Curculionidæ, new to Britain, by the President, as exhibited and examined at the Entomological Society, London; Lixus filiformis and Sibynes canus, Devizes; Ceuthorhynchidius poweri, Silverdale; and Peritelus griseus, Ventnor, Isle of Wight. Three of the species were captured by himself. [See *Zoologist,* December, 1864.]

A martin, entirely white, from Urmstone, by Mr. Linton.

Communications.

Mr. G. E. HUNT announced his discovery of Potamogeton nitens in Loch Ascog, Rothsay. This plant was first observed as British by Mr. David Moore, Dublin, in a lake near the sea, at Castle Gregory, county of Kerry, in July last, [See *Seeman's Journal of Botany*, November, 1864.]

THOMAS ALCOCK, M.D., read a paper on specimens from Roundstone, Connemara. He exhibited many specimens, including forty-three species of Foraminifera, two of which were forms of Entosolenia, hitherto undescribed; also, young shells of Patella vulgata and P. pellucida, with the larval shells still attached and distinctly spiral, evidence not before recorded.

PHYSICAL AND MATHEMATICAL SECTION.

November 10th, 1864.

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

Professor Clifton exhibited an acoustical electric telegraph, by which a note, sounded at one end of the line, is reproduced at the other end.

He also pointed out the principles involved in the construction of this telegraph, viz.:—

1st, The production of a sound whenever a current of sufficient strength commences to circulate round an electro magnet, or ceases so to circulate.

2nd, The vibration of a stretched membrane in accordance with a note sounded near it.

With respect to the second principle, Professor Clifton drew attention to the fact, that the researches of MM. Bourget and Bernard, in agreement with the mathematical investigations of Poisson and M. Lamé, show that a given square membrane will *not* vibrate in accord with *any note,* as stated by Savart. As the same is probably true of circular membranes, such as that used in this telegraph, it follows that only certain notes are capable of being transmitted by one instrument.

December 8th.

ROBERT WORTHINGTON, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. BAXENDELL read a "Note on the Period and Changes of the Greenwich Variable in Vulpecula, No. 1773 of the Twelve-Year Catalogue."

In No. 1500 of the *Astronomische Nachrichten*, Dr. Schönfeld, Director of the Observatory at Mannheim, expresses a doubt as to the variability of several of the stars in Mr. Chambers's Catalogue of Variable Stars. One of the objects which he thus points out as doubtful is No. 1773 of the Greenwich Twelve-Year Catalogue, which was entered in Mr. Chambers's list on my authority, as I had satisfied myself from occasional observations made since 1861, that its light was subject to periodical changes, though not to the extent indicated by the observations of Messrs. Rogerson and Glaisher, made in 1837. I have since reduced my observations, and have obtained from them the following times of maximum and minimum brightness:—

Maxima.	Minima.
1862, Oct. 20.	1862, Nov. 24.
1863, Sep. 12.	1863, Oct. 25.
,, Nov 19.	,, Dec. 25.
1864, June 15.	1864, May 13.
,, Aug. 21.	,, July 21.
,, Nov. 8.	,, Oct. 7.

Treating the equations formed from these data by the method of least squares, we have, from the observed maxima,

Period=67·97 days.

Epoch=1864, Feb. 1·37.

And from the observed minima,—

Period=67·88 days.

Epoch=1864, January 1·59.

The mean of the two values of the period is 67·92 days. The interval, from minimum to maximum brightness, is 30·8 days, and from maximum to minimum 37·1 days. This variable, therefore, like many others, increases in brightness more rapidly than it diminishes. Its magnitude at maximum is 8·8 and at minimum 9·8, the range of variation being, therefore, one magnitude. It is one of the highly-coloured stars, both Mr. Hind and myself having always noted it as being *very red.*

Mr. Baxendell also communicated the following "Observations of the Greenwich Variable in Vulpecula and its Companion Stars. By George Knott, Esq., F.R.A.S., of Woodcroft Observatory, Cuckfield, Sussex."

1864, Nov. 5.—I have this evening examined the Greenwich Variable in Vulpecula and its neighbours. The state of the atmosphere was not by any means favourable, and I could not make out *c;* but *a*, *b*, and *d* were well in view, and *b* decidedly the greatest of the three. I inclined to estimate *b* about 12½ magnitude; and by the method of reduced apertures I found

a=12·8 mag.	d=13·0 mag.
b=12·6 „	1773= 9·1 „

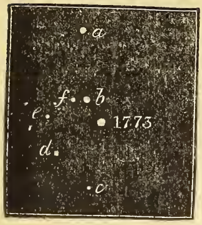
The star 1773 is the Greenwich variable; and *d* is the new star announced by Mr. Baxendell in his communication in the Society's Proceedings, No. 2, Session 1864-5.

Nov. 22.—I secured a hasty, and therefore comparatively valueless observation. After a hasty glance clouds came up, and I could not gauge the magnitudes as I was preparing to do. My rough results were—

a=13·0 mag.	c, invisible.
b=12·9 mag.	d=13·2 mag.

Nov. 26.—On this evening I entered in my journal—"*b*, 12·9; *a*, 13·0; *d*, 13·2. Is there a minute speck at *e*?"

Dec. 1.—This evening it is very clear, and I not only see most clearly a small star in the place indicated, but also that the star *b* has a small and pretty close *comes!* In the annexed diagram *e* and *f* are the new stars. I am not sure of their exact position, but have no doubt of their existence. The following are my rough estimations of magnitude:— *a*, 12·9; *b*, 12·8; *c*, 14±; *d*, 13·1; *e*, 13·7 (?); *f*, about = *e*.



The above observations were made with an equatorially mounted achromatic of $7\frac{1}{3}$ inches aperture, constructed by Mr. Alvan Clarke, of Boston, U.S.

Ordinary Meeting, December 27th, 1864.

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

Mr. John Robinson and Mr. Joseph Spencer were elected Ordinary Members of the Society.

The bones of an adult gorilla from the Museum of the Natural History Society were exhibited, and some remarks on them were made by Edward Lund, Esq., F.R.C.S. *Ex.*, his object being to point out the characteristics of the gorilla skeleton by a direct comparison with the bones in man.

He said that when the bones of this gorilla were shown to him at the Museum, the first he took up happened to be the highest bone of the neck, the atlas, and he was much struck by its close resemblance to the same bone in man; it was however larger, while the hole in the transverse process for the passage of the vertebral artery was very much smaller, indicating a less supply of blood to the brain. With regard to the age of the specimen, he had no doubt that the skeleton was that of an adult, for all the bones were thoroughly ossified and the teeth were much worn down and filled by a secondary deposit of cementum. As to sex, he could not speak positively, for he did not know how far the differences in the pelvis observable in the human subject would hold good in the gorilla, but he pointed out the angular shape of the pelvic arch, which would lead to the supposition that the animal was a male.

He said he would now take the bones in detail, grouping them as the head, the trunk, and the limbs, and would indicate their peculiarities by showing how they differ from the

same human bones whether by deficiency or excess of parts. The skull would attract attention at once by its very peculiar shape, which all seemed to agree in calling helmet-shaped; if however the prominent ridges which give it this appearance were left out of consideration, it would be found to be well formed and fitted to receive a well-developed brain. The front was indeed singular; for the orbits, instead of being excavated as it were out of the regular dome of the skull as in man, were outstanding, and that to such a degree that a perpendicular section which would remove them entire would scarcely lay open the cavity of the skull. It would be seen however that they formed perfect chambers, well walled in on all sides, showing that great care had been taken to protect the organs of sight. He pointed out the great depth and extent of the temporal fossæ, and remarked that they had something of the character observable in the Felinæ, the temporal muscles were very large and were so much expanded over the sides of the skull that they went completely up to the middle line. These large temporal muscles, which close the jaws with a powerful snapping action, had to do, he said, with the very strong and large canine teeth, which would be capable of exerting great force in tearing anything on which the animal might feed. The number of teeth was the same as in man, and they were of the same kinds, namely, in each jaw four incisors, two canines, four premolars, and six molars. The characters of the nasal fossæ indicated a moderate, not an acute sense of smell. In the foramen magnum he observed this difference, that while in man it is oval, in the gorilla it was almost circular or squarish. The base of the skull was convex as in man, corresponding with a concave internal surface upon which the brain would rest. The mastoid processes, which phrenologists considered to indicate destructiveness, but which are really a part of the organ of hearing, were not quite so well developed as in man. In the present case one

of them happened to be broken so as to expose the interior, and, if we might form the same conclusions from it as we should in the human subject, its condition would strengthen the opinion that the animal was of mature age, for in early life these processes consist of solid bone, but as age advances are gradually excavated into larger and larger cells, and in the specimen exhibited these cells were very large. The lower jaw was remarkably large and strong, and the ramus formed a right angle with the body of the bone as it does in middle life in man. There was one peculiarity which he had observed in the condyle, namely, that the smooth articulating surface extended very far round the back of it, indicating that the mouth could be opened to a great extent, and this, together with the formidable teeth, would no doubt give the animal, to say the least of it, an alarming appearance if suddenly encountered in its native wilds.

The bones of the neck were, as usual in the mammalia, seven in number; they were very well joined together, the upper surface of their bodies being concave from side to side to receive the convex lower surface of each one above. They had well marked additional processes or rudimentary ribs, but the most remarkable feature was the great length and strength of the spinous processes, which would give advantageous attachment to very powerful muscles capable of exerting immense force in twisting the head from side to side as in the action of tearing anything with the teeth. The general appearance of the dorsal and lumbar vertebræ agreed with the human bones; the ribs were twenty-six in number instead of twenty-four, and as several of them remained in their natural position, connecting the sternum with the spine, it could be seen that the sternum, instead of having a nearly vertical position as in man, was very oblique, the lower part projecting forward and showing a great capacity of chest. The sacrum was unlike the human sacrum, being much longer and narrower. The haunch bones were also

long in the direction of the length of the body, and were but slightly roughened for muscular attachment, a proof that the gluteal muscles were not much developed. The tuberosities of the ischia, however, were very strongly marked, indicating that the animal is well qualified for the sitting posture.

The upper extremities were very largely developed in proportion to the lower; in this specimen the length between the tips of the fingers of the outstretched arms is said to have measured eight feet. The shoulder blade had a peculiarity which anatomists formerly thought characteristic of man, namely, that the vertebral border was the longest. The arm bone looked immensely large when placed beside that of man; the roughnesses near the upper part, for the attachment of muscles going to the chest and back, indicated that these muscles were of extraordinary power, and when acting together would enable the creature to hug an enemy with terrible effect. The articulating surface at the lower end of this bone was oblique as in man, so that the bending of the elbow carried the hand not to the shoulder, as it would if the hinge-joint was directly transverse, but inwards to the mouth, furnishing a curious little illustration of the prehensile character of the upper extremity, and its essential use as an organ for obtaining food. The deep hollows at the front and back of the bone for the reception of projections of the ulna when the elbow is completely flexed or extended, were in the gorilla joined so as to become a hole, and thus gave even a greater extent of motion to this joint than in man. The bones of the forearm were very large, and a peculiarity in them worth noticing was that they were curved, giving them great power of resisting fracture by allowing a slight spring of the bones when subjected to a direct shock, and also adding immensely to the strength of the wrist in pronation and supinatiou, by increasing the distance between the bones and therefore allowing of a greater length of muscle

and better leverage. (The bones of the hands and feet were not shown, being at present with the skin.) The femur was shorter than the humerus, the reverse of what we find in man. The leg bones were also short, and the lower extremities had altogether the appearance of being less fully developed than the upper. The fibula was considerably shorter than the tibia, while in man the two bones are as nearly as possible the same length. The lower end of these bones formed a very imperfect socket for the ankle joint compared with the same parts in man, where it is most perfectly adapted for sustaining the erect position.

As to the habitual attitude of the gorilla deduced from an examination of the bones, the characters of the ankle joint just alluded to show that though the creature might stand erect it could have but little stability in that position, and progression on the hind legs alone would be difficult and could not be long sustained. Important evidence on the same point was also furnished by the character of the lower part of the vertebral column and the position of the pelvic bones with regard to it, for, as might be seen, there was no deep hollow behind, as in man, for the attachment of the erector spinæ muscles, the sacrum in this case being even placed further back than the iliac bones; the form of the pelvis too, and its direction with regard to that of the backbone, showed that it was fitted for the support of the contained viscera in the horizontal rather than in the erect position; and all these points led to the same conclusion, namely, that the gorilla habitually uses the front as well as the hind legs in progression.

MICROSCOPICAL SECTION.

December 19th, 1864.

J. Sidebotham, Esq., President of the Section, in the Chair.

Exhibitions.

The skeleton of an adult gorilla, by Thomas Alcock, M.D. The more striking peculiarities of the gorilla skeleton were explained by comparison with the human bones, by Edward Lund, Esq., F.R.C.S. *Ex.*

Specimens of comatula rosacea, from the Cove of Cork.—Thomas Alcock.

A jay, with the beaks crossed and very much overgrown.—A. G. Latham, Esq.

Mounted foraminifera, from Dogs Bay, coast of Galway.—Thomas Alcock.

Communications.

Mr. Latham gave the result of his examination of a shell of Helix nemoralis brought by Mr. Glover, from the shore of Gorteen Bay, Connemara. The weight of the shell was 56 grains, while that of an ordinary specimen was found to be only 16 grains. By drying it lost a grain of weight, and by calcination, two grains. It was then powdered and washed with a loss of one grain of soluble salts, the whole loss of weight being 4 grains. The residue was perfectly soluble in muriatic acid. Mr. Parry showed a section of a shell of Helix aspersa, brought by Mr. Glover from the same locality; before cutting, it weighed 126 grains, and the section exhibited very clearly its unusual thickness and solidity.

Mr. W. H. Heys read a communication on the structure of cotton. He said the twisted appearance so often alluded to is caused simply by the collapse of the cell-walls in drying, and is not an actual twisting. Captain Mitchell's experiment with the ammoniacal oxide of copper was not conducted in the manner necessary for showing the spiral threads which have been observed within the outer cell-walls, for in his letter read at the last meeting, he speaks of having put cotton into the solution four months ago, examining it at intervals, frequently at first, and then daily for some time, whereas the action of this solvent is so rapid that the microscope requires to be prepared beforehand, as in observations on crystallization, and it is sometimes difficult even then to follow the changes which take place. Mr. Heys exhibited the action of the solvent under the microscope, and also showed many drawings of cotton which had been similarly treated. He said that if a single fibre of the cotton be selected and watched it will be seen first to become inflated, not uniformly, but with many constrictions, giving it the appearance of an irregular string of beads. After a time the external envelope will entirely disappear, leaving only fragments of the spiral thread, of which the portions corresponding with the constrictions will be seen to have the form of rings. Having exhibited these appearances, he said he would leave the members to form their own opinion upon the structure of cotton, but repeated the conclusion he had arrived at, that it is composed of an external envelope or tube, within this a spiral thread, preventing its collapse, and a pith-like substance in the centre.

Mr. Nevill showed mounted specimens, and handed in a list of 43 forms of Foraminifera, found in sand from the shore of Gorteen Bay, Connemara. Some of the kinds, he said, were not figured in Professor Williamson's Recent Foraminifera, and among these he particularly mentioned a form of Miliolina,

with an appendage like a tail at the end opposite the septal orifice. He specially called attention to his mode of mounting the specimens in many small cells upon a single glass slip, by which much room is saved, and other advantages are gained; in those shown, ten small cells were punched out of an oblong piece of card, in two rows of five each, and the whole covered with a single glass He remarked on the extraordinary assemblage of species found in this Connemara sand, which, Mr. Dancer suggested, might be due to the influence of the Gulf Stream.

PHOTOGRAPHICAL SECTION.

Ordinary Meeting, Nov. 3, 1864.

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

The following gentlemen were elected associates, viz.—Messrs. William Pegg, T. D. Thorpe, George Wardley, E. G. Hughes, William Lockett, Thos. Heywood, G. W. Mosley, F. C. Tobler, A. Lees, and W. G. Coote.

In the absence of the President, the Lord Bishop of Manchester, the Chairman delivered an inaugural address, in the course of which he referred to the three aspects in which photography naturally presents itself to our notice—as an amusement, as a science, and as an art.

Mr. A. BROTHERS exhibited some portraits which he had taken by means of the magnesium light. He had discarded the use of all reflectors; the experiments made were generally with three strands of wire, two flat ones bound together with a round one. Mr. Brothers also submitted a panoramic picture taken in the new revolving camera, and which was perfectly sharp and distinct in every part.

Mr. WARDLEY presented to the Album twelve very beautiful photographs of scenery in Cumberland, Westmorland, &c., taken by the Taupenot process.

Some photographs printed by the Wothlytype process were exhibited and much admired.

Mr. JOSEPH SIDEBOTHAM then read his Paper "On Printing Transparencies for the Stereoscope and Magic Lantern," in the course of which he called attention to the fact that we were indebted to Mr. Dancer for inventing the binocular camera, without which the stereoscope and its attendant pictures would not have been, as at present, found in almost every home. Mr. Dancer's idea, that the pictures should be taken only at a distance of three inches apart, was at first ridiculed; now, however, that distance is almost universally adopted.

Early in the history of the stereoscope the French transparent views, on albumen, were eagerly purchased, although the price was very high, and was still so.

Mr. Sidebotham hoped to prove that glass transparencies might be produced at a cost little over that of paper prints. He then described the various processes at present in use for producing them, pointing out the several disadvantages under which each process laboured. Copying in the camera has been used, and is still used by many with various combinations of lenses—some with one, some with a pair of lenses; but,

as far as Mr. Sidebotham has noticed, none of the cameras for the purpose are satisfactory or suited for the wants of amateurs.

Mr. Sidebotham then described the little camera which he had made for the purpose, and which seemed to answer in every way. The principal is to have the negative in front, in an adapting frame to slide right and left; the distance is adjustable by two screws; the frame which holds the plate also slides right and left, and stops in two places with a drop catch, the lens (a short focussed double steresocopic one) being in the middle. The camera is adjustable with screws for focussing or for enlarging or reducing the image. The camera is then tilted towards the sky, the image is focussed, and the picture taken on wet collodion, moving the negative plate for each half of the picture, developing with iron, and, if requisite, deepening or toning with proto-iodide of mercury. One focussing and adjustment of the screws will be sufficient, and any number of negatives can be copied, if taken with the same camera, without any alteration. The negatives require no varnishing, so that all their original beauty is there to be copied, and it is copied faithfully. Prints by this plan may be produced as fast as the plates can be prepared—much faster than on paper if a single negative be used, as the exposure varies trom five to thirty seconds for each side.

Mr. Sidebotham stated that much of the beauty of the pictures he exhibited were doubtless due to the quality of the negatives, and which had been taken by Mr. Buxton, when in India last year. Mr. Sidebotham trusted that the plan here described would induce amateurs to print their stereoscopic pictures on glass instead of paper, and professionals to lower the price of those they place in the market for sale.

PHOTOGRAPHICAL SECTION.

Ordinary Meeting, December 1st, 1864.

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

Mr. Samuel Cottam was elected an Ordinary Member.

Mr. MUDD presented four large and very beautiful photographs to the society's portfolio,—two taken in Dunham Park, and two in North Wales, by the colodio-albumen process. These landscapes were remarkable for their peculiar softness and delicacy.

Mr. PARRY called attention to a letter in "The British Journal of Photography," by Mr. Hislop, claiming the invention of the camera exhibited and explained by Mr. Sidebotham at the last meeting. He also produced the Journal in which was the drawing and description on which Mr. Hislop founded his claim. After an examination of these it was unanimously decided by the members present that there was no similarity whatever between the two cameras, and that a drawing ought to be published of Mr. Sidebotham's camera.

Mr. NEVILL exhibited some good prints produced by making paper sensitive with a solution of salts of nitrate of uranium and nitrate of silver, and when the image began to appear, developing with sulphate of iron; the prints were only exposed fifteen seconds.

Mr. Sidebotham exhibited several specimens taken by himself, by the Wothlytype process, on different kinds of paper. He had experienced considerable difficulty in finding paper to which the collodion would adhere in the washing process. Silver prints from the same negatives were exhibited, and considered superior to those by the Wothlytype process.

Mr. Dancer exhibited on the screen, by the aid of the oxy-hydrogen light, a series of photographic pictures by members and others. The series consisted of transparencies, taken by various processes, and was a trial of their relative merits for exhibition in this manner.

Many beautiful pictures were exhibited printed on albumen, collodio-albumen, Fothergill, tannin, oxymel, and syrup processes, and also taken on collodion in the camera. The two latter processes were decided to be the most suitable; the photographs taken in India by Mr. Buxton were much admired. Several photographs of the moon were exhibited by Mr. Brothers, one of which was taken by himself.

Ordinary Meeting, January 10th, 1865.

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

Dr. Roscoe exhibited some very interesting photographs of the fixed lines in the solar spectrum made by Mr. Rutherford of New York. These photographs exhibit groups of thousands of lines extending from near the line *b* in the green to beyond H in the violet, and serve as a most valuable confirmation of the accuracy of Kirchhoff's maps. Each line in these maps can be easily and distinctly traced in the photograph, whilst many bands drawn as single ones by Kirchhoff are seen in the magnified photograph to consist of bundles of fine lines. These photographs were prepared with three 60° bisulphide of carbon prisms.

Dr. Roscoe also exhibited two fine photographic prints of the moon, enlarged by Mr. Rutherford from negatives taken by him in New York with an $11\frac{1}{4}$in. object glass of 14ft. focal length, which he had ground with special reference to the highly refrangible rays, and which is therefore unfit for ordinary telescopic purposes.

Mr. Baxendell, and Mr. Wilkinson, F.R.A.S., expressed their opinion that Mr. Rutherford's prints were decidedly sharper than any photographs of the moon they had seen.

A Paper was read "On Some Products Derived from Indigo Blue," by Dr. E. SCHUNCK, F.R.S., Vice-President.

By his experiments on the formation of indigo blue, an account of which was laid before the Society several years ago,* the author was led to make some inquiries regarding the processes employed in tropical countries for the production of indigo from plants. All authorities, it appears, agree that the process of fermentation, which is the one usually adopted for the purpose of extracting the colour, requires to be conducted with the greatest care in order to lead to a successful result. Unless certain precautions are adopted the colouring matter may be entirely lost. This phenomenon may be easily accounted for. Though indigo blue, when once formed is a very stable body, the substance existing in the cells of the plant from which it originates and which the author terms *Indican,* is decomposed with the greatest facility, indigo blue being only one of its products of decomposition, which may be formed or not, according to the nature of the process employed.

There are, however, other facts connected with this subject which cannot so easily be explained. It is well known to those dyers who employ the so-called *woad vat,* in which the reduction of the indigo blue is effected by means of various organic matters, such as woad, madder, and bran, together with lime, that if the process be not carefully managed it may change its character entirely—a change which results in the total destruction or disappearance of the colouring matter. This phenomenon cannot be explained in accordance with what is at present known regarding indigo blue, which is considered by chemists to be a body of such a stable character

* See Memoirs, Vol. XIV., p. 181.

as not to be decomposed by any except very potent agents, such as chlorine, bromine, or nitric acid. It has not hitherto been supposed possible to effect its decomposition by means of fermentation or putrefaction.

Then, again, the author found that when very small quantities of indigo blue are reduced according to Fritzsche's method, which consists in acting on it with alcohol, grape sugar, and caustic soda, the colouring matter does not make its appearance again when the solution is exposed to the atmosphere. The liquid yields no deposit and remains yellow and transparent. This fact is also difficult to account for, since it is usually supposed that by the combined action of reducing agents and alkalies indigo blue merely takes up an atom of hydrogen and then dissolves, and by the action of oxygen is again precipitated unchanged and undiminished in quantity. By the continued action of a large excess of alcohol and grape sugar, together with caustic soda, the author succeeded in causing several grammes of indigo blue to disappear entirely. That the effect was due to the combined action of alcohol and grape sugar, not to that of one or the other only, was proved by subjecting a small quantity of indigo blue to the action of grape sugar and caustic alkali in watery solution, and another portion to the action of alcohol, protoxide of tin, and alkali. Reduction of course took place in both cases; but, though the solutions were boiled for some time, the colouring matter was in each case precipitated again on exposure to the air, apparently undiminished in quantity. Since, by the action of caustic alkalies on grape sugar, acetic and formic acids are formed, it occurred to the author that the effect produced by the sugar

in this process might in reality be due to the presence of one or both of these acids rather than to the sugar itself. This supposition was completely verified by experiment. The colouring matter disappeared quite as rapidly when acetate or formiate of soda was employed in the place of grape sugar. The use of the latter was therefore abandoned in the subsequent experiments. In the present communication the author confines himself to an account of the combined action of alcohol, acetate of soda, and caustic soda on indigo blue.

The process adopted was quite simple. Pure indigo blue was introduced into a large quantity of ordinary spirits of wine, and, after being well agitated, the mixture was raised to the boiling point. A quantity of pure acetate of soda, previously deprived of its water of crystallization, and a little caustic soda were then added, and the boiling was continued for several hours. A reduction of the indigo blue took place in the first instance, as was evident from the deep red colour of the liquid. On agitating with air this red colour disappeared for a moment, the indigo blue being precipitated in powder, but after some time the liquid acquired a dark brown colour and deposited nothing on exposure or agitation. The process was then completed. In order to obtain the products formed, the brown liquid was evaporated, and, when the evaporation was nearly completed, water and an excess of sulphuric acid were added, which threw down a brown insoluble mass, consisting partly of resinous, partly of pulverulent substances. From the liquid, which was of a light brown colour, a crystallized acid was obtained, which after being purified was found to consist of anthranilic

acid. From the mass, insoluble in water, the author obtained five distinct substances, which were separated from one another by means of various solvents, such as alcohol, ether, ammonia, and carbonate of ammonia. These substances were all brown and amorphous. Some of them resembled resins, others were powders. In general they were found to possess very few characteristic properties, and as they presented very little that could be of interest to the chemist, if their origin and their mode of formation be excepted, the author refrained from bestowing names on them and thus adding to the already unwieldy mass of terms with which chemical science has to deal, but preferred to distinguish them by the letters of the alphabet, as A, B, C, D, and E. The body A is easily soluble in cold alcohol and ether, but quite insoluble in alkalies. B is easily soluble in alcohol and ether, as well as in alkalies, both caustic and carbonated. These two have the appearance of resins of a rich brownish-yellow colour. C is very little soluble in alcohol and ether, and insoluble in alkalies. D closely resembles C, but is distinguishd by its solubility in alkalies. E is remarkable for being soluble in a boiling solution of acetate of soda. These three are brown powders. That portion of the mass soluble in alcohol and alkalies, but insoluble in ether, was not examined, as it was sure to contain some of the peculiar resinous product of decomposition, which is always formed by the action of caustic alkalies on alcohol, and which is supposed to be identical with the so-called "aldehyde resin."

The author's analyses of these five bodies led to the following formulæ :—

A.........................$C_{62}H_{39}NO_8$

B.........................$C_{52}H_{35}NO_8$

C.........................$C_{28}H_{11}NO_4$

D.........................$C_{56}H_{24}N_2O_{10}$

E.........................$C_{23}H_{11}NO_6$

The author attaches no importance to these formulæ except in so far as they furnish a means of explaining the mode in which these bodies are formed. It will be seen that they all contain the elements of indigo blue, alcohol and acetic acid in various proportions. Taking as an instance the body C, which is the simplest in constitution, it is apparent that it has been formed by the union of 1 atom of indigo blue, 1 atom of alcohol, and 2 atoms of acetic acid, 8 atoms of water being at the same time eliminated, since

$$C_{23}H_{11}NO_4 + 8HO = C_{16}H_5NO_2 + C_4H_6O_2 + 2C_4H_4O_4.$$

In like manner E originates from the combination of 2 atoms of indigo blue, 1 atom of alcohol, and 5 atoms of alcohol, for

$$2(C_{23}H_{11}NO_6) + 14HO = 2(C_{16}H_5NO_2) + C_4H_6O_2 + 5C_4H_4O_4.$$

The formation of B will be easily understood by a glance at the following equation :

$$C_{52}H_{35}NO_8 + 18HO = C_{16}H_5NO_2 + 6(C_4H_6O_2) + 3(C_4H_4O_4).$$

In the case of A, which is the most complex of all, it is necessary to assume that carbonic acid comes into play, since

$$C_{62}H_{39}NO_8 + 26HO = C_{16}H_5NO_2 + 8(C_4H_6O_2) + 3(C_4H_4O_4) + 2CO_2.$$

It is difficult to say whence this carbonic acid is derived,

but the author supposes it may originate in the decomposition of that portion of the indigo blue which yields anthranilic acid.

Hence it appears that all these products, except anthranilic acid, are formed by a very simple process which consists merely in indigo blue taking up alcohol and acetic acid in various proportions and forming compound bodies in which none of the constituents, as such, can be detected. It is, therefore, not a process of decomposition, but a synthetical process, a building up of complex bodies from others of a simpler constitution. This is proved by the fact of water being given up during the process, whereas in all cases in which complex organic substances are decomposed into simpler ones, water is absorbed. Regarding the real constitution of these bodies, the author hazards no speculations. It might be supposed that they belonged to the class of conjugated compounds, of which organic chemistry furnishes us with so many examples, and that by decomposition we should be able to obtain from them some of the simpler bodies which are known to have entered into their composition, but the author's experiments, as far as they have gone, do not countenance this view. He was unable to obtain from any one of them, either indigo blue, alcohol, or acetic acid.

The occasional disappearance of the indigo blue in the woad vat in consequence of mismanagement now admits, the author thinks, of any easy explanation. By the fermentation of the sugar contained in the madder employed, alcohol is formed, which in its turn may yield some acetic acid and

alcohol, acetic acid and a base (lime) being present, nothing further is required for the development of the process described by the author.

Professor ROSCOE suggested that some of the bodies described by the author might possibly be represented as substitution products, one or more of the atoms of hydrogen of the indigo blue being replaced by one or more organic radicles.

A Paper was also read "On Some Physiological Effects of Carbonic Acid and Ventilation," Part I., by Dr. R. ANGUS SMITH, F.R.S., President.

Ordinary Meeting, January 24th, 1865.

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

Mr. William B. Johnson was elected an Ordinary Member of the Society.

Mr. S. B. Worthington, C.E., stated that it might be of some interest to the engineering members of the society to know, that he had lately constructed a swing bridge for carrying a railway over the Sankey canal, in which the girders are made of Bessemer steel plates. The object of using steel instead of wrought iron, was to reduce the weight of the girders. The girders are four in number, about fifty-six feet long, with bearings varying from thirty to forty feet, and two feet deep. They were manufactured by Messrs. Benjamin Hick and Sons, of Bolton, from steel plates made by the Bolton Steel and Iron Company; and were tested with loads of a ton to the foot, or more than double the weight which they could possibly be called upon to bear. The deflection varied from $\frac{1}{2}$-inch to an inch, according to the length of the girder, and there was no permanent set on removal of the testing load.

The plates used varied from $\frac{1}{4}$-inch to $\frac{7}{16}$-inch in thickness; and the average tensile strength of a considerable number of plates tested, was upwards of thirty-six tons to a square inch.

The weight of the girders was about $\frac{5}{8}$ths of the weight which they would have been if wrought iron had been used.

The contract for this bridge was made in November, 1863, and the bridge was erected during the past Summer.

Mr. Worthington also exhibited a piece of cast iron lately taken out of the Sankey canal. Its exterior, from one-eighth to a quarter of an inch in depth, was so soft as to be easily cut with a dull knife From the form of the casting he thought it very probable that it had not been in the canal more than five or six years. He stated that the water of the canal was strongly impregnated with liquids discharged from the alkali works in the neighbourhood of St. Helens.

Mr. Sidebotham read the following communication from Mr. James Nasmyth, C.E., Corresponding Member of the Society, and exhibited the large and beautiful drawing to which it refers :—

On the 5th of June, 1864, between the hours of 11 a.m. and 2 p.m., I had the good fortune to observe under unusually favourable circumstances, a large and remarkable group of solar spots.

The willow-leaf shaped objects forming the structural element of the entire photosphere, as also forming the details of the penumbral portions and bright margins of the photosphere overhanging the spots, were occasionally revealed with perfect distinctness; but the favorable moments for pure definition had to be watched for, by keeping the eye steady at the eye piece and the hand on the focal adjustment, so as to be ready to make those minute focal adjustments that I find are requisite to meet the constantly varying condition of the atmosphere, which, especially during bright sunshine, is so fertile a source of defective definition, as is well known to practical observers.

Some of the willow-leaf shaped objects were very favourably situated in an insulated position over the dark centres of the spots, and in that situation yielded me excellent opportunity for carefully noting their exact form and proportions. In

this respect I can with the utmost confidence pledge myself to the correctness of what I have represented in the drawing which accompanies this communication.

I continue to employ the term "willow-leaf" shaped objects, as sufficiently exact to convey to any one a general idea of their form, and so to identify them. I might perhaps have hit upon some other term that would have been more exact and descriptive, and in that respect I was much pleased with that which was employed by Mr. Stone, of the Royal Observatory, Greenwich, when he first beheld these remarkable objects by the aid of the great equatorial refracting telescope of the Royal Observatory, on which occasion Mr. Stone described them as "bright rice-like particles." Perhaps the more general term "lenticular shaped objects" would be better than either, as admitting of a certain latitude in respect to proportions, that may admit of a classification of any variety in these respects that future observations may supply.

Any observer, who with due means at his command is fortunate to obtain a satisfactory view of these truly-remarkable structural details of the solar photosphere, can, of course, please himself as to the term that appears to him best to convey a correct idea of them. But the grand fact of their existence is now proved beyond all doubt; and they, as "a great fact," will ever remain so long as the sun exists.

Dr. R. Angus Smith, F.R.S., read a paper "On some Physiological Effects of Carbonic Acid and Ventilation," of which the following is the substance.

That bad ventilation produces effects which are unpleasant, unwholesome, dangerous, or deadly, according to circumstances, has been long known; it has also been well known that the effects of breathing carbonic acid are of a similar kind. We have not, however, been able to say distinctly that the evil effects of bad ventilation are due entirely to carbonic acid. We have had, and not without

good reason, a strong belief that the organic matter was a grave offender.

The author had endeavoured to show elsewhere that the organic matter is in early stages of ventilation the most observable; but that in later stages the effect of the carbonic acid is unmistakeable.

When enquiring into the state of the air in mines he found it needful to make experiments in close places, and had a leaden airtight room built, containing about 170 cubic feet of air. On examining the effect of carbonic acid on the burning of candles, he remained in the chamber until that gas was poured in to the extent of 3·9 per cent. He then found that the pulse fell so low that it was difficult to count the beats, whilst they diminished in number. This effect was rapid, as he was not long in the room. Since there was no time for the accumulation of organic matter, nearly the whole effect must have been due to carbonic acid. Similar results were observed frequently: a few may be given. The first here adduced will show the fall of the pulse and the increase of the respiration more clearly than the others as the details are appended. The carbonic acid increased by means of respiration only. The number of beats diminishes with a regularity equal to the increase of the carbonic acid, whilst the breathing quickens with equal steadiness.

One Person in the Lead Chamber.

Respiration and beats of the pulse taken every ten minutes.

Time.	Pulse.	Respiration.	Temperature, Celsius.	Carbonic acid in the same periods.
h. m. 10 55	73	15·5	18°·2	0·04
min. After 10	73	16	18·2	0·114
„ 20	72	16	18·2	0·187
„ 30	71	17	18·4	0·261
„ 40	71	16	18·4	0·335
„ 50	70	16	18·5	0·408
„ 60	68	16	18·6	0·482
„ 70	67	16·5	18·7	0·556
„ 80	67	17	18·8	0·629
„ 90	66	17	18·9	0·703
„ 100	65	18·	19·0	0·777
„ 110	65	18·5	19·0	0·850
„ 120	64	19	19·0	0·924
„ 130	63	19	19·2	0·997
„ 140	62	19·5	19·1	1·071
„ 150	62	20	19·1	1·145
„ 160	62	20	19·1	1·218
„ 170	61	20	19·1	1·292
„ 180	60	21	19·1	1·366
„ 190	60	22	19·2	1·439
„ 200	59	23	19·2	1·513
„ 210	58	24	19·4	1·587
„ 220	57	24	19·4	1·661
„ 230	57	24	19·4	1·734

Five persons sat for 80 minutes in the room. In all cases there was great irregularity of breathing. They were all conversing with each other, which occupation, as was found, somewhat modified the effect. In two cases the rise of the pulse was considerable, viz., from 60 to 79, and from 84 to 91; but the numbers soon fell down to the natural amount, and would apparently have fallen much lower if one of the two persons had not felt too unwell to remain. The pulses in all cases were difficult to count, being excessively feeble, and the most delicate of the fingers was sought for the operation. No deficiency of strength was found in any of the trials, and the amount of carbonic acid rose equally from first to last.

As the pulse fell and rose according to the individuality, the

author fixed on one young man on whom the effect was most regular for further experiment. It must be remembered, however, that on no person was the effect small or uncertain.

The objects of the farther experiments were these, 1st, to inquire if the influence could be observed when the amount of carbonic acid was small; and 2nd, to separate the effects of the carbonic acid entirely from those of organic matter.

With 3 per cent of carbonic acid evolved in the chamber itself, the pulse fell in 27 minutes from 67 to 62, the breathing rose from 17 to 23; the pulse so low that it was barely perceptible. The exposure was not full 27 minutes, as the gas took some time to evolve.

With 2 per cent the pulse fell in 70 minutes 4, the breathing rose from 18 to 23½. On coming out the pulse rose 8 in five minutes.

With 1 per cent the pulse fell 4 in the hour.

The following results were obtained by breathing air with carbonic acid entirely free from organic matter, the inspirations being taken from a prepared reservoir, and the expirations not being allowed to mix with them:—

With 1 per cent CO_2 a rise of 2, then a fall of 5 beats of the pulse in 26 minutes.

" 0·5 pulse fell 5 in 40 minutes, respiration rose 7.

" 0·25 carbonic acid pulse rose 3, and fell 4 in 30 minutes, respiration rose 4.

" 0·1 carbonic acid rose 1 and fell 1, in 45 minutes, breathing rose 1.

Ordinary air was breathed in the same way, so as to eliminate the effect of the apparatus. Pulse rose 1 and fell 1, but no greater change occurred during a whole hour. Breathing continued unchanged except at one interval, when it fell 1 and then resumed its usual number. In no experiment during the whole period did the breathing of the same experimenter ever fall 1 when there was as much as one tenth per cent of carbonic acid present.

In one person, a youth, the pulse rose on every trial. On entering into air with 3 per cent of carbonic acid and no organic matter, his pulse rose 6 in two minutes and his breathing fell 4. The pulse was so feeble that he could not count it; some one helped in the process. He found the air very unpleasant. Another young man in two minutes in the same air, found that his pulse rose 6 and his respirations 4.

The action of carbonic acid seems, therefore, in all cases to enfeeble the pulse; at first sometimes to cause a rise, but finally to lower the number of the beats.

This effect is instantaneous or nearly so with air having 3 per cent of carbonic acid, but diminishes with the amount of impurity. It is, however, perceptible with an amount of carbonic acid as low as 0·1 per cent, and probably by taking long periods the effect would be found even with smaller quantities. This amount is often exceeded in private houses and public meetings, where it rises to 0·2 or even 0·3.

The second effect of carbonic acid is in the breathing, which it hastens rapidly, although in some cases it causes a diminution of the inspirations. The effect approaches either a gasping or a panting.

The author added that he must leave to physiologists to speak of the ultimate effect of such a condition of things, and would only observe, that in Dr. Peacock's inquiries into the state of health of the Cornish miners, he found that a feeble pulse was one of the peculiarities, a proof that the temporary results found in these experiments may be rendered permanent.

On coming into fresh air the pulse and breathing recovered in a few minutes, showing the value of ventilation.

PHYSICAL AND MATHEMATICAL SECTION.

January 12th, 1865.

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

Mr. HEELIS, F.R.A.S., communicated the following account of a fire ball which he had observed:—

Tuesday, 13th December, 1864, 0 hour 7′ a.m. Fire ball with sensible disc, and brilliant train from middle of seat of Cassiopeia's chair, to a point half-way between *α* Andromedæ and *α* Cygni. Disappeared at an altitude a little greater than that of the latter star (in other words about 10° above horizon); duration about 2″. Train extended from point of appearance to that of disappearance, but disappeared almost immediately after the meteor itself. Color, bluish white, with sparks from head, especially when near the point of disappearance. Did not see it burst. Light intense, and well defined disc. It was nearly calm on the earth at the time, but heavy scud was going rapidly over the moon from the S.E.; and to the best of my judgment the meteor fell directly before the scud. Laying off the course, &c., on a celestial globe, the results are—altitude of point of first observation or origin about 50°; altitude of disappearance 10°; length of track 40°; direction N.W.

There was so much scud that the meteor must have passed behind several clouds, and so much moonlight that the time by watch could be read. Hence the disc could hardly have been the effect of irradiation.

9 a.m. Wind S.E.; light, clear weather.

The only point upon which I have any doubt is the time, which might have been a little later. I took the time by my watch, but it had been wrong for a day or two, and I could not get to any good clock to test it for some days after.

Mr. Vernon, F.R A.S., communicated the following "Note on the Rainfall of 1864."

The fall of rain for the past year has been 4·758 inches below the average of the last 71 years. The fall was also 7·700 inches below that of 1863, and fell upon 44 less days than in that year.

The principal deficiency occurred during the last six months of the year, especially in July and October. The only month having a fall in excess out of the last six months of the year was September, and rain fell upon more days that month than any month during the year.

The falling off in July and October was no doubt owing to the excessive amount of easterly winds. In July easterly winds occurred upon 11 days, whereas the average of 13 years gives only 5·9. In October easterly winds prevailed on no less than 18 days, the average being only 9·5.

On examining the monthly means for the 71 years, there appears to be evidence of periodicity in the distribution of the rainfall: a minimum occurring in April just after the vernal equinox, and a maximum in October just after the autumnal equinox. In September, however, there appears to be an exception to the gradually increasing values from April to October.

If this law really exists as it appears to do, the rainfall will evidently bear a close relation to the sun's course in the ecliptic, the minimum rainfall occurring at a time when the gradually increasing temperature of the air enables it to absorb a larger amount of moisture, and consequently tending to prevent precipitation. The maximum rainfall also occurring at the time, the temperature begins to fall rapidly, and

consequently tending to precipitate the large amount of moisture absorbed during the heat of summer.

RAINFALL, 1864.

Old Trafford, Manchester.

By G. V. Vernon, F.R.A.S., M.B.M.S.

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

Quarterly Periods. 1863. Days.	Quarterly Periods. 1864. Days.	1864.	Fall in Inches.	Average of 71 Years. Inches.	Difference.	Number of days Rain fell in 1864.	Quarterly Periods. 1864. Inches.	Quarterly Periods. 1863. Inches.
		January.....	1·684	2·460	—0·776	12		
45	38	February ...	4·027	2·379	+1·648	13	7·722	6·170
		March.......	2·011	2·310	—0·299	13		
		April	1·602	2·038	—0·436	9		
53	39	May.........	3·175	2·351	+0·824	10	7·722	7·746
		June........	2·945	2·913	+0·032	20		
		July.........	1·687	3·569	—1·882	9		
56	45	August	2·367	3·592	—1·225	15	8·063	12·216
		September..	4·009	3·235	+0·774	21		
		October.....	1·908	3·818	—1·910	13		
61	49	November ..	3·255	3·473	—0·218	19	7·133	12·208
		December...	1·970	3·260	—1·290	17		
215	171		30·640	35·398	—4·758	171	30·640	38·340

Ordinary Meeting, February 7th, 1865.

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

Among the donations announced was a framed photographic portrait of the late Thomas Hopkins, Esq, formerly a Vice-President of the Society, presented by James Mooney, Esq.

On the motion of Dr. Joule, seconded by Mr. Robert Worthington, the thanks of the Society were unanimously voted to Mr. Mooney for his valuable donation.

Mr. Binney, F.R.S.: After the researches of Lindley, Geoppert, Brongniart, Prestwich, Hooker, and others, it really seemed that we had obtained almost a complete knowledge of the internal structure of *Stigmaria*. It is true that only Geoppert had seen the isolated bundles in the pith, all the specimens of the other observers having been imperfect in that portion of the plant, and giving no indication of structure there. In my own researches I have rarely met with a *Stigmaria* shewing any structure in the central axis, even where the small stems of *Sigillaria vascularis* displaying all the structure in that part are found in great abundance.

Many years since, after the examination of a great number of specimens of *Stigmaria* in my collection, it occurred to me that an outer as well as an inner radiating cylinder would be discovered. In my remarks on *Sigillaria*, published in

Vol. IV., part 1, p. 20, of the *Quarterly Journal of Geology*, is the following passage: "That part of *Stigmaria* which intervened between the vascular axis and the bark, appears to have consisted of two kinds of tissue. These have, in most cases, been unfortunately destroyed, so that we cannot positively know their true nature; but they appear to have been of different characters, for there generally appears to be a well marked division. This is often shown in specimens composed of clay ironstone which have not been flattened, and the boundary line is about quarter an inch from the outside of the specimen. Most probably the outer zone has been composed of stronger tissue than the inner one, as is the case with well preserved specimens of *Lepidodendron*." It is singular that such acute observers as those above named had not noticed this line of division, but it was no doubt owing to the imperfect specimens which they had examined. After the discovery of the outer radiating cylinder in *Lepidodendron* by Witham, and the same arrangement in *Sigillari elegans* by Brongniart, it was to be expected that such outer radiating cylinder would be found to exist in *Stigmaria*, if it were the root of *Sigillaria*. After an examination of a great number of specimens, the cabinet of Mr. James Russell, of Chapel Hall, Airdrie, has afforded me four or five different *Stigmaria*, which give clear evidence of the existence of this outer radiating cylinder. They are all in clay ironstone, and have not been much compressed. He has kindly allowed me to slice two of the specimens, which afford decisive evidence of the existence of both an inner and an outer radiating cylinder. The space on the outside of the inner cylinder does not shew distinctly the bundles of vessels communicating with the rootlets, although there is some evidence of their former occurrence. The bell shaped orifices from which the rootlets sprang are well displayed, and the space between them is occupied by wedge shaped masses of elongated tubes or utricles arranged in radiating series, not to be distinguished

in any way from those which I have shewn occur in *Sigillaria vascularis* (*Quarterly Journal of the Geological Society* for May, 1862, Plate 1, fig. 6). Indeed, the transverse section of the specimen there figured would almost do for a representation of *Stigmaria*, if the latter had the central axis preserved, which it unfortunately has not. There is the same internal radiating cylinder and the same space formerly occupied by lax cellular tissue, which gradually passes into the elongated tubes or utricles arranged in radiating series, and parted by large bundles of vessels next to the bark; thus clearly proving from structure alone that *Stigmaria* is the root of *Sigillaria*, each of them having an inner radiating cylinder composed of barred vessels, a space occupied by lax cellular tissue, and an outer radiating cylinder composed of elongated tubes or utricles.

Mr. Binney also exhibited some of the specimens to the meeting, and said that he was in possession of clear evidence both from structure and branching to prove that *Sigillaria*, which was the chief plant from which our beds of coal had been formed, was a tree which branched like the *Lepidodendron*, and was not the strange stunted-topped plant which some authors had represented it to be.

The President drew attention to the late fatal explosion at Peterborough, and asked whether the easy method of testing steam boilers, described some years ago by Dr. Joule, was forgotten or found to be impracticable?

Dr. Joule said that he had taken pains to give his method, by which the testing by hydraulic pressure could be applied with the utmost facility by simply filling the boiler with water and then raising its temperature a few degrees, a very extended publication. He believed that the objection raised by some to its use, was the absurd one that hydraulic pressure injured the boiler. The very object of a test was to detect weak boilers for the purpose of strengthening or rejecting

them. He was at a loss for terms strong enough to express his opinion of the reckless disregard of life, or the ignorance which resulted in the deplorable catastrophes which were constantly occurring; and he believed that the only method of cure would be that proposed by Mr. Binney in the case of the explosion of firedamp in mines, namely, that the parties to blame should be compelled to support the widows and orphans of their victims.

Mr. Alderman Pochin stated that he had made use of Dr. Joule's plan, and found it quite practicable and easy of application.

A paper was read "On a New Re-agent for the Separation of Calcium from Magnesium," by Edward Sonstadt, Esq.

When, in the ordinary course of qualitative analysis, carbonate of ammonium is used to separate calcium from magnesium, unless the former is present in notable proportion to the latter, a very insoluble double carbonate of magnesium and ammonium always accompanies the carbonate of calcium, if this is allowed sufficient time to form. If much magnesium and *no* calcium is present, the magnesium precipitate still falls after a while. Both metals are precipitated by this re-agent, the only difference being that the calcium precipitate forms somewhat earlier than the magnesium precipitate. This fact is cursorily mentioned by Fresenius, more fully by Gmelin, and has recently been made the subject of special notice by Dr. Dyer. Calcium, therefore, can only be separated from magnesium by this re-agent, by fractional precipitation, which necessarily involves loss of substance; and, in qualitative examination, the method is sure to mislead when the proportion of calcium present is small, unless it is controlled by other methods. The same remarks apply in substance to the two other methods of precipitation by sulphuric acid and alcohol, and by oxalate of ammonium. When a moderately strong solution of Epsom salts is treated with sulphuric acid

and alcohol, the solution is mostly converted into a crystalline magma; and if it is desired to separate a small proportion of calcium, which we will suppose to be present, the magma must be filtered, dissolved, and subjected to the treatment again and again to separate the sulphate of calcium, when, if the quantity of that salt present be very minute, it must be wholly lost. Of course these remarks do not apply to solutions of calcium and magnesium salts containing much of the former, except in a modified degree. What is true of the sulphuric acid and alcohol process, is true in a more extended sense of the oxalate of ammonium process. I have precipitated within a trace the whole of the magnesium present in a considerable quantity of solution of chloride of magnesium, simply by successive additions of oxalate of ammonium,—the solution being concentrated to its original bulk after the last addition of the re-agent. Yet, in working with this re-agent the rule is, that enough of it must always be added to transform all the magnesium salt into oxalate, since oxalate of calcium is soluble in solution of chloride of magnesium. That some magnesium salt must precipitate with the lime salt under such conditions is obvious; and that it does so is well known, and is, though incompletely, provided for by the process being directed to be repeated upon the precipitate first obtained. This process, therefore, is also one of fractional precipitation, and for it to approach success, the operator must know pretty nearly beforehand how much calcium, in proportion to the magnesium present, he has to deal with. Nevertheless, it is unquestionable that in skilled hands, either of the two last processes is capable of giving close approximations to the truth, when the quantity of calcium present amounts to a few per cent of the mixed salts. When the quantity of calcium is less than 1 per cent, I do not think it is possible to estimate it accurately by any of these processes; and when the proportion is larger, the processes are at least more troublesome, have a wider limit of experimental

error, and are more apt to fail in less experienced hands, than the analytical processes in use for estimating most of the other commonly occurring elements.

In common tungstate of sodium we possess a test for calcium which is probably equal in delicacy and in certainty to that of chlorine for silver, or of sulphuric acid for barium.

The action of this test, in a preliminary examination, requires to be ascertained—

(1.) With calcium solutions alone.

(2.) The presence of magnesium.

(3.) The presence of magnesium and ammonium salts, and of these with free ammonia.

(1.) *The behaviour of tungstate of sodium with solutions of calcium salts.* A saturated solution of sulphate of calcium, taken at 13° C. remains perfectly clear on addition of an equal volume of a saturated solution of tungstate of soda for a short time. On warming, when the solution attains the temperature of 42° C, it becomes turbid, deposits a film upon the containing glass vessel, and soon after a dense precipitate falls. To ascertain the limit of the action of the test, the solution of sulphate of calcium was successively diluted to various degrees, and precipitates obtained, until the solution was so dilute that it contained but one part of sulphate of calcium in 114,000 parts water. A few drops of solution of tungstate of sodium were added, the solution warmed, and at 56° C, the solution became distinctly opalescent. An experiment was then made on the distilled water used for the dilution, but it gave no reaction. It was evident that it was possible to push the attenuation much further, and yet get indications of calcium. But this proportion ($\frac{1}{114000}$) is near the limit at which sulphate of calcium may be rendered *distinctly* visible. A solution of chloride of calcium behaves similarly. Sulphate of magnesium is not precipitated by tungstate of sodium, unless the solutions of the two salts are strong. The experiments were made with a solution of pure sulphate of magnesium, of specific gravity

1·114, and containing 11·283 per cent of the anhydrous salt. The solution of tungstate of sodium was saturated (at common temperature), and contained about one-third its weight of dry salt. A mixture of equal parts of these solutions gave no precipitate in the cold, but quickly crystallized when warmed, the crystals being difficultly soluble, and leaving a very slight residue of an insoluble variety of tungstic acid, or of some compound of that acid. But when the mixed solutions above described were very little diluted, the solution remained perfectly clear at any temperature, until the fluid was concentrated by evaporation, when no precipitate, but clear crystals appeared. It is only, therefore, in very concentrated solutions that tungstate of sodium gives—not then a precipitate—but crystals, with sulphate of magnesium. The chloride of magnesium solution behaves similarly, though it was not so closely examined.

(2.) *The behaviour of tungstate of sodium with solutions containing calcium and magnesium.* The earlier experiments seemed to indicate that the presence of magnesium did not at all interfere with the precipitation of the calcium. But on continually diminishing the quantity of the calcium salt while that of the magnesium salt was kept constant, it was found that the latter exercised a very appreciable solvent power. The limiting experiment was as follows:—To 5 cc. of a solution containing 7 parts in 100,000 of sulphate of calcium, were added 3 cc. solution of sulphate of magnesium, containing 11·283 per cent anhydrous salt, 12 cc. water, and a few drops of tungstate of sodium. There were thus, in 2,000,000 parts of fluid, 35 parts sulphate of calcium, and 33,849 parts sulphate of magnesium—the remainder being water, except the small quantity of tungstate of sodium. The reaction was not visible till the fluid reached the temperature of 70° C, when it became apparent, and, on putting it aside to cool, a perfectly distinct film formed on the glass. A similarly attenuated solution of the lime salt, but containing no magnesium,

was exposed to the same conditions with the re-agent, and the reaction in the latter case occurred earlier, at a lower temperature, and was more distinct. Nevertheless, the fact remains that, in a mixed solution of the sulphates of calcium and magnesium, the presence of the former may be clearly detected up to the proportion of about 1 part in 56,000 of fluid containing about 1,000 parts of magnesium salt. Rougher experiments made with the corresponding chlorides led to similar results.

(3.) The influence of ammonium salts in obstructing the precipitation of calcium in presence of magnesium is very marked. A calcium salt, in presence of a very large proportion of both magnesium and ammonium salts, cannot be certainly recognised except somewhere near $\frac{1}{4000}$th of the calcium salt be present in solution. The influence of free ammonia with sulphate of ammonium and sulphate of magnesium, in like large proportions, is so great as to only just admit of the recognition of the calcium when from $\frac{1}{600}$th to $\frac{1}{1000}$th is present. Nevertheless, enough, and rather more than enough, ammoniacal salt may be present to prevent any precipitation of magnesium by excess of ammonia, and a moderate excess of ammonia may also be present, without sensibly affecting the estimation of the lime in a quantitative experiment. Chloride of ammonium does not dissolve the precipitate when it is once formed.

The analytical experiments on weighed mixtures of calcium and magnesium salts, imperatively necessary in introducing a new re-agent, are not yet completed, most of the experiments of this kind made till now, having been vitiated through ignorance of the conditions necessary to ensure success. I give, however, the results of one experiment, the conditions of which approached more nearly to those I now know of as being necessary than the others, reserving the series, together with the methods adopted for obtaining pure materials to work with, for a second Paper.

	Taken.	Found.
Magnesia	0·3097 grms.	0·3120
Carbonate of calcium...	0·0043	0·0042

The weighed quantities of carbonate of calcium and of magnesium were dissolved in a slight excess of hydrochloric acid; neutralised carefully by ammonia, precipitated by tungstate of sodium, and then the filtrate, with the usual precautions, by common phosphate of sodium. The excess in the weight of pyro-phosphate of magnesium, led to the suspicion that some tungstic acid had been carried down; a suspicion amply confirmed by the colouration obtained from the solution of the ignited precipitate in dilute hydrochloric acid when treated with tin.

A little in anticipation of my intended future paper upon the subject, I now add such details respecting the manipulation required in separating lime from magnesia by tungstate of sodium as my experience has shown to be necessary. It is convenient to have the solution of the magnesium and calcium salts made somewhat alkaline by ammonia, but a very large quantity of this, as well as of ammoniacal salt, is, as we have seen, to be avoided. The beaker in which the precipitation is to be effected should, while perfectly dry and warm, be rubbed within by chamois leather on which a drop or two of fine oil (such as is used for oiling balances) has been put. If this precaution be not taken, it will be found impossible to detach the precipitate of tungstate of calcium from the sides and bottom of the vessel. A considerable excess of the re-agent is not necessary; but, if it occur, is not material. If, on addition of the re-agent, a white, floculent precipitate forms immediately, it is well to add a few drops of ammonia, when the floculent precipitate will re-dissolve, but if it does not re-dissolve, after warming, there is some other element present, which, if ordinary Epsom salts are used will probably be manganese. The tungstate of calcium precipitate is very dense; it forms slowly in very dilute solutions, and, in all

cases, several hours should be allowed for it to form. The solution should be warmed meanwhile, but must not be allowed to boil. The precipitate must be washed till the filtrate shows no cloudiness on standing, with nitrate of silver when the salts are chlorides; or, if they are sulphates, till chloride of barium gives no cloudiness. The precipitate must then be further washed with dilute solution of ammonia, but these washings need not be saved. The filter should be burnt separately, after the precipitate is cleared from it as nearly as possible. After the ignited precipitate is weighed, a little strong solution of ammonia should be poured upon it, and allowed to stand for awhile, when the ammonia is decanted, and supersaturated with acid. If a precipitate falls after a time, the tungstate of calcium precipitate should (without being removed from the crucible) be allowed to stand for some hours with more ammonia—it is then washed by decantation, again ignited, and weighed. The ignited precipitate should be perfectly white.

The filtrate, containing the magnesium salt and tungstate of sodium, may be at once precipitated by phosphate of sodium in the usual way, but if this is done, much washing is required to get rid of the little tungstic acid that adheres obstinately to the precipitate. It is better, especially when a great excess of the re-agent has been used, to first precipitate the tungstic acid, by a considerable excess of hydrochloric acid, and boil until the precipitate becomes dense and intensely yellow. The solution is then filtered, supersaturated with ammonia, and the magnesia precipitated in the usual way; but, even in this case, it is better to wash lastly with stronger ammonia solution than ordinary.

MICROSCOPICAL SECTION.

January 16th, 1865.

J. SIDEBOTHAM, ESQ., President of the Section, in the Chair.

Exhibitions and Presentations.

A box of two dozen slides of botanical specimens, beautifully mounted, presented to the Section by Mr. J. E. Whalley.

Specimens of carbonate of magnesia from Greece; the surfaces showing very delicate dendritic markings of oxide of manganese, well seen under the microscope.—Mr. A. Brothers.

A fine specimen of Polyporus versicolor.—Mr. Grindon.

A set of mounted specimens of leaves from the south of Europe and the Rocky Mountains, showing curious and novel markings.—Mr. W. H. Heys.

Communications.

The following note from Mr. Dancer, addressed to the President of the Section, was read:—

"SIR,—I beg to state that, since our last meeting, I have carefully examined, with various powers of the microscope, the cotton hairs whilst undergoing dissolution in Schweizer's

Ammoniacal Solution of Copper. I am inclined to the belief that cotton hairs do not contain spiral vessels properly so-called. I think that the spiral apparatus, which has been described by Mr. C. O'Neill and Mr. Heys as spiral vessels, can be clearly traced to a mechanical action which the solvent exerts on the vegetable cell. At some future time I hope to illustrate this to the Section.—Yours truly,

"J. B. Dancer."

Mr. Heys explained that he had not intended to describe the cotton hairs as containing spiral vessels, in the botanical sense of that term, but had spoken of the appearance within them as that of a spiral thread.

Mr. Watson read a communication "On the Plumules or Battledore Scales of the Lycænidæ," in which he showed that they will serve the purposes of identification by exhibiting generic and specific alliances, and differences similar to those found in the plumules of the Pieridæ, described by him in a previous paper. Fifty-three figures of the plumules, drawn by Mr. Sidebotham, were shown as illustrations of the subject. He said the points he desired to insist upon as likely to be useful in this investigation were—That the plumules are always identical in individuals of the same species, and mere varieties can therefore be detected by this test; and that, in very closely-allied species which are difficult of distinction by the more ordinary characters, these scales will often be found to be different.

Mr. Sidebotham read "Notes on the Development of the Wings of Lepidopterous Insects." He said that their great and rapid increase of size soon after the insect emerges from the chrysalis is caused by air, taken in through the spiracles, being sent into the vessels of the wings; the membrane is expanded in consequence, and the scales, which were before

packed under each other as closely as possible, are made to slide out until they remain in the fully developed wing like the tiles of a roof. He exhibited preserved specimens of the Currant moth and the Tiger moth, with the wings both in their small and in their expanded state, also a coloured sketch of one of them, and it was seen that in the unexpanded state the wings lie flat without any folding, and all their markings are a correct representation in miniature of what they ultimately become.

January 30th, 1865.

J. SIDEBOTHAM, ESQ., President of the Section, in the Chair.

Exhibitions and Presentations.

Two powers for the Microscope ($\frac{1}{2}$in. and $\frac{1}{4}$in.), made by Mr. Dancer, presented to the Section by the Right Rev. the Lord Bishop of Manchester.

Remains of an Ichthyosaurus.—T. Alcock. The points noticed were the bones situated in the triangular space enclosed by the lower jaw, which were well shown as the creature lay on its back; a broken rib reunited during life; the numerous small bands of bone, three or four corresponding with each rib, encircling the body in front; remains of skin in various parts; and, in the situation of the stomach, a layer of minute teeth and fragments of scales, the remains of food taken during life.

Mountain Limestone fossils from Clitheroe. A new Rhynchonella not yet specifically named; many beautiful specimens of Crania; and a set of extreme varieties of Orthis resupinata.—Mr. Parker.

Double feathers from pheasant.—Mr. Latham.

A set of the photographs of Dinornis in the York Museum. Dried specimens of the male flowers of Aucuba.—Mr. Grindon.

Communications.

Dr. Alcock read "Notes on a Visit to Walton Hall," with remarks on Mr. Waterton's method of preserving animals.

Dr. Carrington read communications "On an Annelidan Larva," and "On the Embryology of Annelids."

Ordinary Meeting, February 21st, 1865.

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

Mr. Thomas Worthington was elected an Ordinary Member of the Society.

M. Breguet, of Paris, exhibited and explained the construction of Dumas' lamp for use in coal mines, the principle of which consists in the employment of the light from a Geissler's vacuum tube, excited by a small Ruhmkorff's induction coil.

Professor Roscoe stated that he had very frequently been asked for information respecting the mode of preparing the sealed bulbs, containing exactly equal volumes of chlorine and hydrogen gases, which he employed for exhibiting the chemical combination of these gases effected by the action of light, and as the successful preparation of these bulbs depends upon exactly observing certain minute conditions, he ventured to submit the following particulars to the Society. The apparatus needed, consists of a stout tube or narrow bottle of about 120 cubic centimetres capacity, fitted with a caoutchouc stopper with three holes bored through it. Into one of these holes a vent gas delivery tube passes, on to which three small wash bulbs are blown; into the other two holes are inserted the rounded ends of two lengths of the gas carbon,

commonly used as terminals for the electric lamp; these poles are of such a length that they pass to the bottom of the glass bottle. This is then filled with strong aqueous hydrochloric acid containing about 30 per cent of the anhydrous acid; the stopper in the poles and wash bulbs containing a few drops of water is then fixed into its position, and the evolution vessel placed in a beaker of cold water whilst contact is made with the terminals of four ordinary sized Bunsen's cells, the whole apparatus being placed in a dark room. The mixed gases at once begin to be given off, and ought to pass through the wash bulbs at the rate of about two bubbles per second. It is absolutely necessary that the gas be allowed to come off at this rate for three hours* before it is collected, as up to this time it does not attain a sufficient degree of purity and sensitiveness, whilst after the lapse of this time it is generally found to be fit for use. In order to absorb the excess of chlorine, the waste gas may be led into a condenser containing slacked lime and charcoal in alternate layers. When the evolution has gone on for the above mentioned time a bulb tube, connected by caoutchouc joinings, is placed between the evolution vessel and the condenser, and the gas allowed to pass through. The bulbs, which are made of fusible glass tubing, are blown about the size of a hen's egg, and so thin that they easily break when pressed with the finger. At each side of the bulb the tube is drawn out so as to be very thin in the glass, and to leave the internal diameter not less than 1mm., whilst at the extremities the tube is wider so as to fit ordinary joinings. When the gas has passed through the tube for about ten minutes, the joinings are loosened and each end stopped with a piece of glass rod. The bulb tube thus closed is then removed from the evoluting vessel, and the thinnest part of the tube brought some little distance above a very small Bunsen's flame; the glass then softens below a red-heat, and the ends may be drawn out and

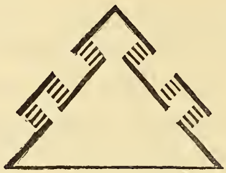
* See Bunsen and Roscoe, Photochemical Researches, 1857, p. 355.

sealed with safety. It is advisable to number the bulbs, and to test the first and last by exposing them to a strong light. Frequently, in spite of every precaution, the gas explodes during the act of sealing, so that in this operation it is advisable to hold the bulb with a cloth rather than in the open hand. As soon as one bulb tube is removed another is placed in connection with the evolution flask, and after ten minutes sealed as described. The above quantity of acid will serve for the production of sixty bulbs. Thus prepared the sealed bulbs may be bept in the dark for any length of time without injury; some, which were known to have been made more than a year, were found to be perfectly good. To explode these bulbs it is only necessary to expose them to diffuse daylight or sunlight, when the combination occurs instantly. Of artificial lights the bright flash produced by the combustion of the vapour of bisulphide of carbon in nitric oxide is most effective; but the light of burning magnesium wire, of phosphorous in oxygen, or the electric light, answers perfectly well Professor ROSCOE stated that Mr. Dancer, of Cross Street, had undertaken to supply the bulbs to persons unable to prepare them.

A paper "On a new form of roof for Dyehouses," by Mr. JOHN THOM, was read, communicated by the President.

The object of the present communication is to describe the construction of a roof for buildings in which there is a good deal of vapour, so as to produce the minimum amount of condensation of such vapour inside the buildings, and thus avoid the production of drops, as well as the minor evil of an obscure atmosphere. Any one practically acquainted with dyeing processes knows well the loss which arises from these two evils. In all cases that I have seen, except where the dyehouse was the lower flat of a series, and where, of course, the covering of the dyehouse was the floor of the room above, the roof of a dyehouse is made about the usual angle or

pitch of an ordinary building supplemented with openings, and these openings on different plans, for allowing the steam or vapour to escape. The usual one being made thus:—



This form seems to send out a large amount of steam or vapour from the quantity of cold air which is mixed with the vapour inside and escapes with it. If one watches the action of the cold air and vapour in this form of roof, it will be observed, that the cold air comes in by all the windward openings, strikes the hot moist air or vapour, drives part out and forces a great part down into the building. I found by taking off the side lifts, and leaving only the top ones, that an improvement was effected. Still, however, the same evils were produced by the top louvre boards, although in a less degree, owing to a less quantity of cold air being admitted.

I also tried removing the top cover, hood, or louvre boards, and contracting the opening, leaving three feet all along the top of the roof without any cover whatever; and this, in spite of the admission of rain, I found to be the best of the old form. Even with this, however, we had always drops and an obscure atmosphere, except in bright dry days. But with this form the number of drops were less and the building clearer than with so many openings.

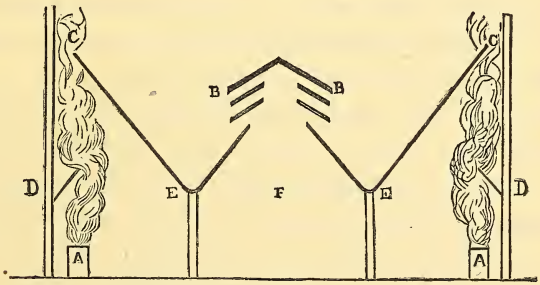
With the old form of roof the most successful plan of keeping the building free from vapour or drops, is to cover over every vessel from which vapour arises, and to have a tube passing from each cover quite through the roof. But this plan has several objections to it; the chief being, that owing to the boxing in of the winces, &c., one does not see how the goods are going on, and whether the processes are being conducted successfully or not. The workmen also aggravate

this evil, by avoiding as much as possible the opening of the covers or boxes to see the goods, on account of the blast of steam which issues out when an opening is made. I have twice fitted up such covers and tubes to all our dyebecks, and both times have been obliged to remove them. The last time I fitted them up I made for the front a light frame with canvass, and this could be raised by a cord and pulley, so as to allow the man to avoid the steam when he opened the box.

Being dissatisfied with all the plans I had tried of getting rid of the vapour, I determined to try a new roof.

Now, if all idea of the old form of roof is laid aside, and it is asked what it is that is really wanted, the conclusion will be that condensation of the vapour must be avoided; that the less air admitted the better, and that the air be admitted under the vapour and pass with it in the same direction, out of the building, mixing as little as possible with it. The admission of air must be at the command of the foreman of the building, and the roof should be at such a pitch that if drops be condensed on it they should not fall; but trickle along the spar or glass into the gutter made to receive them.

Adjoined is a sketch of the plan which I have ultimately adopted to effect these objects.



A A becks along the sides. B louvre boards which can be opened or shut to admit air. C C openings for escape of hot vapour, having the same number of feet of opening as surface

of hot water. D D boards which catch rain drops from unprotected part of roof. F part for assorting goods, &c. All the roof is timber and glass; there are no slates.

By this plan the opening for emission of steam is as small as can be allowed, and all the air necessary is admitted at the proper place, and in such quantity as may be desired. This roof has now been at work in summer and winter, and a drop has never been seen to fall from it except sometimes from gutter E E, and this being invariably in one place it is of no consequence. Except in unusually close weather, the whole place is quite free from condensed vapour. The men are all dry and comfortable, instead of being stifled with the hot vapour and drenched with drops as they formerly were before this new form of roof was tried. There are many details which I cannot describe here, but which I show on the model and drawings. Modifications would be required for different arrangement of becks; but the same principle could always be carried out.

A paper was also read "On the Action of Caustic Soda on Ethylic and Methylic Alcohol," by Mr. A. Mylius, communicated by Dr. E. Schunck, F.R.S.

Schunck's experiments concerning the action on indigo blue of acetate of soda, caustic soda, and alcohol, first led me to examine in his laboratory the influence of caustic soda on alcohol and methylic alcohol in sealed tubes.

I obtained in each case a resin, differing considerably from the resinous body which is obtained by boiling an alcoholic solution of caustic soda for some time at the ordinary pressure and then precipitating by an acid. This so-called aldehyde-resin has a different composition according to the proportion of acid employed in its preparation, so that I think it not improbable that it may be composed of two resins. Its colour is dark brown, and it is soluble in alkali.

On heating the same solution for some time in sealed tubes,

so as to obtain increased pressure, there is formed by the influence of the alkali a resin which is insoluble in alkali. In a short time the liquid becomes red, and on the addition of water the resin is precipitated, the liquid becoming colourless. This takes place also when methylic alcohol is employed.

The resins thus formed are of a red colour.

After filtration and washing with water the resin was dissolved in alcohol, and the residue, after evaporation, dried at 100° C. When cold these resins are hard and brittle, but they have no crystalline structure. They are soluble in alcohol and ether. The resin formed from methylic alcohol melts at 59° C., the other at 65° C. The odour of the former is like that of cedar wood, while that of the latter more resembles the smell of oranges. In general there is a great resemblance between these resins and the natural resins, such as copal, &c. In employing methylic alcohol the formation of the resin takes place much more easily, and a greater quantity of product is obtained. I tried in various ways to decolorise the resins, but did not succeed.

Through the alkaline liquid filtered from the resin a current of carbonic acid was passed, then it was evaporated, and the saline residue having been treated with sulphuric acid in excess, the liquid was distilled. The distillate was acid; silver solution was reduced by it in an instant, so that there was little doubt of the presence of formic acid (but the peculiar smell of propionic acid could not be perceived).

From the analysis it must be inferred that the two resins have the same composition; but still, as their properties are not identical, I think they are only isomeric.

The analysis afforded the following results:

I. 0·1860 grm. (methylic alc.) gave 0·5080 grm. carbonic acid and 0·1550 grm. water.

II. 0·2465 grm. (methylic alc.) gave 0·6795 grm. carbonic acid and 0·1965 grm. water.

III. 0·2090 grm. (ethylic alc.) gave 0·5745 grm. carbonic acid and 0·1590 grm. water.

IV. 0·1805 grm. (ethylic alc.) gave 0·4960 grm. carbonic acid and 0·1330 grm. water.

These numbers lead to the following composition:

	I.	II.	III.	IV.
C......	74·50	75·10	74·97	74·95
H......	8·90	8·85	8·95	8·26
O......	16·60	16·05	16·08	16·79
	100·00	100·00	100·00	100·00

The formula $C_{12} H_8 O_2$ with which they correspond requires in 100 parts

C......................	75·00
H......................	8·40
O......................	16·60
	100·00

The formation of the resins from the alcohols might perhaps be explained by assuming that aldehyde is formed by the oxidation of alcohol, and then by simple loss of water the aldehyde would be converted into resin, since

$$3(C_4 H_4 O_2) - 4HO = C_{12} H_8 O_2.$$

But the formic acid in this case would not stand in any relation to the resin, and even the formation of the resin from methylic alcohol could not easily be explained.

If we adopt the formula $C_{24} H_{18} O_4$, which requires 74·28 per cent carbon and 9·10 per cent hydrogen, we must assume that carbonic and formic acid are taken up by the alcohol, though it is very doubtful whether these acids could separate from a strong base like soda in order to form a neutral resin. The following equations will show what may be imagined to take place in this case:

$$5(C_4 H_6 O_2) + C_2 O_4 + C_2 H_2 O_4 = C_{24} H_{18} O_4 + 14 HO.$$
$$10(C_2 H_4 O_2) + C_2 O_4 + C_2 H_2 O_4 = C_{24} H_{18} O_4 + 24 HO.$$

Taking two atoms of water from $C_{12}\,H_8\,O_2$, the formula of benzole, $C_{12}\,H_6$, remains. I tried to form this carbo-hydrogen by distilling the resin; the distillate had a strong smell of carbolic acid; I acted on it with nitric acid; oxidation took place immediately after the addition of the acid. I now added water, which gave a yellow deposit. I dissolved the latter in alcohol, but on evaporating the alcohol I obtained a resin of the same appearance as the original one. Nitrobenzole had not been formed.

The two resins are of a constant composition, which is proved by the accordance between the third and the fourth analyses, which were made with specimens prepared at different times.

In order to ascertain whether formic or acetic acid takes part in the formation of the resins, I made two other experiments. In one case I added acetate of soda to the caustic soda and alcohol, and in the second case formiate of soda. I analysed the products, and arrived at the following results:

I. 0·2050 grm. (acetate of soda) gave 0·6190 grm. carbonic acid and 0·1625 grm. water.

II. 0·2255 grm. (acetate of soda) gave 0·6800 grm. carbonic acid and 0·1745 grm. water.

These numbers lead to the following composition:

	I.	II.
C	82·37	82·24
H	8·80	8·58
O	8·83	9·18
	100·00	100·00

The formula $C_{28}\,H_{18}\,O_2$ requires

C	82·90
H	8·91
O	8·19
	100·00

Assuming that acetic acid is necessary for the formation of this resin, which differs from the products obtained without the addition of the acetate, the following equation will show the way in which the resin has been formed:

$$4(C_4\,H_6\,O_2) + 3(C_4\,H_4\,O_4) = C_{28}\,H_{18}\,O_2 + 18\,HO.$$

In employing formiate of soda I obtained a resin of the same composition as that produced when no formiate was present.

0·2100 grm. gave 0·5795 grm. carbonic acid and 0·1610 grm. water = 75·25 per cent carbon and 8·57 per cent hydrogen.

The investigation has therefore led to the discovery of the following facts:

(1) The resins which are obtained by the action of caustic soda on ethylic and methylic alcohol in sealed tubes differ from the resin that is formed by the same substances at the ordinary pressure in open vessels.

(2) Methylic and ethylic alcohol produce resins of the same composition.

(3) Formic acid is formed.

(4) When acetate of soda is added the resulting resin differs in its composition.

PHOTOGRAPHIC SECTION.

January 5th, 1865.

JOSEPH SIDEBOTHAM, Esq., in the Chair.

Mr. E. C. Buxton was elected an Ordinary Member.

Some enlarged photographs by Dr. Van Monkhoven were exhibited. The original negatives were four inches square, and some of the enlargements upwards of forty-four inches.

Some specimens of panoramic pictures taken by Mr. J. R. Johnson with his pantascopic camera were exhibited. In this camera the lens turns on its optical centre, depicting the objects composing the view on the screen or collodionised film, which moves tangentially in a contrary direction to the motion of the lens, and thus successive portions of the view are received by successive portions of the moving plate.

Mr. DANCER, F.R.A.S., read a paper "On the exhibition, stereoscopically, of Photography on a large scale." Diagrams were exhibited of the single image prism stereoscope, the double reflecting stereoscope, and the opera glass stereoscope instruments devised by Sir David Brewster for uniting bin-

ocular pictures for the use of lectures, &c. Some beautiful stereoscopic transparencies were cut in two, and each half placed in its position in oxhydrogen lanterns mounted with achromatic object glasses. The half stereographs were then projected in juxtaposition on a long screen, and to realize the proper stereoscopic effect, the members were supplied with achromatised prismatic stereoscopes which had been prepared expressly for the purpose. Mr. DANCER made some remarks on the perfection which the lenticular stereoscope had attained by the employment of achromatic lenses, and urged the necessity of photographers and opticians agreeing to uniform foci for the lenses of stereoscopic cameras and stereoscopes, as it was only by viewing the pictures with lenses of equivalent foci to those with which they were taken that they got a truthful representation. If that rule were observed, the objects would have the same apparent magnitude as they would have to the eyes of a person standing on the spot from whence the view was photographed; that well known rule was too frequently forgotten.

February 2nd, 1865.

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

Mr. A. BROTHERS, F.R.A.S., exhibited a stereoscopic picture of the Blue John Mine in Derbyshire, which he had taken by aid of the magnesium light, giving an exposure of five minutes. The negative was slightly fogged, owing to the lenses not having been wiped when descending from the cold atmosphere of the surface into the mine, which is some 3 to 400 feet below it. The dense fumes of magnesia caused by so long an exposure prevented another trial.

Dr. Joule exhibited a camera, which was an improvement on the one described in the Photographic Notes, September, 1856. The body of the camera consists of a mahogany box holding a wide glass bottle, which is made use of to hold the silver solution. The plate is sensitised by turning the camera on its back, which causes the nitrate of silver to flow over it.

Mr. Montefiore described some experiments which he had made for enlarging negatives by aid of the magic lantern, using magnesium wire for the illuminating power. The wire had been found to burn very steadily in a jet of ordinary gas.

Mr. A. Brothers, F.R.A.S., read a paper entitled "Was Daguerre a Discoverer?"

Mr. Sidebotham read a paper "On the proper focus of lens to be used in taking photographic landscapes, also on some modes of measuring the size of objects therein depicted."

The object of the paper was to show that neither lenses of very long or very short focus give correct perspective, the former making distant objects appear too near, and the latter just the contrary. Lenses of less focus than 7 inches, and greater than 14, make this defect painfully visible.

The latter portion of the paper advocated the placing a rod or mark of such a length, and in such a position in the picture, that it could be used as a scale of measurement for the principal objects. Also that a correct scale be made for each focus of lens, by placing poles of measurement at the distance of 100 yards apart, and marking each 10 yards distance from the camera, so that when once the scale was formed, all photographs taken by that focus of lens (the

distance of objects being known) their size could be measured; or their size being known their distance.

The author, also explained the mode of obtaining the exact measurement of objects, by laying divided scales lengthways and across, so that any person could verify the measurement from the photograph without risk of error. This had been suggested to the Astronomer Royal for Scotland, Professor Smyth, who was at present in Egypt, taking measurements of certain portions of the interior of the great Pyramid, and extracts from some of his letters were read, approving the same; the illumination of the objects being accomplished by the use of Magnesium.

Ordinary Meeting, March 7th, 1865.

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

The following paper "On the Action of Sea water upon certain Metals and Alloys," was read by F. Crace Calvert, Ph.D., F.R.S., F.C.S., &c., and R. Johnson, F.C.S.

We were induced to examine the action exerted by sea water, in consequence of the rapid changes which have taken place of late years in naval architecture, and especially in the substitution of metals and alloys for wood.

To carry out the above views, we took 20 square centimetres of each metal, which we cleaned with great care and attention, in order that the action of the sea water might have its full effect; then two plates of each metal were placed in separate glass vessels, and immersed in equal volumes of sea water. After one month the plates were taken out, and any compounds that had adhered to the surface carefully removed; the plates were then dried and re-weighed, and the loss estimated. To render our results of more practical value, we have calculated the action of 100 litres of sea water upon one square metre of each metal, and the following are the amounts of metals dissolved:—

	Grammes.
Steel	29·16
Iron	27·37
Copper (best selected)	12·96

	Grammes.
Copper (rough cake)	13·85
Zinc	5·66
Galvanised Iron	1·12
Block Tin	1·45
Stream Tin	1·45
Lead (virgin)	Trace.
„ (common)	Trace.

These results appear to us to lead to the following conclusions:—

1. That the metal now most in vogue for shipbuilding, namely, iron, is that which is most readily attacked.

2. That this metal is most materially preserved from the action of sea water when coated with zinc, and therefore, in our opinion, it would amply repay shipbuilders to use galvanised iron as a substitute for that metal itself.

The above facts perfectly confirm those which we have already published in our paper "On Galvanised Iron for Armour Plated Ships," in which it was shown, that when iron was in contact with oak they mutually acted upon each other, producing a rapid destruction of the two materials, whilst little or no action took place between galvanised iron and the wood.

3. The extraordinary resistance which lead offers to the action of sea water, naturally suggests its use as a preservative to iron vessels against the destructive action of that element; and although we are aware that pure lead is too soft to withstand the wear and tear which ship bottoms are subjected to, still we feel that an alloy of lead could be devised which would meet the requirements of shipbuilders.

Feeling that experiments made with a limited amount of sea water might not be a fair criterion of the action of the ocean upon metals, we repeated our experiments upon plates of 40 centimetres square, which were immersed for one month in the sea on the western coast (Fleetwood), taking the precaution that they should be constantly beneath the surface

of the water, and suspended by flax rope attached to a wooden structure, to prevent any galvanic action taking place between the plates and the structure to which they were attached.

The following are the amounts of metals dissolved:—

	Grammes.
Steel	105·31
Iron	99·30
Copper (best selected)	29·72
Zinc	34·34
Galvanised Iron	14·42
Lead (virgin)	25·69
„ (common)	25·85

The above figures suggest the following remarks:—

That the action has been much more intense, in this instance, than when the metals were placed in a limited amount of water at the laboratory. These results are due probably to several causes acting at the same time, viz.:—that the metal was exposed to the constantly renewing surface of an active agent; and that there was also a considerable friction exerted on the surface of the plate by the constant motion of the water, there being at Fleetwood a powerful tide and rough seas. What substantiates this opinion is, that the lead plates undoubtedly lost the greater part of the weight, not by the solvent action of the sea water, but from particles of lead detached from them, in consequence of their coming in contact with sand and the wooden supports to which they were attached; but this cause of destruction having been observed with lead plates, it was afterwards carefully guarded against in the case of all the other metal plates.

We also deemed it desirable to examine the action of sea water on various brasses. We therefore immersed for one month, plates of various alloys in that fluid, and proceeded to record our results;—

ACTION OF 200 LITRES OF SEA WATER UPON 1 SQUARE METRE SURFACE OF THE FOLLOWING BRASSES:

Composition of the Brasses.		*Quantity of Metals Dissolved.*			
		IRON.	COPPER.	ZINC.	TOTAL.
Pure Copper ...	50				
Pure Zinc	50				
	100	—	1·110	10·537	11·647
Commercial Brass:					
Copper.........	66·				
Zinc............	32·5				
Iron and Lead	1·5				
	100	0·579	3·667	3·324	7·570
Muntz Metal (Sheet):					
Copper.........	70·				
Zinc............	29·2				
Iron and Lead	0·8				
	100·0	0·438	4·226	2·721	7·385
Muntz Metal (Bars):					
Copper.........	62				
Zinc............	37				
Lead and Iron	1				
	100·0	0·501	2·697	3·493	6·691
Prepared Brass:					
Copper.........	50				
Zinc............	48				
Tin	2	TIN.			
	100	0·365	7·04	3·477	10·882

The above table shows how very differently sea water acts upon divers brasses and the influence exercised upon the copper and the zinc composing them, by the existence in them of a very small proportion of another metal; thus, in pure brass the zinc is most rapidly dissolved (which, *en passant*, is the contrary to what takes place in galvanised iron), whilst it acts as a preservative to the copper.

Tin, on the other hand, appears to preserve the zinc, but to assist the action of sea water upon the copper.

The great difference between the action of sea water upon pure copper and upon Muntz metal seems to us to be due not only to the fact that copper is alloyed to zinc, but to the small proportion of lead and iron which that alloy contains; and there can be no doubt that shipbuilders derive great benefit by using it for the keel of their vessels.

We were so surprised at the inaction of sea water upon lead that we were induced to compare its action with that of several distinct varieties of water, viz., Manchester Corporation water—well water—distilled water in contact with air—the same deprived of air—and the following are the amounts of metals dissolved by 200 litres of these waters upon 1 square metre of surface during eight weeks.

	Grammes.
Manchester Corporation Water	2·094
Well Water	1·477
Distilled Water (with air)	110·003
,, ,, (without air)	1·829
Sea Water...	0·038

These figures require no comment, as they confirm our previous result that sea water has no action on lead.

Mr. John Robinson exhibited specimens of iron and brass which had been acted upon by the water of the river Medlock, and stated he had found that an alloy of lead, tin, and antimony, resisted the action of sea water better than any other metal or alloy he had tried.

PHOTOGRAPHIC SECTION.

March 2nd, 1865.

Mr. John Parry in the Chair.

Mr. Dancer read a paper entitled, "The Opaque Microscope not New," in which he proved that the so-called new instrument was of very old date, and was described and used previous to the year 1780; and that an improved form of it was exhibited nightly at the Manchester Mechanics' Institution in the winter of 1840-1841.

Mr. Sidebotham exhibited some prints taken by the Wothlytype process, and described the mode of their production. Others printed by the ordinary silver process from the same negatives were also exhibited, and pronounced to be superior in every way.

Mr. Brothers exhibited two photographic prints taken by Mr. Pouncy, of Dorchester, one of them obtained direct from the negative in printers' ink, the other printed from stone. Mr. Brothers also exhibited, with a copying camera, the mode of obtaining photographs of microscopic objects by burning magnesium; also a negative and a print from it of an insect dissection so enlarged.

Mr. E. C. Buxton, jun., read a paper entitled "Photographic Experience in India," and illustrated it by a number of large and beautiful views he had taken in 1863 and 1864.

The difficulties of photography in hot climates such as India are usually believed to be very great, and until quite recently Indian photographs—or, at least, such as *I* had the chance of seeing—were little more than patches of black and white. No doubt there were many exceptions, but I think that was the general character of Indian photography. The hardness and want of half-tone was attributed to the intense light of the sun, the heat, and the difficulty of preserving chemicals in good working order.

I remember reading a letter in one of the journals from a photographer in India, in which he said that the plates could not be kept moist more than a few seconds after leaving the bath, and that to prevent the surface of the film from drying so rapidly he was obliged to fasten a pad of wet blotting-paper inside the shutter of the dark slide. How he contrived to draw out the shutter I cannot understand. My own opinion is, that a photographer who can take good negatives in this country will succeed equally well, if not better, in India, or other hot climates.

A few days before leaving Calcutta, at the beginning of last April, I gave a friend some lessons in the art of coating and developing a plate. In July I received prints from his negatives superior to the ordinary productions of amateurs in this country of two year's practice.

The Exhibition of the Bengal Photographic Society closed just after my arrival in Calcutta. I could only pay one short visit; but my impression was that the number of good pictures was larger than in the annual Exhibition of the London Photographic Society. Probably this improvement in the art is owing to the general use of bromo-iodised collodion, and the greater purity of the nitrate of silver and other chemicals.

About four years since I spent the winter in Algeria and Egypt, taking photographs both with the wet and dry process. Neither country is very warm at that time of year; but the excessive dryness of Egypt gave me some little insight into what was wanted in the photographic line when I started for Singapore in September, 1863. I had previously asked advice from photographers who had worked in India, mentioning the little difficulty I experienced in Egypt. They shook their heads, evidently pitying my ignorance. "Only wait," said one, "till you find your nitrate bath as black as ink some fine morning!" I humbly ventured to hope that such a sad fate was not in store for my bath. Another positively declared that no man except Ottewill could make a camera warranted not to warp or crack. I was also advised to make

my own collodion on arriving out, or at least to mix the iodisers myself. My knowledge of chemistry being very limited, the prospect of turning collodion manufacturer was anything but agreeable. Nevertheless, I laid in a stock of ether and alcohol, and all sorts of iodides and bromides. Gun cotton I did not trouble my head about, though where I expected to find any in Singapore is more than I can now remember. Fortunately my skill was never put to the test.

In *this* country Thomas's collodion with magnesium iodiser had always been my favourite, but after a few weeks of hot weather the loss of sensitiveness is very great; therefore, in addition to several pounds of Thomas's collodion packed in tin cases with sawdust, I took a quantity of Mawson's. This was ordered specially for India. Nothing could possibly work better than it did: it was perfection. All the other chemicals required—such as nitrate of silver, pyrogallic and acetic acids. &c.—should be taken from this country. They are to be had in Singapore, Hong Kong, and the principal towns in India; but the prices usually charged are ruinous, and no purchaser can tell how long the article he requires may have been in stock.

The most important matter, after all, is the choice of cameras, lenses, and a good dark tent. The tent I used was Smartt's, made by Murray and Heath. The time required for setting it up and arranging the bath and developing bottles in their proper places is about five minutes. Murray and Heath also supplied me with a square bellows camera for 11 × 9 plates, fitted with Dallmeyer's No. 3 triplet and Grubb's C lens, together with a field box for chemicals and glass plates. Mr. Rogerson made all the rest of my apparatus, viz., a rigid stereoscopic camera, with rack-and-pinion, fitted with Dallmeyer's single and combination lenses, plate boxes, and draining boxes for holding negatives in the field. These draining boxes are also most useful for carrying negatives on a long journey, such as the overland route, and will stand almost any amount of knocking about without injury to the

contents. I need only say that a box similar to the one you now see, when full of negatives, fell to the ground from an elephant's back: the box was a little worse, and that was all. A nest of three ebonite funnels will be found very useful: glass funnels are certain to be broken. The developing cups may also be of ebonite, but I must say I prefer glass.

On arriving at Singapore the keys of my boxes could nowhere be found. A couple of Chinamen kindly undertook to pick the locks and make new keys. Their "little bill" amounted to three dollars—about thirteen shillings! During my stay in the island there was scarcely a morning when the foliage was absolutely still. At sunrise a breeze nearly always sprung up, and continued more or less till sunset; then it became perfectly calm, and during the night there was frequently heavy rain, with thunder and lightning.

My first attempt at photography was rather disagreeable. I had placed the camera under the shade of a large bamboo, and was focussing carefully. I ought to mention that the morning dress in Singapore is made as light and cool as possible. Loose trousers of silk or calico, grass slippers, and a flannel shirt. In this costume I was suddenly attacked by a swarm of great red ants. They had dropped down from the branches overhead, and were all over me before I noticed them, biting ferociously. At first I thought a snake had bitten me. As soon as I got clear of the focussing cloth and saw the ants, I had nothing for it but to run indoors and plunge into a bath of cold water. After this I carefully avoided bamboo trees. These red ants make nests by gumming the leaves together, in the same way as caterpillars in this country.

The scenery in Singapore is more suitable for the stereoscope than for large pictures. Knowing nothing of the language, and having to do everything myself, small plates were quite as much as I could manage. At first the tremendous heat inside the tent made me feel very sick and giddy, but it soon ceased to be an inconvenience.

Though I used a glass bath with glass top, and silver wire dipper, a kind of scum always formed after a day's work. This had to be cleaned off, and the solution filtered before use. Once, after taking three successful instantaneous negatives, the bath solution became suddenly turbid, and the surface covered with a thick black scum. After filtration it worked as well as ever. With this exception, which I can in no way account for, the bath was never out of order.

The tent was always pitched in a shady spot, if possible, the ground well watered, and the inside of the tent rubbed with a wet sponge to prevent dust. For a few minutes the atmosphere was bearable enough, but after a couple of negatives had been developed it was quite another matter. The stopper of the collodion bottle used to blow out, and the smell of ether, mixed with cyanide, in such a temperature, was anything but agreeable.

After a little experience, however, I found less difficulty in taking good negatives than in this country. Mawson's collodion worked splendidly. The developer I first tried was eight grains of iron and twenty drops of acetic acid to the ounce of water. This did not answer, so I tried fifteen grains of iron and fifteen drops of acetic acid; and, finally, thirty grains of iron, thirty drops of acid, thirty drops of alcohol, and two drops of ammonia to one ounce of water. Nothing could be better than the last formula. I used it afterwards in India both for large and small plates.

Going up from Hong Kong to Shanghae, most unfortunately my bath was left behind. There were plenty of interesting objects near Shanghae, and some very fine bridges over the canal at a town called Wong-dow, which had been burnt by the Taepings. Here I saw a man catching fish with cormorants, and would have given anything for my camera. There was no help for it, so I shot pheasants instead.

The "Alabama" was coaling in the New Harbour, Singapore, when I arrived there on my way to Calcutta. Of course my only thought was how to take a picture of her. All my ap-

paratus had been laid up for a month, and it was raining hard. At night when I was going to bed a report got about that she was to sail early next morning. I had to come on board, and, spite of heat and swarms of the most bloodthirsty mosquitoes, to clean plates and filter bath solution. At four o'clock in the morning everything was ready; but, meanwhile, one of Captain Sherrard Osborne's fleet had come in, and totally spoilt the point of view I had chosen. At daylight I crossed the straits in a boat, and set up the tent under a bank, hoping to avoid the direct rays of the sun. I was slightly mistaken. Before long the sun shone directly on the tent, and the woodwork inside soon became unpleasantly hot to the touch. I got two pretty good views of the "Alabama" at anchor, and another in which she appeared steaming out of the harbour.

The steamer left Singapore on December 24th, and the same evening a ship was reported on fire. At nine o'clock we were close to the burning vessel, which proved to be the "Martaban," set on fire by the "Alabama." The water being perfectly smooth, the effect was magnificent.

On the 26th I arrived at Penang early in the morning. Nothing can exceed the beauty of this island. The heat on the low grounds near the sea is very great; but on the hills, which are often hidden by great fleecy clouds, the atmosphere is deliciously cool and healthy. The steamer only stopped a few hours, but I secured some beautiful negatives. A crowd of Chinese and Malays favoured me with their company. The water running out of the waste pipe caused great astonishment. I was told they called my tent "ruma Setan" (the "Devil's house"), which name is also applied to the Freemason's lodge at Singapore.

On New Year's Eve we anchored at Diamond harbour, on the Hooghly, and landed at Calcutta next morning. As soon as possible I got my baggage on shore and prepared a silver bath of thirty grains of silver to the ounce of water. Finding the light far quicker than at Singapore, I thought it well to

make the solution tolerably acid, and added one drachm of acetic acid to ninety ounces of bath solution. With this acid bath I took a great many instantaneous stereoscopic views, and several negatives 11 × 9 with the triplet, in less than a second.

Within a mile or two of Calcutta there was at that time a most beautiful jungle of palms, cocoa-nuts, bamboos, &c.—a perfect paradise for photography. All this, or the greater part of it, together with the splendid trees in the Botanical Gardens, have since been destroyed by the great cyclone.

Here I used Thomas's collodion, newly iodised with magnesium, with great success. After being iodised a few days, however, the film invariably split at the lower edge of the plate when immersed in the nitrate bath. The crack was about two inches long and one-eighth broad, extending upwards. I should be glad if any one could explain how this was caused. After this I used Mawson's collodion almost entirely. Once I mixed the two together, and found them work well.

The chief drawback to wet photography in India is the enormous weight of apparatus required. It was bad enough by railway, but when I had to transport three heavy boxes by horse *dâk*, as the miserable public carriages are called, it became simply ruinous.

My visit to the ruins of Gaur, in the Maldah district, some 200 miles above Calcutta, gave me more pleasure than anything I saw in India. From Calcutta to Rajmahal there is a railway. There I put my baggage into bullock carts to cross the Ganges, and proceed to Gaur, a distance of thirty or forty miles. I myself rode to the ferry, and was delighted to see a couple of elephants waiting on the opposite bank to convey me to Mootrapore, the residence of an indigo planter—the most hospitable man in the world, and the slayer of at least 200 tigers. Next day I went on with the elephants, and the day following arrived at Gaur, found a tent pitched, and the carts unloaded.

Gaur was the ancient capital of Bengal, and the ruins of the city, surrounded by dense jungle, are magnificent. My

first act was to drink a bottle of beer, the second to light a cheroot, and the third to filter the everlasting bath, and clean half-a-dozen plates. Having performed all these interesting operations satisfactorily I went to sleep, and started early next morning for the ruins—my servant on one elephant, with tent, camera, and field box, while the bath accompanied me on the other elephant. I may as well tell you that I never trusted the bath out of sight longer than I could help, knowing that if any accident occurred there was no chance of getting another between Calcutta and Agra.

The first subject I tried was a fine old minaret, standing by itself, near a large tank swarming with alligators. The attempt was a total failure—nothing but fog, I could not account for it then, nor can I now. Out of fifty-six large plates, however, exposed in India, this was the only failure.

I made matters worse directly afterwards, by stamping on the focussing-screen: luckily half the glass escaped being broken. I would advise any photographer going to out-of-the-way places always to carry a spare focussing-screen. A collodionised plate will answer the purpose at a pinch, but with a badly-lighted subject it is difficult to focus accurately without ground glass.

The next plate was all right, so I moved on to a grove of date palms, every tree of which was full of monkeys, and it was plain that unless they were driven away I might as well attempt to photograph the palm trees in a gale of wind. At last they all took their departure—or at least I thought so. A dark gateway at the end of the grove became beautifully lighted, and not a leaf stirred. I lit a cheroot, placed the dark slide in position, and uncovered the lens, intending to give an exposure of one minute and a-half. Twenty seconds were wanting to complete the time, and already I saw a perfect negative developed, when suddenly an old grey monkey dropped from the top of a tree, and swung on a bough which, with infinite pains, I had brought into focus by an unlimited use of the swing-back. Of course I covered

the lens immediately; but the mischief was done. The father of monkeys grinned and made horrible faces, then dropped to the ground and disappeared. Presently afterwards, while looking about for another view, a whole tribe of monkeys came and sat close to the camera, huddled together like a swarm of bees. The light coming through the branches of trees was very feeble, and having only a long-focus lens, it was useless to attempt taking their portraits.

The last day I spent in Gaur was one of downright hard work. The ruins were a long distance apart, and the tent had to be pitched and taken down five times. As it was getting dusk we passed through a magnificent gateway in the old wall. I was quite tired out and half asleep; but the mahout, or elephant driver, called my attention to it, whereupon I woke up and told my servant to pitch the tent for the fifth time. While he was doing so I looked out for the best point of view. There was no difficulty in finding it. The difficulty consisted in the clouds of dust raised by a long line of pilgrims incessantly passing. A troop of monkeys chattered in the branches overhead. For the monkeys there was no remedy; for the pilgrims there was. Coming out of the tent I was surprised to find a perfectly clear atmosphere. At a little distance the elephants were standing right across the narrow road, and behind them at least 200 pilgrims, partly held in awe by the elephants and partly by a *berkendars,* which, I think, means a "fighting man," who had accompanied me.

After leaving Gaur I went by train to Benares. Stayed three days, and took a few negatives. There are some very very fine mosques in Benares, but so built round as to prevent photographing with ordinary lenses. A globe lens would be of immense service in such places, and it is surprising that they are not in more general use.

The railway is now open from Calcutta to Delhi; but at the beginning of last year there were two breaks in the journey from Benares to Allahabad, and from Agra to Delhi.

I was obliged to travel by *dâk* from Benares to Allahabad, thence by rail to Cawnpore, and again by *dâk* to Lucknow. At the latter place the weather was miserably cold, and the wind too high for photography. The Kaiser Bagh reminded me of the Palais Royal at Paris. It would make an excellent picture in a pantoscopic camera.

I was much disappointed at not being able to do more at Lucknow: once I did set up my camera near the Presidency, but a sudden squall of wind upset it, diminishing the size of my unfortunate small focussing screen.

Leaving Lucknow at midnight, I had time to photograph the memorial over the well at Cawnpore: it stands at the end of a large garden. No natives are allowed to enter unless by special permission; and Europeans driving through the garden do so at foot's pace.

No words can describe the magnificence of the Taj Mehal, at Agra—a mausoleum of white marble, built by the Sultan Akbar over the tomb of his wife. The marble is inlaid in the most exquisite manner with precious stones, in the form of leaves and flowers. In front of the building is an avenue of dark cypress trees, and a long line of fountains, at the end of which stands the Taj flashing like the sun itself. It would be hard to conceive a more difficult subject for a photograph; yet by using a weak developer, the black cypress trees, the shining white marble, and the fleecy clouds were all perfectly rendered.

From Agra to Delhi is one hundred and thirty miles by *dăk*—a most wearisome journey through sandy plains, dotted here and there with low jungle. The Delhi water is full of salt; at least I found it so, though a professional photographer living on the spot told me "it was good enough in his hands, and answered perfectly." I acquiesced humbly, but took a pint of distilled water he was kind enough to give me.

The splendid monument called the Kutub, on the site of ancient Delhi, is about eleven miles from the present city. The height of the pillar is even now, I believe, 340 feet, and

is supposed formerly to have reached 400 feet. The water was quite fit to use, so I brought back several bottles full, intending to take views of the Cashmere gate, &c.; but a letter from the great tiger-slayer, before mentioned, warned me to return at once if I intended to join his tiger party on the 9th of March.

Photography is great, but tiger hunting is greater; so I left Delhi one Monday morning, and, travelling incessantly, reached Calcutta on the Saturday. Here I left all my apparatus, except a plate or two for the benefit of a dead tiger. This I accomplished, but after seven or eight hours' shooting on the top of an elephant even photography becomes a burden.

Having been in at the death of seven tigers, I left Calcutta the first week in April, and I, with all my negatives—namely, fifty-five 11 × 9 inches, and six dozen stereoscopic—arrived in England towards the end of May.

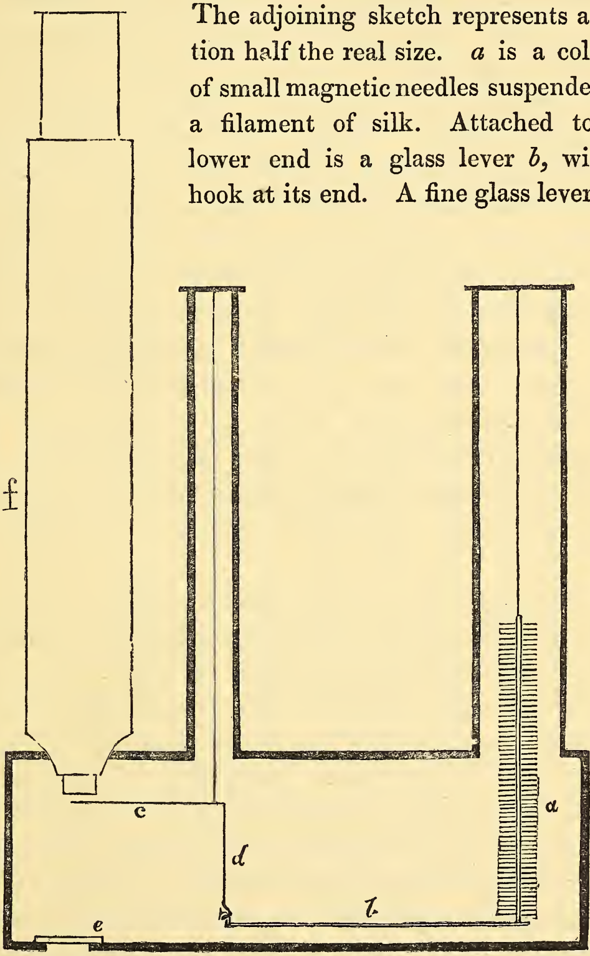
I cannot conceive anything more delightful than a photographic tour in India—arriving out at the end of October, and returning in April. If a man's health be good there is no difficulty in obtaining good pictures. In fact, one great source of failure in this country—dirty plates—scarcely exist in India. If a plate be washed with clean water, and rubbed dry, it is as clean as man can make it. According to my experience collodion cannot be too much shaken before use. I think it a great mistake to use a bottle which has been standing quiet for any length of time; but I know plenty of learned photographers who hold the very opposite opinion.

In conclusion, I venture to hope that photographers intending to pursue the art in India or other hot countries may find the foregoing remarks of service in the selection of apparatus and chemicals.

Ordinary Meeting, March 21st, 1865.

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

Dr. Joule described an instrument he had constructed for showing rapidly minute changes of magnetic declination. The adjoining sketch represents a section half the real size. *a* is a column of small magnetic needles suspended by a filament of silk. Attached to its lower end is a glass lever *b*, with a hook at its end. A fine glass lever *c* is



suspended by a single filament of silk; its shorter arm being connected with the first lever by means of a small hook attached to the fibre *d*. The whole is enclosed in a stout copper box, into which light is admitted through a lens *e*, cemented into an orifice immediately under the object glass of the microscope *f*.

The microscope magnifies about 300 linear, and has a micrometer in its eyepiece, with divisions corresponding to $\frac{1}{2000}$ of an inch. One division corresponds to a deflection of the needle of $4\frac{1}{2}''$, and as a tenth of a division can be very readily observed, the instrument measures deflections to within half a second. So rapid is the action, that on applying a small magnetic force the index takes up its new position steadily in two seconds of time. Besides being a damper to the motion of the needle, the copper box, by its conducting power, equalizes the temperature rapidly, so that the indications are not to any considerable extent disturbed by currents of air. The success of the present instrument encourages the hope that very much greater delicacy may be obtained by a further multiplication of the motion and the use of a more powerful microscope. Dr. Joule stated that he had observed an extensive magnetic disturbance the preceding evening, the index being driven entirely out of the field of view.

The President said that three meteorological instruments of true originality and of unprecedented delicacy had been described to this Society by the inventor Dr. Joule. For common observation the instruments were too refined, but in some fields of enquiry they seemed the only hopeful guides. Manchester has not yet a Meteorological Observatory, although the proposal has often been made to establish one. Private spirit, as in the instance of Mr. Vernon and others, has made the necessity less felt than before. But there is now an opportunity of beginning one with entirely new apparatus of Manchester origin, which would probably very much alter

the quality of the enquiries made in meteorological establishments.

Mr. Baxendell stated in reference to Dr. Joule's observation, that on the same evening he had observed a very fine auroral arch, which at 8h. 20m., G. M. T., passed over ε Bootis, *α* Ursæ majoris, Capella, and a point midway between the Pleiades and ε Tauri. There was at the same time a considerable segment of bright auroral light in the northern part of the heavens. No streamers were visible. About twenty minutes later it was noticed that the position of the arch had changed, the western portion having moved very sensibly in a southerly and the eastern portion in a northerly direction. This change of position was probably connected with the remarkable disturbance of the magnetic needle noticed by. Dr. Joule. The western portion of the arch was brighter than the eastern, and was estimated to be from ten to twelve times brighter than the milky way in Cepheus and Cassiopeia.

Mr. Baxendell also stated that the Society had in its possession a thermometer constructed by the late Dr. Dalton, and which, it is believed, was used by him in many of his meteorological observations. The scale has the initials J.D. and the year 1823 engraved upon it; and the freezing and boiling points of water are indicated on the stem by fine file marks. As it is known that the zero points of thermometers sometimes change to the extent of *one* or even *two* degrees in the course of several years, it occurred to Mr. Baxendell that it would be interesting to ascertain whether any change had taken place in this thermometer, and he had therefore lately tested very carefully the position of the freezing point, but found that no sensible alteration had taken place; and he believed therefore that great confidence might be placed in the observations which Dalton had made with this instrument.

Mr. Dyer communicated a letter from Mr. Joseph Barratt, of Southport, in which the writer endeavoured to account for storms on principles which appear to be almost identical with those advanced many years ago by the late Professor Espy and Mr. Thomas Hopkins.

A Paper was read entitled "Further Observations on the Permian and Triassic Strata of Lancashire," by E. W. Binney, F.R.S., &c.

In previous memoirs published in the Transactions of the Society, the author had given what information he possessed in a fragmentary state, just as he obtained it, of the permian strata of Lancashire and the north-western counties of Westmoreland, Cumberland, and Dumfries, as well as the north-western corner of Yorkshire, and he took sections where he was fortunate enough to obtain them, but he made no attempt to lay the strata down continuously on a map, his materials being far from sufficient for such a purpose.

By looking at a map of the county of Lancaster, the observer will find a great gap between the permian beds of Grimshaw Delph, Bradley Brook, and Skillaw Clough, to the north-west of Wigan, and the sections described by the author at Rougham Point near Cartmel, and Stank near Ulverston. The lower coal measures from Harrock Hill can be traced pretty well towards Chorley, and thence to near Withnell, and then the millstone grit runs to Hoghton, and across the country not very well seen to Griesdale, Scorton, Cleveley, Ellel, Ashton, near Abbey Lighthouse on the Lune, over the mouth of that river to Robshaw Point, and on to Heysham. The country forming the western boundary of the above line is a low district, a good deal covered up with drift, and affording few natural sections to show clearly the relation of the carboniferous to the permian strata. The district probably may afford some sections if carefully investigated, but up to this time it has been quietly dismissed by colouring it red for trias.

In this communication the author gave a little more information which he had lately obtained in a line from Hoghton Tower to Fleetwood; also at Cockersand Abbey, south of the mouth of the Lune, and Robshaw Point and Heysham to the north of the same river; and made a few remarks on some singular red sandstones hitherto classed as trias in the neighbourhood of Whiston, Huyton, Knowsley, Croxteth Park, and Rainford, and laid down as such on the maps of the Geological Survey.

No doubt it is a very difficult matter to determine with absolute certainty where the trias strata end and the permian begin, when there are no organic remains to guide us and we have to trust to a bed of red marl or a deposit of red sandstone. In the author's several memoirs published on this subject, so far as South Lancashire was concerned, the red marls and limestones of Newtown and Bedford are assumed to be the uppermost permian deposits found. It is quite true, as stated in his third memoir—"Some of the sections near Manchester, especially that seen in the valley of the Irk in Cheetham and Newtown, would apparently show that the red marls containing limestones and fossils of the genera *Bakevellia*, *Schizodus*, &c., passed into the overlying trias;" but as a whole it was assumed from other facts that the red sandstone of the trias was unconformable to the underlying permian beds. In a paper published by Sir R. I. Murchison and Professor Harkness, printed in the *Quarterly Journal of the Geological Society* for May, 1864, as well as in the author's last memoir, the thick red sandstones of St. Bees are described as permian and not as trias, and were traced down, as Professor Sedgwick had previously followed them, into Furness near Howcote and Barrow. Anyone who sees the red sandstones much used for building purposes at Shawk, Maryport, and St. Bees, and compares them with that at Howcote, will not be able to distinguish the one from the others. It is only from their physical characters that we can compare these sand-

stones; for up to this time, so far as his knowledge extends, no organic remains have been met with in them. Now this is bringing a permian red sandstone above the Newtown and Bedford red marls and limestones, and introduces for the first time an upper permian sandstone into Lancashire; and this rock runs into the trias so regularly that it will be very difficult to separate it by any well marked boundary from the lower soft sandstone or pebble beds of the trias as laid down and described in the maps and memoirs of the Geological Survey.

It is pretty clear if some of these permian and triassic sandstones are to be classed by their physical characters alone, that certain of the latter rocks, as laid down by the Geological Survey in the Huyton, Croxteth, and Knowsley districts, will probably have to be put into the permian, for no one can tell the red flaggy sandstone of Knowsley quarry from the Howcote and St. Bees sandstones, and it must be taken as permian just as the Howcote rock is identified with that at St. Bees.

As it is desirable to attempt to connect the permian deposits of the South and West of Lancashire with those in the north of the county, as seen at Rougham Point near Flookborough, and Stank near Furness Abbey, he gave what information he was possessed of. The only section hitherto seen near Preston, is one in the Ribble below that town, and appears to be a portion of the pebble beds of the trias. How far it extends up the valley of the Ribble and to Roach Bridge in Samlesbury, where the soft red and variegated sandstones and conglomerate (permian beds) rest unconformably on what appears to be limestone shale, has not been yet determined. Near Cockersand Abbey on the south side of the mouth of the Lune, west of the town of Lancaster, below high water mark, is a small patch of what appears to be permian sandstone.

To the north of the last named place across the Lune at

Robshaw Point, the same soft red sandstone is seen on the beach covered by the tide, and appears to be a continuation of that rock seen to the south, but much better exposed. With these exceptions no further evidence has yet been obtained of any permian beds until we reach Rougham Point.

The triassic beds in South Lancashire, as seen near Liverpool, according to Mr. Hull, are as follows* :—

Formation.	Division.	Subdivision.
New Red Sandstone.	Keuper.	1. Red marl, with beds of upper keuper sandstone.
		2. Lower keuper sandstone or waterstone, with a base of breccia or conglomerate.
	Bunter.	1. Upper red and mottled sandstone.
		2. Pebble beds.
		3. Lower red and mottled sandstone.

Next, as seen near Manchester, where the same author classes the bunter as composed of

1. Upper red and mottled sandstone.
2. Pebble beds.

It will be seen from the above divisions that the lower soft red and mottled sandstone of Liverpool is left out at Manchester altogether, the lowest member of the trias there being the pebble bed. There certainly is the Vauxhall sandstone, which would pass very well for the lower soft red; but the Newtown fossils found above it, clearly cut off that rock from the trias, and establish it with the permian beyond all question.

The author had not made many divisions of the bunter portions of the trias. No doubt they are useful in different places, and have sometimes to be varied with the districts to which they are applied. In the north, about Carlisle, up to this time, only one bed of soft red sandstone without pebbles has come under his notice. But at Sutton, as previously alluded to, there is a soft red sandstone without pebbles

* Manchester Geological Society's Transactions, Vol. II. p. 23.

resting on permian red marls. Similar sandstones, in the same position, are seen near the canal at Bedford; below Messrs. Hampson and Co.'s Printworks, at Clayton Bridge, Manchester; and near Messrs. Brocklehurst's Lime Works, at Ardwick, near Manchester. There is also a soft red sandstone, apparently dipping, under the pebble beds of Heaton Mersey, near Stockport, well seen on the banks of the Mersey from Stockport to Fogg Brook, which would pass for the lower soft sandstone of the trias; but for some reason with which the author is unacquainted, the gentlemen connected with the survey prefer (he is informed) to class this sandstone underlying the pebble beds with the permian rather than the trias.

It appears that through the western part of Cheshire and the adjoining county of Flint, as well as in West Lancashire, where there are few, if any, permian strata exposed, the Geological Survey has always had a lower soft red and mottled sandstone; but when the east part of Lancashire is reached and undoubted permian beds are found, this supposed lowest member of the trias disappears.

The soft yellow and variegated sandstones of Whiston, Croxteth Park, and Rainford, all resting unconformably on coal measures, described by Mr. Hull, are evidently of the same age as the Rainford and Grimshaw delph beds. Unfortunately in no instance have any red marls been yet found lying either above or below them. He had described the latter as permian, whilst Mr. Hull thinks they are the lower red and mottled sandstone of the trias. But with respect to the Knowsley quarry, it so much resembles the St. Bees and Howcote upper permian sandstones, that if they were found in Furness, Sir R. J. Murchison and Professor Harkness would, without doubt, claim them as permian.

It is many years since the author first saw the Knowsley quarry, and he then in his note book remarked that those sandstones, especially that belonging to Mr. Littler, could

not be distinguished from the upper permian sandstones of the neighbourhood of Dumfries, which he had just returned from examining. Now, if they can be proved to overlie immediately the coarse grained, false bedded, soft red and mottled sandstones of Whiston and Croxteth Park, both the latter as well as the former, will have to be classed as permian rocks according to the present Geological nomenclature of the North West of England; and he thinks that the new sections he now describes at Roach Bridge, Cockersand Abbey, and Robshaw Point, tend to confirm this view.

In all the quarries of Lancashire where the trias sandstones have been wrought, the author has never seen so hard and thin bedded a stone as that found at Knowsley. It was formerly used for paving sets in Liverpool, and large quantities of it were broken for road metal purposes, for which he never knew a trias rock used. Some of its beds also afforded fined grained flags, with faces as smooth as any permian sandstone he had even seen in the neighbourhood of Dumfries. A good example of pebble beds is seen at Kirkby Rough; but this rock bears no resemblance to the stone at Knowsley quarry, and the two stones cannot well be classed as the same from their characters.

MICROSCOPICAL SECTION.

February 20th, 1865.

Dr. W. C. Williamson, F.R.S., in the Chair.

Dr. Alcock showed mounted specimens of the Carapaces of Entomostraca, picked from shore sand from the coast of Galway. They included Cythere albo-maculata, angustata, variabilis, flavida, convexa, impressa, pellucida, and quadridentata; besides about thirty other species of Cythere and Cythereis which are not described in Dr. Baird's Monograph.

Professor Williamson said that the further we extend our observations of the lower forms of animal life, the greater become our difficulties in determining specific distinctions; and in the present case it must be remembered that we have only the shell or carapace for examination, and this outer skin is of less value for distinctive characters than the internal parts. Then again it is known that some of the entomostraca undergo several metamorphoses similar to those passed through by the higher crustacea before they become adult, so that he should not be at all surprised if some ten or twenty of these different forms turned out to belong to one and the same species. There still remained a third difficulty, the greatest of all, namely, the doubtfulness of what a species is; and in the lower forms of life the variability is so great that he doubted if specific distinctions could be made with any certainty. The same remark is applicable to the lowest forms of the higher divisions of animals, and the entomostraca standing low in the articulate class are probably subject to similar variability.

Specimens of Hydra with Paramœcium aurelia and Trichodina pediculus parasitic upon them were shown by Mr. A. Brothers.

February 27th, 1865.

J. Sidebotham, Esq., President of the Section, in the Chair.

Exhibitions.

The shell of an oyster with a thick layer of soft chalk-like deposit lining its inner surface.—Mr. Nevill.

A ball composed apparently of fragments of some vegetable fibre felted, from the shore of the Mediterranean at Cannes.—Mr. Sidebotham.

Flowers of a species of Euphorbia, and a leaf of Cycas.—Mr. Grindon.

Mounted specimens of the larval shells of Saxicava rugosa, Arca tetragona, and Ostrea edulis; also of Lima, Pecten, Cardium, and others not yet specifically distinguished, from Connemara shore sand.—Thomas Alcock, M.D.

A series of shells of Helix nemoralis, Helix hortensis, and Helix hybrida were shown by Mr. Sidebotham, who stated his belief that the two former are good species, and the third a true hybrid.

Communication.

Mr. G. E. Hunt read the following "Notes on Mosses."

Campylopus setifolius, Wils.—This species was described by Wilson in his Bryologia Britannica, from specimens collected by the late Dr. Taylor on Carrig Mountain, Ireland. Since then it has been observed by Dr. Moore, of Dublin, in County Wicklow and on Cromaglaun; by myself, in great abundance at Cromaglaun and Gap of Dunloe, Killarney. In these stations it is the female plant that we find. In

August, 1863, however, I met the male plant on the moors of the Isle of Skye, this being the only recorded occurrence of the male plant, and the only occasion of the species being found out of Ireland. It is at once distinguished from every other Campylopus by the large auricles of the base of the leaf, which are composed of perfectly colourless, diaphanous cells, and by the large red quadrate cells above this base.

In Dicranodontium longirostre, which presents some characters similar to this species, the large quadrate cells above the base are green. Specimens were exhibited of both the above species; also Scotch ones of Dicranodontium aristatum.

At Southport, in November last, I observed a new species of Brachythecium, intermediate between campestre and rutabulum, differing from the former in its less plicate leaves and very rough setæ, and from the latter in its slightly plicate leaves, lanceolate, gradually tapering from a wide base to a very acute point, not at all accuminate, shining; inflorescence, as in these species, monoicous. If a variety, it must be united with Brachythecium campestre, which has not yet been certainly identified in Britain. Specimens were exhibited.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 16th, 1865.

Robert Worthington, F.R.A.S., Vice-President of the Section, in the Chair.

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

President.
E. W. BINNEY, F.R.S., F.G.S.

Vice-Presidents.
ROBERT WORTHINGTON, F.R.A.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.
THOMAS CARRICK.

Secretary.
PROFESSOR R. B. CLIFTON, M.A., F.R.A.S.

Mr. Baxendell communicated a series of Raingauge and Anemometer Observations made at St. Martin's Parsonage, Castleton Moor, during the year 1864, by the Rev. J. Chadwick Bates, M.A., F.R.A.S., F.G.S.

The geographical position of St. Martin's Parsonage is lat. 53° 35′ 20″ N.; long. 2° 10′ 31″ W.; and the site of the raingauges is 475 feet above mean sea level. The gauges employed comprise three of five inches diameter, constructed on Howard's principle, and made by Casella, of 23, Hatton Garden; and three of eight inches diameter, on Mr. Glaisher's plan, made by Negretti and Zambra. One gauge of each set is placed at an elevation of one foot, another at five feet, and the third at twenty feet above the ground; and they have all been leveled and tested by Mr. Symons.

The anemometer is by Casella, and is a Robinson's, as improved by Col. Sir Henry James.

To ensure correctness as far as possible, every instrument has been carefully tested both before being placed and also afterwards; and they are all read by Mr. Bates every morning at nine o'clock, and in his absence by his schoolmaster; and that their readings may be as much as possible alike, they often read them together for the purpose of comparison.

The monthly amounts of rain received by the different gauges and the movement of the wind are shown in the following table:—

	5-inch Gauges.			8-inch Gauges.			Total Movement of Wind.
	20 feet elevation.	5 feet elevation.	1 foot elevation.	20 feet elevation.	5 feet elevation.	1 foot elevation.	
January	2·087	2·219	2·350	2·060	2·244	2·320	6422
February	4·135	4·248	4·312	4·385	4·607	4·564	6663
March	1·999	2·108	2·268	1·915	2·210	2·273	9016
April	1·205	1·285	1·396	1·119	1·309	1·406	5927
May	3·185	3·250	3·349	3·245	3·293	3·348	5997
June	3·414	3·566	3·719	3·529	3·643	3·725	6799
July	2·042	2·141	2·269	2·124	2·196	2·304	6170
August	2·205	2·326	2·504	2·277	2·378	2·452	5142
September	3·624	3·803	4·104	3·776	3·923	4·091	6230
October	2·615	2·738	2·849	2·690	2·704	2·785	7556
November	3·548	3·776	4·077	3·699	3·947	4·068	6302
December	2·287	2·456	2·672	2·344	2·543	2·718	6089
	32·346	33·916	35·869	33·163	34·997	36·054	78313

From these results it appears that in every month of the year, and with both sets of gauges, the highest gauge received the least amount of rain; and that the 8 inch Glaisher gauges received slightly greater amounts than the corresponding 5 inch Howard's. The movement of the wind was greatest in March and least in August. No relation appears to exist between the monthly amounts of rainfall and the monthly movement of the wind.

NOTE BY MR. BAXENDELL.

The differences between the amounts of rain received by gauges placed at different elevations at the same station, are

attributed by some meteorologists to the influence of the wind. In order to test the soundness of this view, Mr. Bates's observations were arranged in three groups; the first group, comprising all the rainy days on which the movement of the wind did not exceed 200 miles; the second, those on which it was greater than 200 miles, but did not exceed 300; and the third, those on which it exceeded 300 miles per day. The number of days in each group, the mean daily movement of the wind, and the amounts of rain received by the different gauges were as follows:—

Group.	Number of Days in Group.	Mean Daily Movement of Wind.	5 Inch Gauges. Elevation.			8 Inch Gauges. Elevation.		
			20ft.	5ft.	1ft.	20ft.	5ft.	1ft.
			In.	In.	In.	In.	In.	In.
1	61	133	8·329	8·635	9·071	8·517	8·786	9·007
2	59	249	12·713	13·388	14·171	13·117	13·875	14·390
3	55	385	11·304	11·893	12·629	11·526	12·342	12·660

Representing by unity the quantity of rain received by the lowest gauge in each set, we have the ratios given in the following table:—

Group.	Mean Daily Movement of Wind.	5 Inch Gauges. Ratios of quantities of Rain at Elevations of			8 Inch Gauges. Ratios of quantities of Rain at Elevations of		
		20ft.	5ft.	1ft.	20ft.	5ft.	1ft.
		In.	In.	In.	In.	In.	In.
1	133	0·918	0·951	1·000	0·945	0·975	1·000
2	249	0·897	9·944	1·000	0·911	0·964	1·000
3	385	0·895	0·941	1·000	0·910	0·974	1·000

Comparing the results for group 1 with those for group 2, it would appear that an increase in the velocity of the wind from 133 to 249 miles per day, has a very sensible effect in diminishing the ratios for the higher gauges; but on comparing groups 2 and 3, it appears that the effect of a still further increase in the velocity of the wind from 249 to 385 miles per day, is hardly appreciable. The differences between

the ratios in groups 1 and 2 must therefore be due, to a great extent, to some other cause than the influence of the wind. Determining for each group the mean daily rainfall, we have

Group.			5 in. Gauge at 1 foot elevation. In.	8 in. Gauge at 1 foot elevation. In.
1	Mean daily Rainfall	=	0·148	0·147
2		=	0·240	0·243
3		=	0·229	0·230

The mean daily rainfall is therefore much less in group 1 than in either of the other groups, thus indicating a less abundance of rain-forming moisture in the atmosphere, owing to which the rain drops in falling could not increase in size so rapidly as under the more favourable conditions which existed on the days included in groups 2 and 3.

Separating the rainy days from those on which no rain fell, we have for each month the following results:—

	No. of Days of Rain.	Total Movement of the Wind.	Mean Daily Movement of Wind on Days of Rain.	No. of Fair Days.	Total Movement of the Wind.	Mean Daily Movement of Wind on Fair Days.
January	13	3234	249	18	3188	177
February	12	3583	298	17	3080	181
March	17	6039	355	14	2977	213
April	11	1900	172	19	4027	212
May	12	2153	179	19	3844	202
June	19	4669	245	11	2130	193
July	10	2451	245	21	3719	177
August	16	3153	197	15	1989	132
September	22	5184	235	8	1046	131
October	12	3911	326	19	3645	192
November	17	4532	266	13	1770	136
December	14	3224	230	17	2865	168
	175	44033	251·6	191	34280	179·4

From this table we see that in every month, except April and May, the mean daily movement of the wind was greater on rainy than on fair days; and that the mean daily movement on rainy days for the entire year was 251·6 miles, and on days on which no rain fell only 179·4 miles.

The mean results for the meteorological quarters, and their ratios, are—

	Mean Daily Movement of the Wind on Rainy Days.		Mean Daily Movement of the Wind on Fair Days.		Ratios.
Winter	257	...	175	...	0·68
Spring	252	...	208	...	0·82
Summer.........	228	...	166	...	0·72
Autumn.........	267	...	161	...	0·60

From the numbers in the last column it appears that the mean velocity of the wind on days when no rain fell, as compared with that on rainy days, was greatest in spring and least in autumn; and that the relative velocities in winter and summer were very nearly equal. It will be interesting to ascertain whether this remarkable relation holds good through a series of years.

Arranging the rainy days in four groups, the first containing the days on which the rainfall did not exceed 0·2 inch; the second those on which it was above 0·2 inch, but did not exceed 0·4 inch; the third those on which it was above 0·4 inch, but did not exceed 0·6 inch; and the fourth those on which it exceeded 0·6 inch; and determining for each group the mean daily movement of the wind, we have the following results:—

Group.		No. of Days in the Group.		Daily Rainfall.		Mean Daily Movement of the Wind.
				in. in.		
1	...	108	......	0·0 to 0·2	......	236
2	...	40	......	0·2 — 0·4	......	270
3	...	17	......	0·4 — 0·6	......	300
4	...	10	......	above 0·6	......	263

It appears, therefore, that the maximum mean daily movement of the wind occurs when the daily rainfall is about half an inch, and that during excessive falls of rain the velocity of the wind is very sensibly diminished.

Mr. G. V. Vernon, F.R.A.S., communicated the following "Note on the Rainfall of the last Twenty-nine Years at Royton, Oldham," by John Heap, Esq.

Seeing in the proceedings of the Manchester Literary and

Philosophical Society a few days since, a statement of rainfall in the past year and comparisons with the average fall for 71 years, I was led to examine the mean monthly values from my observations during the last 29 years, in order to ascertain how far they would bear out the theory of a maximum value in October and minimum in April, and find they agree with the theory in every particular, even with the exception in September. And moreover I find that the sum of the monthly mean values of any two months, six months apart, as January and July, February and August, &c., is very nearly the same value, being about one-sixth of the yearly average.

Below are the results, and also Mr. VERNON'S mean values:—

1864.	Days.	Fall in Inches.	Average of 29 Years.	Difference from 29 Years Average.
January	11	1·479	2·369	−0·890
February	12	3·585	2·201	+1·384
March	17	2·275	2·133	+0·142
April	10	1·260	2·093	−0·833
May	11	2·833	2·388	+0·445
June	18	3·450	3·117	+0·333
July	9	2·035	3·294	−1·259
August	13	2·832	3·595	−0·763
September	21	4·130	3·071	+1·059
October	13	2·800	3·680	−0·880
November	18	3·885	3·026	+0·859
December	17	2·860	2·656	+0·204
	170	33·424	33·623	−0·199

Monthly means of 1864.	Ins.	Monthly means of, for 71 years.	Ins.
January and July	5·663	January and July	6·029
February ,, August	5·796	February ,, August	5·971
March ,, Sep.	5·204	March ,, Sep.	5·545
April ,, Oct.	5·773	April ,, Oct.	5·856
May ,, Nov.	5·414	May ,, Nov.	5·824
June ,, Dec.	5·773	June ,, Dec.	6·173
	33·623		35·398

Ordinary Meeting, April 4th, 1865.

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

Messrs. H. M. Ormerod and W. Brockbank were appointed Auditors of the Society's Accounts for the present Session.

A communication was read entitled "An Instance of the Injurious Action of Alkalies on Cotton Fibre," by Messrs. Heinrich Caro and William Dancer.

A remarkable instance of the deleterious action of alkali on cotton fibre has lately come under our notice, when examining some indigo prints, which had been stiffened or finished with silicate of soda, and kept in bales during about two years. The strength of the fibre of the greater part of these goods had decreased to about one-third of the strength of some pieces which had been packed in the same bales, and which differed in no other respect from the others except in their having been finished with starch. We therefore surmised that silicate of soda had been the primary cause of the deterioration of the goods. Further observations convinced us, however, that the injury was due to the long continued action of free or carbonated alkali upon the cotton fibre.

Some of the sound pieces (which, as before mentioned, had been finished with starch) had been packed between the silicated goods, and had abstracted soda from them which had penetrated from the places of contact into the interior of the pieces to a considerable depth. In the same ratio in

which the pieces had taken up soda it was found that they had diminished in strength. On the other hand it was found that in such places of contact the silicate of soda of the silicated goods had suffered a partial decomposition, extending to the depth of four or five layers of the pieces. The silicate of soda in the middle of the pieces contained from 70 to 74 per cent of silicic acid, combined with from 30 to 26 per cent of soda; whilst the analysis of the silicate of soda contained in the contact layers showed that from one-third to two-thirds of its soda had been abstracted This loss of soda was accompanied by a change of strength of the cloth which appeared to bear some proportion to it; the layers or folds of the cloth *decreasing* in strength as they were removed from contact with the starched goods until the silicate of soda attained the same composition as that found in the most rotten parts of the piece, this generally taking place about the fourth or fifth layer or fold of the piece, as before stated.

The following table shows the changes in strength produced by this decomposition of the silicate of soda.

	Finished with Starch.		Finished with Silicate of Soda.					
	Middle.	Contact Layer.	Contact Layer.	2nd.	3rd.	4th.	5th.	Middle.
Strength ...	100	81	89	68	62	54	48	35

The silicate of soda had evidently been decomposed with the formation of free alkali and an acid silicate which appears to have very little action on the cotton fibre.

In some places the decomposition had gone further, and free silicic acid had separated out in the form of a white powder upon the surface of the cloth. The same decomposition, accompanied by the same changes in the strength of the cloth, was observed upon all pieces which had been in contact with the paper used for wrapping the bales. In this

instance the paper had absorbed the liberated soda, and the cloth in contact with it had almost entirely retained its original strength.

The white portions of the patterns were in a further advanced state of decay than the blue ones, in most instances retaining only 10 per cent of their original strength. In the goods finished with starch only, the whites were equally as strong as the blues. In the goods finished with silicate of soda the whites were almost as strong as the blues in all places where the before-mentioned decomposition of the silicate of soda had been accompanied by an abstraction of soda; but in the interior of the goods, where the silicate of soda had retained its original composition, the strength of the whites had decreased to about one-third of that of the blues. It was therefore evident that this excessive decay of the whites was due to some cause which had assisted the action of the alkali upon them, and we believe to have found an explanation of this in the action of the silicate of soda upon the sulphate of lead contained in them to the amount of about 10 per cent of the mineral ash.

Sulphate of lead has been an ingredient of the resist paste printed upon the places intended to remain white, and by the subsequent action of lime and sulphuric acid it has become fixed in the fibre. We have noticed that sulphate of lead decomposes solutions of silicate of soda very rapidly, with formation of sulphate of soda, free silicic acid, and silicate of lead.

These changes give rise to the production of a crystallisable and strongly efflorescent salt, and to an increase in bulk; and we think that the mechanical effect produced by the crystallisation of the sulphate of soda formed may have caused a further and final disintegration of the fibre already weakened by the action of the alkali. Under the microscope the fibre of the white portions of the pattern presented the appearance of *cylindrical* tubes, partially covered with

minute crystals (soluble in water); in some places these tubes appeared to be split longitudinally.

A paper on the same subject was lately read before the Chemical Society of London by Dr. F. Crace Calvert, F.R.S.

A paper, entitled "Remarks on the Microscopical Appearances of Cotton Hair during Dissolution in the Ammoniacal Solution of Copper," was read by J. B. Dancer, F.R.A.S.

The structure of cotton hairs has occasionally furnished an interesting topic for conversation at the meetings of our Microscopical Section.

Two of our members, Mr. Chas. O'Neill and Mr. Heys, have given considerable time and attention to this subject. Mr. Walter Crum, F.R.S., communicated to the Chemical Society a memoir "On the Cotton Fibre," and the manner in which it unites with colouring matter. His paper is illustrated with some beautifully executed drawings of the microscopical appearances of cotton in the natural state, and when mordanted, mercerised, and treated with various dyes; this paper is well worthy the attention of those interested in this branch of inquiry. Mr. Crum has presented a copy of his memoir to this society. His description of the ordinary appearance of the cotton fibre agrees so nearly with what I believe it to be, that I will take the liberty of referring to his printed paper at page 5. To Mr. Crum's description I may add, that many specimens of cotton, especially on the cylindrical portion of the hairs, shew transverse markings. At times these appear at tolerably regular intervals, they have been claimed as evidences of spiral structure; when, however, they are examined with magnifying powers of 1,000 to 1,200 diameters they proved to be cracks in the external membrane. Other portions of cotton exhibit longitudinal furrows, irregular

in length and direction—having a shrivelled appearance something like the bark of an aged tree. In gun cotton, the transverse cracks are very numerous. From an examination of transverse sections of cotton, I incline to the opinion that there is an external membrane distinct from the true cell wall or cellulose matter;* inside the cellulose there is an irregular cavity, this, in some specimens (when viewed longitudinally), appears to contain granules, probably the remains of the organising fluid contents of the cell, the mucous matter which is seen in growing cotton as mentioned by Captain Mitchell, in his letter to Mr. Hurst, read at this society, March 22nd, 1864.

On the 21st of April, 1863, Mr. Chas. O'Neill made a communication to this section, "On the Appearance of Cotton Fibre during Solution and Disintegration;" these experiments referred to the application of Schweizer's solution of copper and ammonia.

Under the action of this solvent, Mr. O'Neill considers that cotton exhibits spiral vessels situated either inside or outside the external membrane. In a paper, read by the same gentleman, on the 18th of May, 1863, it is stated that spiral vessels are seen during the solution of gun cotton in ether and alcohol. On the 21st of December, 1863, Mr. Heys read a paper before this section, in which he refers to spiral vessels in cotton hairs which seem to prevent the collapse of the tubes. The announcement of the discovery of spiral vessels excited my curiosity. Having often examined varieties of cotton under the microscope, without suspecting any such structure, I was naturally desirous of witnessing its appearance during dissolution.

A careful examination of cotton in the copper solvent, with powers varying from 50 to 1,200 diameters, showed me the appearances described by Mr. O'Neill. I could not, however, endorse his interpretations of them. On the 16th

* See Mr. O'Neill's Paper, April 25th, 1863.

of January, 1865, I sent a letter to the chairman of the microscopical section, stating my belief that the spiral appearances could be clearly traced to a mechanical action which the solvent exerted on the vegetable cell, and that at some future time I hoped to illustrate this to the members of the section.

Since December last, I have subjected cotton during microscopical examination to a variety of influences in acids, alkalies, metallic solutions, iodine, and also gun cotton in varied proportions of ether and alcohol. Repeated experiments tend to confirm my disbelief in the existence of spiral vessels, properly so-called, either inside or outside cotton hairs.

It would be difficult to explain, by means of drawings, how these pseudo spirals are created, and have, therefore, supplied a number of microscopes for the purpose of showing at the close of the meeting the actual appearances.

Some of the gentlemen present have witnessed these experiments, but, for the benefit of those who have not, I shall attempt a brief explanation to enable them to comprehend more readily what they will see under the microscopes.

In order to observe the action of the copper solvent on cotton, place a few hairs about a quarter of an inch in length on a glass plate, and cover them with thin glass; it is useful to rub a little beeswax on the glass plate, in such a manner, as will just support the covering glass to prevent too great a pressure on the cotton; then arrange the cotton under the microscope with a power of not less than 200 diameters. The solvent should be applied by a glass pipette to the edge of the covering glass whilst the observer is looking through the microscope (this is important). If the solvent is very strong, the action is too rapid for the eye to follow, if of moderate strength it will be seen that as soon as the solvent comes into contact with the cotton in the field of view, a rapid rotation or twisting of the hairs takes place.

In my opinion, it is this rotating action which brings about the appearances which have been mistaken for spiral vessels.

The explanation which I have to offer for the phenomenon is this: first, we have the external membrane of the cotton, then the cellulose and primordial utricle, and finally, the dried contents in the cell, which I take to be the remains of the organising fluid.

Observation shows that the external membrane is not elastic and only partially soluble.

The cellulose is exceedingly elastic and soluble, and expands to a remarkable degree in the act of dissolution. The contents of the cell behave in a similar manner to that of the external membrane; it is neither elastic nor very soluble. The most successful experiment is made by allowing the copper solvent to come at once into contact with some length of the cotton hair. The solvent permeates some parts of the external membrane more easily than others, and causes a rapid expansion of the cellulose, which bursts the external membrane, and as this action is taking place at various portions of the same hair, a tangential force is exerted which twists and contorts the cotton in the direction of its length, and thus a spiral appearance is given to the whole structure of the cell.

The non-elastic external covering is twisted round the expanded cellulose, sometimes as a single band, at others like a bundle of fibres.

In those parts where the external covering has given way all round the hair, the cellulose expands into a bulb, pushing back the external membrane into a series of folds which form a ligature, and resists the expansive force of the cellulose. A number of these ligatures cause the expanded cellulose to assume the appearance of a string of beads. The lateral expansion of the cellulose contracts the length of the hair, and this causes the contents in the cavity of the cell to assume a corrugated appearance; this corrugation has also

been subjected to the twisting power along with the other parts of the cell, and thus its spiral appearance is produced.

What becomes of the primordial utricle, I cannot state with certainty. After the disappearance of the cellulose there is an envelope left, which surrounds the contents of the cavity, this may be the primordial utricle, or the film left by the drying up of the protoplasmic or organising fluid.

If the solvent is made to come into contact with the ends of recently-cut cotton a beautiful trumpet mouth is produced—the exposed surface of cellulose has expanded and pushed back the external covering into folds—the contents of the cell may, in this case, be seen projecting from the mouth of the trumpet form.

Long after the complete dissolution of the cellulose has taken place, the external membrane remains just as the rotation or twistings had left it, some portions in the form of rings, which had been the ligatures between the bulbous expansions, other portions as irregular spirals.

The cell contents also remain as twisted corrugations. From the observed difference in solubility between the cellulose and the external and internal matter, I should imagine a difference in constitution.

A few experiments have led me to suspect that some of the spiral appearances observed in hemp and flax fibres during dissolution may possibly be caused by the mechanical action of the solvent employed.

P.S.—In making the cupric oxide with ammonia, the oxide of copper requires a thorough washing before dissolving in the ammonia. The presence of any salt of ammonia, even in very small quantities, interferes with its power in dissolving cotton.

Mr. Dancer also read a paper "On Pseudoscopic Vision through Prisms."

If we look with both eyes at an object, such as the flat top of a table for example, and then interpose a prism between one eye and the object, we discover, after a short time, that the portion of the surface to which the sight is particularly directed has apparently changed its distance. If, in trying the experiment, the thin edge of the prism is turned inwards to the nose, the flat surface will appear concave, if, on the contrary, the base or thick edge is turned towards the nose, the surface will appear convex. The full effect of this alteration in the appearance of the object is not realized immediately, some persons see it perfectly in a few seconds, others require some moments of steady gazing before it becomes evident to them.

The character of the surface to which the vision is directed exercises some influence in producing the effect. A circular table covered with a cloth of a bright pattern, having a few articles disposed towards the edges, exhibits this fallacious vision in a marked degree.

The angle of the prisms for shewing these experiments should be about 15 degrees, if less than this, the elevation or depression of surface is not sufficient to produce a good effect; if the angle is much greater than 15 degrees, many persons are unable to unite the refracted image of the prism with the real image seen by the other eye.

Achromatic prisms are much to be preferred in these experiments to those which are uncorrected for colour. Experiments with these prisms have shewn that the power of converging the optic axes, differ very considerably in individuals.

Oculists occasionally recommend prismatic lenses mounted in spectacles to assist persons who suffer from insufficiency of the recti interni muscles; it would be interesting to know if those so assisted, have noticed the fallacious appearances which the healthy eye can appreciate. The pseudoscopic effects are exaggerated by using a prism to each eye, but in most persons this produces a painful sensation.

The explanation of these phenomena, which I offer with some hesitation, is based upon the supposition that in binocular vision we estimate the distance of an object by the degree of convergence of the optic axes. In these experiments, when a flat surface appears concave by the interposition of the prism: the optic axes are made to converge on a point situated behind the real surface, and the imagination gradually removes the object to this apparent distance.

When the base of the prism is towards the nose, then the flat surface becomes convex, in this case the optic axes cross in front of the real surface, and the imagination raises the object to that point. A diagram of the convergence of the optic axes on an object, before and after the interposition of the prism, will show that when the thin edge of the prism is turned towards the nose, the effort made to unite the real and the refracted image is the same as if the vision was directed to a point more distant than the real object. The opposite to this takes place when the base of the prism is turned towards the nose. It is very possible that the pseudoscopic vision through prisms may have been noticed by others, but I have not been able to discover any description of such in the works to which I have access.

Dr. Angus Smith explained a mode of analysis which he has called minimetric. The idea, he said, may perhaps not be quite new, but it is well to give the method a name. It is based mainly on the fact that we can retain in the memory with great exactness the character of a precipitate of a given degree of translucency. For carbonic acid the author finds a precipitate of carbonate of baryta caused in baryta by ·2515 cub. c. of carbonic acid, or nearly three times that amount in lime water. If the carbonic acid in air is sought, the air is made to act on the baryta until the precipitate is obtained. In other words we use the *smallest measure* of air which will produce the precipitate. For this reason the name *minimetric* is adopted. The plan may be used for hydrochloric acid, sulphuric and sulphurous acid, sulphuretted hydrogen, &c., and probably has been used frequently without bringing it forward as a method for accurate use.

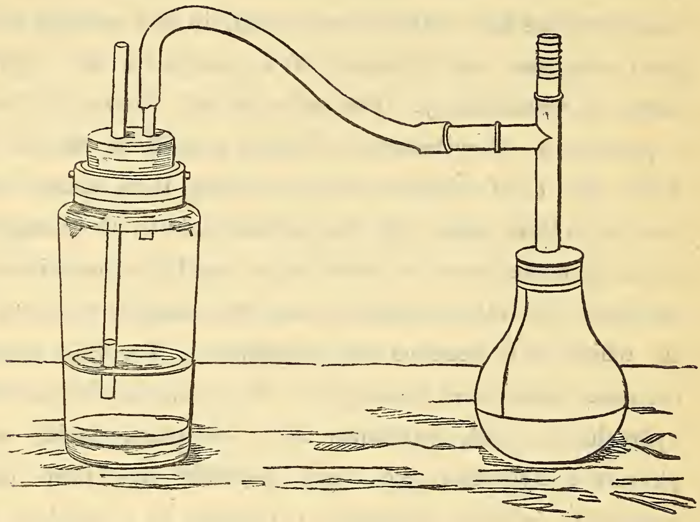
Two modes of using this mode of analysis were described. The first was by the use of a finger pump, an elastic ball with two valves. When pressed the air was driven out, and when expanding the air was drawn through the liquid. The air and liquid were then shaken together. This was repeated until the precipitate was attained.

One easy method of finding the precipitate for comparison was by shaking half an ounce avoirdupois (14·17 cub. c.) with 23 ounces of air in Manchester, or nearly 30 in London, or elsewhere, according to residence.

Experiments made with this apparatus shew it to be extremely delicate. The carbonic acid in the air of a room can be estimated in a few minutes.

A table is made of the following kind, but it must be

adapted to the size of the bulb. A cut showing the bottle and finger pump is given.



For very bad air smaller bulbs were shewn, such as were recommended for workshops, mines, &c.; a convenient size for common life is here given:—

No. of Strokes of the Finger Pump, or No. of Ballfulls of Air.	Per cent. of Carbonic Acid indicated in the Air.	Actual Amount of Carbonic Acid in the Air of the Ball.
		Grammes.
1	6·0	0·2515
2	3·0	0·1257
3	2·0	0·0838
4	1·5	0·0629
5	1·2	0.0503
6	1·0	0·0419
7	0.8	0·0359
8	0·75	0·0316
9	0·66	0·0279
10	0·056	0·0251
11	0·054	0·0229
12	0·0499	0·0209
13	0·0460	0·0193
14	0·0428	0·0180
15	0·0399	0·0167

In all cases only half an ounce of baryta solution was used.

Minimetric House and Workshop Method.

A.

This method is partly described in the report on the air of mines, and long tables given. There were shown two modes of using it, first, with baryta water; second, with lime water.

Suppose we desire to know if the air contains more than 0·04 per cent of carbonic acid, we fill a bottle containing 5·422 ounces with air by pumping as elsewhere described, with a little finger pump, and shake in it half an ounce of baryta water. If there is any precipitate at all, the amount of carbonic acid in the air is above 0·04 per cent. This would indicate that the air is less pure than outside.

If we allow 0·06 p.c. of carbonic acid in a room, we take a bottle of the size of $3·6+\frac{1}{2}$ ounce=4·1 ounces, or 116·23 c.c., and if, after a trial as before, we find a precipitate, however small, or a decided, although slight milkiness, the air is deteriorated beyond 0·06. This relates to dwelling-houses. If for workshops $\frac{1}{4}$ p.c. (0·25) is allowed, a bottle of 0·867+0·5 ounce=1·367 ounces or 38·744 c.c. is sufficient. This could go into the waistcoat pocket.

If 0·5 or $\frac{1}{2}$ per cent. is permitted, a bottle of 0·433+0·5 ounce is enough=0·933 ounces or 26·475 c.c. This amounts to nothing more than shaking an ounce bottle. The addition of half an ounce is for the baryta water.

B.

The lime water method will probably be adopted more usually, as lime is so common. The experiment is exactly the same as with baryta water, but larger bottles are required.

0·06 carbonic acid in the air gives no precipitate or milkiness when ½ ounce of lime water is added to a bottle of the air, containing.....................................10·9 ounce.
(=309 cub. cent.)

0·25 ditto	ditto	2·997 ounces. (=84·958 cub. c.)
0·5 ditto	ditto	1·748 ounces. (=49·564 cub. c.)

The author said that by this simple method the greatest refinement could be attained.

Annual Meeting, April 25th, 1865.

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

On the motion of Mr. BROCKBANK, seconded by Mr. CARRICK, it was resolved unanimously—"That the election of Sectional Associates be continued during the next session."

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

In presenting their annual report the Council have again to congratulate the Society on the satisfactory position of its finances. Although several payments of a somewhat exceptional character have been made during the past year, the balance in the Treasurer's hands has only been reduced from £371. 8s. 1d. on the 31st of March, 1864, to £360. 4s. 3d. on the 31st of March, 1865.

The number of ordinary members on the 1st of April, 1864, was 193, and *ten* new members have since been elected. The loss by resignations has been *three*, by defaulters *four*, and by deaths the unusually large number of *nine*. The number of ordinary members on the roll of the Society on the 1st of April instant was 187.

The deceased members are,—Messrs. John Atkinson, C. F. Ekman, John Gould, John Hetherington, Thomas Hopkins John Jesse, F.R.S., William Nield, John Shuttleworth, and Robert Walker, M.D.

In the death of their late Honorary Librarian, Mr. C. F. Ekman, the Society have lost an officer whose intelligence, zeal, and devotion to his duties and to the interests of the

Society, have rarely been equalled. In his hands the Society's library has become one of the most valuable libraries of reference in the kingdom; and before his death he had succeeded in establishing a system of exchange of publications with nearly all the leading home and foreign literary and scientific societies. He took a very prominent part in the framing of the new code of rules passed by the Society on the 22nd of January, 1861; and his unwearied exertions and influence, and the valuable assistance he was ever ready to render to members or others engaged in original researches, contributed materially to raise the Society to its present high position.

Mr. John Atkinson, F.G.S., had been a member of the Society for many years, and was a constant attendant at its meetings, to which he made frequent communications. During late years he supplied the Society with his meteorological observations made at Thelwall, and which were printed in our proceedings. He was for several years the active Secretary of the Manchester Geological Society, to which he contributed several papers published in its memoirs.

Mr. Thomas Hopkins was one of the oldest members of the Society, having been elected on the 18th of April, 1823. He was for many years a very active and efficient member of the Council, and was for some time one of the Society's Vice-Presidents. He contributed several valuable papers on meteorological subjects to the Society's memoirs, and was the author of a work on "Atmospherical Changes."

In accordance with the resolution passed at a meeting of the Society held on 12th January, 1864, the Council procured dies for a medal, and six copies of the medal in bronze; and appointed a committee to consider and report upon a scheme for regulating the selection of subjects and services for which a medal should be awarded. Owing, however, to the importance of the questions which have been brought under their notice, and the necessity of giving them careful and mature

consideration, the Committee have not yet been able to present their report, but hope to be able to do so at an early meeting in the ensuing session.

Since the last annual meeting the proceedings of the Sections have been marked by the same activity and interest which have characterised them from their establishment. In connexion with the Sections the Council refer with satisfaction to the success which has attended the scheme for admitting Sectional Associates. Twenty gentlemen have availed themselves of the advantages which it offers; and by the communications some of them have made, and the part they have taken in the discussions of the various subjects that have been brought under notice, they have added much to the interest and usefulness of the meetings.

The following is a list of the communications made to the Society at its ordinary and sectional meetings during the past session:

October 4th, 1864.—"Remarks on Mr. Dyer's Paper, entitled 'Notes on Spinning Machines,'" by H. Brierly, Esq., communicated by E. W. Binney, Esq., F.R.S., &c.

October 18th, 1864.—"On the Relation of Force to Matter and Mind. Part I.," by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Mem.

October 13th, 1864.—"Note on a New Star near the Greenwich Variable, No. 1773, of the Twelve-Year Catalogue," by Joseph Baxendell, F.R.A.S.

October 17th, 1864.—"On a Contrivance for regulating the amount of light transmitted from the Source of Illumination to the Mirror of the Microscope," by J. B. Dancer, F.R.A.S.

November 15th, 1864.—"On the Composition of the Atmosphere," by R. Angus Smith, Ph.D., F.R.S., President of the Society.

November 29th, 1864. — "On Differential Equations," by His Honour Chief Justice Cockle, M.A., F.R.A.S., &c., communicated by the Rev. Robert Harley, F.R.S., &c.

November 29th, 1864.—"Notes on Marine Shells found in Stratified Drift at Macclesfield," by R. D. Darbishire, B.A., F.G.S.

November 28th, 1864.—"On Specimens of Foraminifera from Roundstone, Connemara," by Thomas Alcock, M.D.

December 8th, 1864.—"Note on the Period and Changes of the Greenwich Variable in Vulpecula, No. 1773 of the Twelve-year Catalogue," by Josh. Baxendell, F.R.A.S.

December 8th, 1864.—"Observations of the Greenwich Variable in Vulpecula and its Companion Stars," by George Knott, F.R.A.S., communicated by Josh. Baxendell, F.R.A.S.

December 13th, 1864.—"On the Discovery of the Bones of the Mammoth (Elephas Primigenius) in a Fissure of the Carboniferous Limestone at Waterhouses, near Leek," by William Brockbank, Esq.

November 3rd, 1864.—"On Printing Transparencies for the Stereoscope and Magic Lantern," by Joseph Sidebotham, Esq.

December 19th, 1864.—"On the Structure of Cotton," by W. H. Heys, Esq.

January 10th, 1865.—"On some Products derived from Indigo Blue," by Dr. E. Schunck, F.R.S., &c.

January 10th and 24th, 1865.—"On some Physiological Effects of Carbonic Acid," by Dr. R. Angus Smith, F.R.S., President.

January 24th, 1865.—"Observations of a large group of Solar Spots," by Jas. Nasmyth, Esq., C.E., Corresponding Member of the Society.

January 12th, 1865.—"Account of a Fire-ball seen December 13th. 1864," by Thomas Heelis, F.R.A.S.

January 12th, 1865.—"Note on the Rainfall of 1864," by G. V. Vernon, F.R.A.S.

February 7th, 1865.—"On a New Re-agent for the Separation of Calcium from Magnesium," by Edward Sonstadt, Esq.

January 16th, 1865.—"On the Plumules or Battledore Scales of the Lycænidæ," by John Watson, Esq.

January 16th, 1865.—"Notes on the Development of the Wings of Lepidopterous Insects," by Josh. Sidebotham, Esq.

February 21st, 1865.—"On a new form of Roof for Dyehouses," by John Thom, Esq.

February 21st, 1865.—"On the Action of Caustic Soda on Ethylic and Methylic Alcohol," by Mr. A. Mylius, communicated by Dr. E. Schunck, F.R.S.

January 5th, 1865.—"On the Exhibition Stereoscopically of Photographs on a large scale." by J. B. Dancer, F.R.A.S.

February 2nd, 1865.—"On the proper focus of Lens to be used in taking Photographic Landscapes; also on some modes of measuring the size of objects therein depicted," by Joseph Sidebotham, Esq.

March 7th, 1865.—"On the Action of Sea Water upon certain Metals and Alloys," by F. Crace Calvert, Ph.D., F.R.S., &c., and R. Johnson, F.C.S.

March 2nd, 1865.—"The Opaque Microscope not New," by J. B. Dancer, F.R.A.S.

March 2nd, 1865.—"Photographic Experience in India," by E. C. Buxton, Jun., Esq.

March 21st, 1865.—"On an Instrument for showing rapidly Minute Changes of Magnetic Declination," by J. P. Joule, LL.D., F.R.S., &c.

March 21st, 1865.—"Further Observations on the Permian and Triassic Strata of Lancashire," by E. W. Binney, F.R.S., &c.

February 27th, 1865.—"Notes on Mosses," by G. E. Hunt, Esq.

March 16th, 1865.—"Results of Raingauge and Anemometer Observations made at St. Martin's Parsonage, Castleton Moor, during the year 1864," by the Rev. J. C. Bates, M.A., F.R.A.S., and Note on the same, by Josh. Baxendell, F.R.A.S.

April 4th, 1865.—"An Instance of the Injurious Action of Alkalies on Cotton Fibre," by Messrs. Heinrich Caro and William Dancer.

April 4th, 1865.—"Remarks on the Microscopical Appearances of Cotton Hair during Dissolution in the Ammoniacal Solution of Copper," by J. B. Dancer, F.R.A.S.

April 4th, 1865.—"On Pseudoscopic Vision through Prisms," by J. B. Dancer, F.R.A.S.

April 4th, 1865.—"On the Minimetric Method of Analysis," by Dr. R. Angus Smith, F.R.S., &c., President of the Society.

The printing of volume second of series third of the Society's Memoirs, comprising the papers read before the Society during the Sessions 1861-2, 1862-3, 1863-4, has been

completed, and bound copies are now ready for distribution at 5s. to members of the Society and 10s. to the public.

A new volume has already been commenced, containing papers read during the past session; and arrangements have been made by which papers intended for the Memoirs will be printed immediately after they have been passed by the Council.

The Librarian reports that when he took office he did it with the idea of preparing a Catalogue which had for some years been much required. On further examination he found it necessary both to rearrange the books, and to prepare a Stock-book; some delay was thus entailed. He is now, however, glad to inform the members that an alphabetical catalogue is in process of printing.

On the motion of Professor Clifton, seconded by Mr. Darbishire, the Annual Report was unanimously adopted.

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

President.

R. ANGUS SMITH, Ph.D., F.R.S., &c.

Vice-Presidents.

JAMES PRESCOTT JOULE, LL.D., F.R.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.
EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S.

Secretaries.

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

Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

Librarian.

THOMAS WINDSOR, M.R.C.S.

Other Members of the Committee.

REV. WILLIAM GASKELL, M.A.
PETER SPENCE, F.S.A., F.C.S.
GEORGE VENABLES VERNON, F.R.A.S., M.B.M.S.
ROBERT BELLAMY CLIFTON, M.A., F.R.A.S.
JOSEPH SIDEBOTHAM.
HENRY MERE ORMEROD.

ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER, FROM 31ST MARCH, 1864, TO 31ST MARCH, 1865.

Dr.

Date	Particulars		£ s. d.	£ s. d.
1864. April 1.	—To Balance in the Bank of Heywood Brothers and Co. ..			371 8 1
1865. Mar. 31.	—To *Members' Contributions:*			
	Arrears, 1863-4 .... 5..		10 10 0	
	,, half Subscription, 1864 .... 6..		6 6 0	
	,, Admission Fees, 1863-4 .... 6..		12 12 0	
	Members on the Roll, April 1, 1864, 193, at 42s.	£405 6 0		
	Deduct Compounders 3			
	,, Dead 9			
	,, In Arrear, March 31, 1865 11			
	23	48 6 0	357 0 0	
	8 Members elected in 1864, April—December		16 16 0	
	2 ,, 1865, half subscription.	£2 2 0		
	Deduct in arrear	1 1 0	1 1 0	
	Nine Admission Fees received 1864-65		18 18 0	423 3 0
,, ,,	To *Sale of Publications:*			
	Memoirs—Ser. 3, Vol. I.		1 1 0	
	Proceedings		0 4 6	
	Memoir of Dalton		0 2 6	
	For Books not returned to Library		1 2 6	2 10 6
,, ,,	To *Sundry Income:*			
	Section Contribution: Mathematical	£2 2 0		
	,, ,, Microscopical	2 2 0		
	,, ,, Photographic	2 2 0		
	Eight Associates of Microscopical Section ..	4 0 0		
	Ten Associates of Photographic Section	5 5 0	15 11 0	
	Late Photographic Society, Attendance, Coal, &c., at Meetings		5 0 0	
	Interest allowed by Bankers		18 7 0	38 18 0
				£835 19 7
	Library Fund		£86 7 0	
	Compositions		72 10 0	
	General Balance		201 7 3	
	Total Balance		£360 4 3	

Cr.

Date	Particulars		£ s. d.	£ s. d.
March 31, 1865.	By *Charges on Property:*			
	Chief Rent		12 11 7	
	Fire Insurance		7 17 6	
	Property Tax		4 5 0	24 14 1
	By *House Expenditure:*			
	Water, Gas, Candles, and Coal		21 19 11	
	Cleaning and Petty Expenses		3 13 3	
	Furniture, Bookcases, &c.		61 7 5	
	Repairs and Alterations		67 17 11	
	Tea and Coffee at Meetings		19 14 9	174 13 3
	By *Administrative Charges:*			
	Wages—Temporary Keeper of Rooms	3 10 0		
	Ditto —Assistant ditto	1 0 0		
	Ditto —Keeper of Rooms	38 5 0		
	Ditto —Assistant Librarian	21 0 0	63 15 0	
	Attendance on Sections		6 6 0	
	Postage and Parcels		18 0 0	
	Stationery, Printing Circulars, &c., and Receipt Stamps		18 12 4	106 13 4
	By *Publishing:*			
	Memoirs—Printing, Engraving, &c.		22 6 0	
	Printing Proceedings		48 15 0	
	Editor of Memoirs and Proceedings		50 0 0	121 1 0
	By *Library:*			
	Periodicals, Binding, &c.		18 13 2	
	Subscription to Ray Society		1 1 0	
	,, Cavendish Society		1 1 0	
	,, Palæontographical Society		1 1 0	21 16 2
	By *Sundry Expenditure:*			
	Advertising for Keeper of Rooms		0 4 6	
	Society's Medal		26 13 0	26 17 6
	Total Disbursements			475 15 4
	Balance in Bank of Heywood Brothers & Co.		335 0 3	
	,, Treasurer's hands		25 4 0	360 4 3
				£835 19 7

ROBERT WORTHINGTON, TREASURER,
1st April, 1865.

Examined and found correct, 21st April, 1865:

HENRY M. ORMEROD, } AUDITORS.
WM. BROCKBANK,

Dr. Joule communicated the following extracts of letters referring to the auroral arch of the 20th of March last:—

The Astronomer Royal to Alexander S. Herschel, Esq.,
Royal Observatory, Greenwich,
1865, March 31.

"I have referred to our Magnetic Photograms on the 20th inst. The magnets were considerably disturbed for many hours before and after the appearance of the arch; but the great disturbances of declination and hor. force were from 7h. to 8h. 45m., and those of vert. force nearly 45m. later. Of these you shall have copies when the time-scales are properly attached. There is scarcely a more important inquiry at the present time than that of the connexion between the *individual* phases of auroras, and the corresponding *individual* phases of magnetic disturbance. The first requisite is accurate clock time. By all means establish a reference to my Time Signals.

I have seen many auroras, probably more than 20, fewer than 50. I am quite familiar with the ordinary appearances: low arch, grey or red streamers rising to or beyond the magnetic zenith, great red clouds, light flashing over limited spaces (the successive portions of space taking the light in successive portions of time) and others; but I think I have seen the lofty well defined pale arch only twice. In some accounts of arches which I have read, two or three arches have followed in succession, all based in the magnetic E and W, and rising like a skipping-rope. As they approach very near the magnetic zenith, they exhibit a radiated structure  And, finally, it appears that an arch really is a very long slender parallelopiped, its length in magnetic E and W consisting *of beams in the direction of magnetic dip.* If you have not been possessed of this idea before, the possession of it may sharpen you in future observations or future inquiries."

Alexander S. Herschel, Esq., to R. P. Greg, Esq.

Collingwood, Hawkhurst, Kent,

1865, April 11.

"I was much obliged by the receipt of an observation of the auroral arch of March 20 (from the *Liverpool Mercury*), as seen at Windermere. Compared with another letter from the same writer (Mr. Hall) in the *Standard*, it gave a good place by the stars. In the 'Proceedings of the Manchester Literary and Philosophical Society' is another description by Mr. Baxendell. The arch was well observed here, almost stationary, varying its altitude from 18° to 20°, and at last to 22° or 23° in the course of half an hour. The first observations correspond with those of Windermere, at the time when the arch was nearly vertical over Windermere; and the last with those at Manchester when the arch, as seen by Mr. Baxendell, was nearly overhead at Manchester. Both give the height of the arch 102 miles above the earth, and I doubt if a better double observation of an arch of the aurora of the 'skipping-rope' form has ever been obtained. The result can hardly be more than five miles, certainly not so much as ten miles in error either way. Mr. Airy says that among twenty to fifty auroras that he has witnessed he has only twice seen the auroral arch, 'rising like a skipping-rope.' Perhaps you would have the goodness to communicate Mr. Airy's letter to Dr. Joule, as it will interest him in his observations of the magnetic variations; and at the same time to Mr. Baxendell."

The Rev. T. P. Kirkman, F.R.S., made the following communication:—

As the printing of my completion of the Theory of Groups is delayed longer than I expected, I desire to insert the following table of *corrections* to be made in the list of *titles* at page 142 of the Proceedings of this Society, July, 1863.

CORRIGENDA, p. 142, &c.

In the group of order 6·4·2, for 13_{2^3} read $7_{2^3} + 6_{41}$; in that of the order 5·2, erase 10_{41}; in the second title, page 144, for $4_{2^31^2}$ read 4_{42^2}; in the eighth title of that page, for $12_{42^2} + 8_8$ read $4_{42^2} + 16_8$; in the ninth title, p. 142, for 4_{2^4} read $4_{2^31^2}$; in the title 8·6·4·2, for $52_{2^31^2} + 24_{42^2}$ read $28_{2^31^2} + 36_{42^2} + 12_{41^4}$.

ADDENDA.

$4 = 1 + 3_{2^2}$, $Q = 1$.

$6\cdot 2 = 1 + 3_{2^21^2} + 8_{3^2}$, $Q = 15$.

$6\cdot 4 = 1 + 6_{41^2} + 3_{2^21^2} + 8_{3^2} + 6_{2^3}$, $Q = 15$.

$8\cdot 4 = 1 + 6_{2^21^4} + 13_{2^4} + 12_{4^2}$, $Q = 210$.

$8\cdot 4 = 1 + 6_{2^21^4} + 4_{4^2} + 5_{2^4} + 16_{42^2}$, $Q = 315$.

$8\cdot 4 = 1 + 6_{2^21^4} + 4_{4^2} + 5_{2^4} + 16_8$, $Q = 315$.

$8\cdot 4 = 1 + 8_{4^2} + 5_{2^4} + 2_{2^21^4} + 8_{2^31^2} + 8_8$, $Q = 630$.

$8\cdot 8 = 1 + 10_{2^21^4} + 8_{421^2} + 4_{4^2} + 9_{2^4} + 16_8 + 16_{42^2}$, $Q = 315$.

$8\cdot 8 = 1 + 4_{41^4} + 8_{2^31^2} + 6_{2^21^4} + 5_{2^4} + 20_{4^2} + 20_{42^2}$, $Q = 315$.

$8\cdot 6\cdot 4 = 1 + 6_{2^21^4} + 12_{4^2} + 13_{2^4} + 32_{3^21^2} + 32_{62} + 24_{2^31^2} +$
$12_{41^4} + 48_8 + 12_{42^2}$ $Q = 105$.

The omission of the above-given groups of 6·2 and 6·4 in my first processes was the main cause of the above errors and defects.

MICROSCOPICAL SECTION.

March 20th, 1865.

J. Sidebotham, Esq., President of the Section, in the Chair.

Mr. Dancer read a Paper, "On the Microscopical Appearances of Cotton Hair during dissolution in the Ammoniacal Solution of Copper."

Mr. Heys, in proposing a vote of thanks, said that he was gratified to find that the important subject of the structure of cotton, in which he had himself for some time felt an interest, was now being taken up in a manner likely to clear away all doubts.

Mr. Heys said he had been asked by Mr. Dale to introduce to the notice of members a preparation of Canada Balsam for mounting, which has the property of hardening in a very short time. It consisted of Balsam first made perfectly solid by evaporation, and then dissolved in Bisulphide of Carbon. He found the smell a strong objection to its use, but the results were very satisfactory, the balsam on a slide becoming perfectly hard in a few hours. He thought however the dry balsam might prove to be the more important element in the preparation, and that its solution in Chloroform would probably be found to answer all practical purposes.

Mr. Dancer exhibited many beautiful photographs of microscopic objects by Dr. Maddox.

March 27th, 1865.

John Parry, Esq., in the Chair.

Exhibitions.

Sections of various Shells:—Mr. Parry.

An apparatus for applying pressure to the cover-glasses of objects freshly mounted in Canada Balsam. It consisted of a dozen small upright pistons placed in a frame, and each furnished with a spiral spring coiled around the rod and pressing against the upper horizontal bar of the frame.

Communications.

Dr. Alcock exhibited a second time his specimens of shells of Marine Entomostraca, from the coast of Galway. He said that renewed examination of them and the collection of many more specimens had strengthened his belief that, however numerous, their forms, they are entitled to be considered as so many distinct species, and he did not think that the general arguments used by Prof. Williamson in support of an opposite opinion at the meeting of February 20th, had any application to their particular case. In the first place it was stated that the outer skin or shells of these creatures is of less value for distinction than the internal parts; but Dr. Baird had described nine out of his fifteen species from living specimens, and yet in these his specific distinctions are mainly derived from the shells. Again, we were reminded that some of the Entomostraca are known to undergo metamorphoses, and that this might probably be the case with the genera Cythere and Cythereis; but Cypris, which approaches very nearly in character to Cythere, does not undergo these changes, and might furnish some ground for the supposition that these Marine Entomostraca also do not; but it would be sufficient for the present purpose to state that there is no

support for the supposition that they do, on the general ground that some kinds of Entomostraca are known to undergo these changes, because it is also well known that other kinds do not.

But a further objection was that low forms of animal life are liable to extreme variability, and that the same remark is applicable to the lower groups in the higher divisions of animals, the argument being that in these cases the variability may be so great as to render specific distinctions of little or no value. He felt compelled to state that he did not admit this excessive variability as a certain fact, even in the lowest forms of animal life, until it could be actually proved to be true by most careful observation, and even then he would apply it no further than to the particular cases in which it had been proved; but as to making it a general rule with the lowest groups of the higher divisions of animals, he saw not the slightest reason for doing so, and here, more than in the former case, the diversity of character in the different groups would render it extremely unlikely that what might apply to one would also be found to apply to another. Even among low forms of animal life such as the Foraminifera, he had searched in vain for examples of that extreme variability which must necessarily confound specific distinctions, and he was sure that every young working naturalist, at all events, would agree with him that a conclusion so discouraging to exact observations as that suggested by Prof. Williamson, ought to be accepted by no one unless he became convinced of its truth by his own painful experience. With regard to the Foraminifera he had no hesitation in saying that, with only a few exceptions, the extraordinary uniformity of character in individuals of the same type was a fact that must strike every observer. In the cases of Miliolina and Polymorphina he would admit there is difficulty, but an examination of many of their numerous forms did not suggest to his mind that a great tendency to variation, in the ordinary

sense of the term, would explain their diversity. Knowing, however, from his own observations what is meant by the statement that the Foraminifera are liable to great variation, he was prepared to say that the shells of the Marine Entomostraca do not at all show this liability; the forms are clearly defined, and distinct from one another, and intermediate forms blending the characters of two others rarely if ever occur.

Then as to the last objection which was brought forward, namely the difficulty of defining what a species is, he believed that practically it might be put aside altogether; for the fact was that any creature whatever which could be shown to be clearly distinct from all others that had been described must be admitted as a species, and must remain a species until it could be proved to be unworthy of this distinction. In conclusion he would say that he had no strong opinion as to whether this question of what a species is could or could not be answered, but he felt sure that to take Prof. Williamson's suggestion, and merge some ten or twenty of these forms of Marine Entomostraca into one species would be to make it impossible to form even a conjecture of what is meant by that term.

A Paper was read "On the Chætopod Annelides of the Southport Sands," by BENJ. CARRINGTON, M.D.

AMPHINOMADÆ.

Aphrodite aculeata, L. Found sparingly near low-water mark; more frequent after storms.

Pholoë inornata, Johns. Very rare.

Polynoa squamata, Sav. Found occasionally within old shells; frequent among oysters, and the refuse from the fishing boats.

There are two well-marked varieties:

a. with a dark-brown crescentic mark on each of the elytra.

β. ochracea, uniform pale orange or stone colour.

Polynoa cirrata, Johns. Very rare.

P. asterinæ, sp. nov. Linear-oblong, scales twenty pairs or more, smooth, with a black entire border, seated on each third ring; intermediate feet cirriferous, bearing at the base a ciliated crest. Upper antennæ three, the central one longest.*

Not uncommon, occupying the groove between the suckers, of *Asterias aurantiaca*. I was first led to suspect the presence of some foreign species, by observing a blue phosphorescent light, given off from defined points of the rays, when the star-fish was placed in fresh water. It seems a very sluggish worm, and how it contrives to escape the surrounding suckers, and whether it shares the food captured by the star-fish, are points yet to be determined.

Body one to two inches long, by a line in breadth, posterior segments narrowed, ending in two filiform styles. Peach-blossom, or flesh coloured; very fragile, so that it is almost impossible to obtain an entire specimen. Scales white, chartaceous, with a narrow black border; first six pairs placed on alternate feet, the remainder on each third foot, not broader than the body, so that the feet are exposed, easily detached. In a line with the pedicels of the scales, on the intermediate rings, we find on each side a crest-shaped process, ending towards the mesian line in a short papilla. These are ciliated, as are the upper margins of the feet, so

* In describing a species, I have thought it best to follow the nomenclature now in use, although I agree with Professor Huxley that a change is desirable. He proposes, after Milne Edwards, that the rings shall be called *somites*, the head *prestomium*, central antenna *prestomial tentacle*, upper and lower lateral antennæ *superior* and *inferior prestomial cirri*, foot tubercle *parapodium*, its upper and lower rami *notopodium* and *neuropodium*, &c.

I think it right also to state, that as my acquaintance with Marine Zoology is very recent, and I have been unable to consult several foreign works on the subject, I introduce the following species with great diffidence. To the best of my belief they are new to the British fauna, nor have I been able to identify them with species described by *Aud.* and *Edwards*, *Oersted*, *Grube*, *Ehlers*, &c.

that horizontal currents are produced, as well as a central one from before backward. The dorsal papillæ seem to perform the functions of branchiæ. They also contain ova, which the ciliary currents serve to distribute.

Feet simple, with seven to ten strong spear-shaped golden setæ, apex toothed on one side. Near the dorsum of the foot is a small fascicle, containing four to six short curved toothed bristles.

Head concealed, roundish, emarginate. Upper antennæ three, the lateral ones very short, two jointed, central one much longer, equal to the two lower antennæ. Eyes four, distant. Tentacular cirri two pairs.

Readily distinguished by the long, flesh-coloured body, and marginate, smooth scales. In some young specimens, the black border is absent or ill-defined.

P. maculosa, sp. nov. Scales kidney-shaped, smooth, entire, membranous, having a dark curved spot round the centre, seated on alternate feet, intermediate feet bearing cirri; superior antennæ three equal; ventral surface of posterior rings, marked with four black dots.

Very rare; only one specimen found in company with *P. asterinæ*.

The specimen before me, which unfortunately has lost the anal segment, is oblong-obtuse, slightly narrowed from the middle, breadth two lines, by $\frac{3}{4}$ inch long. Scales twelve pairs, covering the head and feet, firmly attached, hyaline, especially near the border, which is slightly undulated, crossed from the inner margin by a retort-shaped black mark. Feet obtuse, obscurely biramous; upper branch much shorter, the setæ short, falcate, and serrate; lower branch bearing a tuft of twenty to thirty slender, half spear-shaped, oblique, pale setæ, toothed on one side, and ending in one or two larger teeth; shaft long, smooth, terete.

Head concealed, round, notched in front; eyes four, placed on the occipital portion; upper antennæ three equal, two

jointed, the central one stout apiculate; two lower antennæ much longer, and exceeding the tentacular cirri. When viewed from the ventral surface, the basal portion of the antennæ is nearly black, the three central ones converging like the rays of a tripod, of which the dark-coloured oblong oral opening forms the handle. Posterior segments after the fifteenth marked with four rows of stellate spots.

Distinguished from *P. asterinæ* by the larger thin translucent scales, which are firmly attached and not bordered, by the more numerous and slender setæ, the equal upper antennæ, and the absence of the ciliated processes on the dorsal surface.

There is no other British species with smooth scales with which it is likely to be confounded. From *P. spinifera*, Ehlers, (which seems to me identical with Johnston's *P. scabra*,) and from *P. pellucida*, Ehlers (Annelid. Chætop., t. iii. f. 1—13), it may be known by the scabrous cirri and antennæ of those species. *P. maculata*, Grube, seems to be a form of *P. cirrata*, having the scales garnished with a few large papillæ.

Sigalion Carringtonii, nov. sp. Body vermiform, obtuse at both ends. Scales very numerous, attached to each ring, pellucid, outer border fringed with pectinate glands; feet exposed, bifid, densely setiguous. Attached to the pedicel at the base of each foot is a curved ciliated cirrus.

Met with occasionally, near low water mark, on the Birkdale shore, buried in the moist sand, where it lies coiled in a spiral manner. First discovered in July, 1864, when exploring the sands with my friend *Mr. C. H. Brown*, who named it as above.

Body linear, obtuse in front, tapering very gradually towards the anal segment, which terminates abruptly in two long styles. Length two to three inches by two lines in breadth. Colour greyish-white, opalescent, reflecting prismatic tints. Feet very numerous, slender at the base, biramous, upper

branch gibbous at the apex, from which depends a short cirrus, furnished with a dense tuft of long silky setæ, fine as spun glass, which arch backward towards the scales, and a lower tuft of jointed ones ; inferior ramus, bearing two kinds of setæ, the lower very long and flexible, and the intermediate ones jointed, the blade filiform, rough, articulate like the hair of a mole.

Scales smooth, hyaline, persistent, convex, closely imbricated, not covering the feet, fringed at the outer margin with a few pectinate-pinnate processes. Some of the anterior scales are seated on alternate feet, but the majority arise from a large ovate tubercle, which is found at the base of each foot. These tubercles are filled with ova, and appear like a white opaque spot through the scales, and from their outer border a stout curved cirrus originates, clothed with vibratile cilia on the lower side, and extending a little beyond the scales.

Head small, concealed, hemispherical; eyes four, minute, the pairs approximate. Upper antennæ minute, two on each side, placed at the angles of a broad basal portion, which, like the feet, bears a tuft of silky setæ; lower antennæ much longer. The anterior feet exceed the head in length, and project beneath it almost to the median line, so that it is difficult to make out the exact details.

Proboscis as broad as long, compressed; the lips clothed with a row of simple fimbriæ; jaws four, alternate, pointed.

The worm is sluggish in confinement, generally remaining coiled spirally like a serpent. Under the lens it is a beautiful object, the long silky setæ spreading like the feathers of a bird of paradise—the daintily fringed, translucent scales, through which the ciliated tentacles are seen in constant motion—and the play of prismatic colours on the surface—are sure to excite wonder and admiration.

NEREIDÆ.

Phyllodòce lamelligera, Johns. Rare; at low water mark

buried beneath the sand. It is a beautiful species, swimming freely in sea-water.

P. Vittata, Ehlers, *Annelid. Chætopod.*, 1864, t. vi. f. 7—14. New to Britain. Two specimens appeared in water containing a mass of *Sabellaria alveolata* from New Brighton, May, 1864 *(C. H. Brown)*, and I have since met with one or two others at Southport.

Distinguished from *P. lamelligera* by its smaller size, never more than a line in breadth by two to three inches long, filiform, very active, generally assuming serpentine or spiral curves; pale olive, convex above, each ring crossed by a narrow, *stippled, steel-grey band, about a third of its breadth.* Head short, broadly ovate, obtuse; antennæ four apical, spindle-shaped; tentacular cirri four pairs, the two lowest very long (equal to five or six rings). Eyes large, black, crossed behind by the narrow first segment. Branchial leaflets ovate, reticulate, parallel with the body; lower branchiæ short obtuse, attached to the base of the feet. Anal segment bearing two leaf-like processes, resembling the branchiæ.

This active little worm resembles one figured by Sir G. Dalyell, but wants the central antenna.

P. attenuata, sp. nov.? Body very slender, from half a line to a line broad, four to six inches long; anterior rings as broad as long; branchial leaflet broadly ovate, seated on a pedicle as long as the feet, olive-brown veined; lower leaflet ovate acute; middle and posterior rings attenuated, *twice as long as broad;* branchiæ as wide as the segments over which they arch; feet small simple; bristles cultrate, curved, jointed, finely toothed at the base of the blade.

In the shape and relative size of the feet and branchial leaflets it agrees with *P. lamelligera*, but the disproportionally slender body, and oblong segments, distinguish it at a glance from any form of that species with which I am acquainted.

A solitary specimen and a portion of another are all that remain of this curious worm. Unfortunately, both head and

anal segment have been rendered indistinguishable by the action of the spirit.

P. Clava, sp. nov. Worm minute, one to two inches long by $\frac{1}{12}$in. to $\frac{1}{16}$in. broad; rings narrow, depressed, pale drab or greenish; feet simple, bearing on the upper side ovate *tumid* branchiæ, lying parallel to the body; near the base on the ventral side of the feet short obtuse lower branchial papillæ are attached. Rings gradually tapering, and ending in an obtuse anal segment, which bears two clavate solid styles, larger than the branchiæ. Head broad at the base, terminating in a thin ornithorhynchus-like snout, apex obtuse; antennæ four, short, divergent. First and second rings half as broad as the rest, giving attachment to four pairs of short tentacular cirri. I am doubtful about the the identity of this Annelid, as I have seen no figure of *P. clavigera, Aud. and Ed.*

Rare, occupying vertical burrows in the sand, about half tide mark.

Goniada Alcockiana, sp. nov. Body tapering at both ends, anterior third terete terminating in a conical horn-like snout, posterior segments depressed, broader, channelled above, ending in two long jointed styles. Eyes, and tentacular cirri 0. Proboscis very long, clavate curved, on each side of the base are eight Λ-shaped dentacles, mouth armed with seven jaws.

Body filiform, $1\frac{1}{2}$ inch long by a line wide, colour reddish brown; heteromorphous, anterior rings to the 45th, very convex, narrow; feet minute papillæform. Lower two-thirds of the worm depressed, rather broader; feet longer, oblique, from a dilated base, composed of four acute segments, the two outer shorter and divergent (branchial); each foot bears two fasciculi of bristles, the upper short curved, arising from the basal portion; lower composed of long, white, falcate jointed bristles. Apex of the conical snout bearing four minute antennæ, when the proboscis is exerted it stands up like a

small horn. Proboscis nearly as long as the terete portion of the body, curved, fluted above, armed with seven minute black jaws, five in the upper and two in the lower half; middle jaws larger tridentate. At each side of the basal portion there is also a row of minute inversely V-shaped black dentacles, eight in number.

Very rare, only one specimen collected. I have great pleasure in associating the name of Dr. Alcock, who has done so much for the spread of natural science in Manchester, with this curious species.

G. maculata, Johns; the only other British species is distinguished by its greater length, 4 to 6 inches, or, according to Oersted, 18 to 20 inches; whilst its extreme breadth is only a line and a half! This species, according to Johnson, is destitute of jaws, and Dr. Baird informs me from Oersted's figure there appear to be no anal cirri. It is distinguished also by having three brown maculæ on each segment. In *G. Norvegica,* Oers., there are eighteen dentacles on each side of the proboscis.

Glycera alba, Lam.

A single specimen only obtained among tufts of *Antennaria antennina.*

Pollicita peripatus, Johns.

Several specimens found at the base of *Alcyonium digitatum* brought from deep water after storms.

Scyllis prolifera, Müll.

Probably abundant in wet places, covered with a stratum of mud, but from its minute size easily overlooked.

Nereis pelagica, L.

N. viridis, L. (*N. cerulia,* Penn.) Both these species are abundant in wet hollows, about half-tide mark, occupying a deep burrow in the sand. They vary much in colour, from a deep velvetty green to orange. There is another form, with longer feet, bright orange or flesh-coloured, shaded with olive, which is frequent near high-water mark, where the tide is absent for months together, which may be distinct.

N. brevimana, Johns.

N. margaritacea, Leach.

N. Dumarillii, Aud. and Edw.

These species are found occasionally among oysters dredged from deep water, or the refuse from fishing boats.

Nereis bilineata, Johns.

Not uncommon. Always found occupying the terminal coils of old whelk shells, and generally those which have been taken possession of by *Pagurus Barnhardus*.

It is one of the handsomest of the Nereids.

Nepthys margaritacea, Sars.

N. Hombergii, Sav. Common in wet places, buried among the sand. Some specimens are six to eight inches long, and as thick as the little finger. Besides the above species there are several small ones, which I have not yet examined minutely, but which are probably new to Britain.

Ariciæ.

Nerine vulgaris, Johns.

I am doubtful whether my specimens belong to this species.

N. coniocephala, Johns.

Common in damp hollows about mid-tide, along with *Arenicola*. It occupies a friable tube, descending a foot or more below the surface.

Spio seticornis, Bast. }
S. crenaticornis, Mont. } (*Leucodore ciliatus*, Johns.) These seem to me to be forms of one species, sometimes excavating a burrow between the laminæ of old shells, at others constructing a sandy tube.

S. quadricornis, Lam. Very common below high-water mark, forming a slender cylindrical sand tube; it has four tentacles, the two lower shorter, and the anal segment terminates in four ovate styles. *Branchiarius quadrangularis*, Mont., seems identical with this worm, but, as

frequently happens, the specimen had lost the anterior segments.

Ophelia coarctata, M. Edwards.

One specimen only met with. New to Britain.

Mæa mirabilis, Johns.

Frequent near low-water mark, in wet places where the sand is intermingled with mud. It bears a close superficial resemblance to the smaller Nemertoid worms, Astemma, &c., and has the same white colour and elastic texture. Dr. Baird informs me there is one specimen in the British Museum, from the coast of Fife. Like myself, he failed to identify it with any known form, and I had named it provisionally *Rhynophylla bitentaculata*; but, since this paper was in type, he advises me that it is probably identical with the worm described in Dr. Johnston's Catalogue, at p. 278, as *Mæa mirabilis*.

As I have not been able to compare it with the description, and it may prove distinct, I append the notes I had drawn up from the examination of living specimens.

Prestomial segment leaf-like, ovate, broader than the body, strengthened in the centre by five ribs, ciliated below, margin mobile undulated reticulate. Proboscis cordate, retractile, tumid, shorter than the upper lip, from the lower margin of which spring two long flexible trigonous tentaculæ, clothed throughout the inner surface by four to six rows of conical papillæ, resembling the suckers of Asterinæ. Eyes and antennæ 0.

Its hold on the sand is so firm that specimens are seldom obtained entire. When creeping through the sand the thin mobile upper lip acts as a wedge, and the turbinate soft probosis is rapidly protruded like a bladder, enlarging the opening. When the surface is reached the head is partially withdrawn, and the two papillose tentacular cirri are directed upwards.

Body three to six inches in length, white, opalescent, as

thick as a crow quill; segments very numerous, quadrangular, slightly winged, as broad as long, the upper eight a little narrower, especially at the base, where it joins the lower portion. In addition to the small transparent branchial laminæ on the ventral surface of each of the upper segments, there is an oval appressed scale. Intestine simple, containing sand and mud. Numerous ova are found at the lateral margins of each ring after the eighth. The setæ of the eighth segment are very numerous and delicate, resembling in form the pendulum of a clock, while those of the lower rings are stronger, shaped like golf sticks.

This worm shows no disposition to swim in water, but remains in one place, with the leaf-like snout curved upwards. The blood is colourless or nearly so, and I could make out no circulation as in many worms, but the margin of the snout is covered with a delicate net-work of vessels, and the tentacular papillæ are each supplied with a vascular loop. These papillæ are depressed at the apex, and supplied with muscular fibres like true suckers, but I have never seen them used to seize any object, or aid in progression.

Arenicola piscatorum, Lam. Very abundant.

Lumbricinæ.

Lumbricus lineatus, Müll.

Very rare, among surface mud.

L. capitatus, Johns.

Only one specimen, constricted below the snout.

L. pellucidus, Temp. (Clitelis minutus, Temp.)

Found within putrid specimens of the heart urchin, and among other rejectamenta of the tide.

Capitibranchiata.

Pectinaria belgica. Lam.

Empty tubes common, living worms occasionally found near low-water mark.

Sabellaria Anglica, Grube, (*S. alveolata*, Sav.)

Very common within shells, especially the whelk.

S. Crassissima, Lam. Rare.

Terebella conchilega, Pall.

Frequent near low water.

T. chrysodon Mont.
T. nebulosa, Mont.
T. constrictor, Mont.
} Attached to shells, &c.—not rare.

I have found two specimens of a minute species with only eight tentacles, among tubes of *sabellaria,* perhaps the young of some larger form, they resemble *T. ostreata,* Dalyell.

Ops, gen. nov. (*One of the names of Cybele.*)

Tube slender, strong but flexible when moist, coated with minute, closely imbricated fragments of shell, attached edgewise.

Worm terete, of equal breadth throughout ($\frac{1}{20}$ inch). Branchial fans two, terminal, very short, composed of soft, thick, pectinated processes, the apices bifid, obtuse, incurved, surrounding the mouth like a star, not ciliated. Between the fans is a small scoop-like lip.

Rings distant, the upper one contracted at the apex, with lateral tufts of setæ, much shorter than the succeeding segments, which are six to eight times longer than broad.

Ventral surface channelled, on each side of it are pencils of slender setæ: and surrounding all but the ventral aspect of each ring, a *narrow rough band like a rasp,* the surface studded with conical papillæ. No lateral hooks.

Anal rings narrower, ending in an obtuse point.

O. digitata, sp. nov. Frequent opposite the Whitworth guns, near low tide mark, accompanying species of *Terebella.*

Tube three to four inches long, tapering a little at each end, which is open, as thick as a crow-quill. It has a very neat appearance from the uniform size of the shell fragments. Intestine simple, undulating, filled with mud and sand. Peristome not longer than broad, when closed conical, the

flat, obtuse, fleshy segments curling inwards over the mouth. Colour pinkish. There is a blood vessel on each side of the intestine, but I could trace no circulation in the pectinated fans, which seemed to be used by the worm to collect the grains of sand, &c. The rasp-like collars, surrounding each ring, are very curious.

There is an animal figured by Dalyell, *Powers of the Creator*, &c., Vol. II., Pl. xxxv. f. 4, 5, under the name of *clymene borealis*, which may be identical with our species, but the peristome is said to consist of 16 to 24 teeth, which are figured as simple and recurved, " forming a shallow funnel."

This Annelid is simple in structure and very sluggish, and it bears more resemblance to the *Sipunculidæ* than to the higher worms. In the peristome it reminds us of *Chirodota digitata*, Esch. From *Orthonia* it differs in the rasp-like band which surrounds each segment.

Sabella ventilabrum. Rare, cast up from deep water after storms.

Serpula triguetra, L. } Not uncommon on shells.
S. contortuplicata, L. }

Spinorbis communis, Flem. } Attached to sea-weed, sometimes abundant.
S. lucidus, Mont. }
S. minuta, Mont. }

Eccles, April 2, 1865.

April 24th, 1865.

Thomas Alcock, M.D., in the Chair.

Donations.

Captain Mitchell of Madras, three slides of Indian Diatomaceæ, and contents of seers fishes' stomachs almost entirely consisting of Diatomaceæ.

Dr. Alcock read a communication on " Southport Natural

History." He showed specimens of Tellina donacina, Modiola tulipa and Fissurella reticulata found on the shore, also of the large variety of the Solen ensis, some distorted shells of Mactra stultorum and Syndosmya alba. Fissurella reticulata had not hitherto been recorded as a Southport shell. He made some observations on the Natterjack Toads, which were spawning in marshy places among the sand hills, (April 18th to 22nd,) exhibiting males and females living, and spawn preserved in glycerine.

Mr. R. D. Darbishire mentioned the occurrence of the Old English or black rat in Messrs. Whitbread and Co's brewery in Chigwell street, London, and exhibited stuffed specimens of male, female, and young.

Mr. G. E. Hunt exhibited specimens of a Campylopus allied to C. setifolius, or possibly a new species.

Annual Meeting. May 15th, 1865.

Mr. J. Sidebotham, President, in the Chair.

The following officers for the ensuing session were duly elected : —

President

ARTHUR G. LATHAM.

Vice-Presidents.

JOSEPH SIDEBOTHAM.
R. D. DARBISHIRE, B.A., F.G.S.,
JOHN D. DANCER, F.R.A.S.

Treasurer.

THOMAS H. NEVILL.

Secretaries.

H. A. HURST.
THOMAS ALCOCK, M.D.

Of the Council.

JOSEPH BAXENDELL, F.R.A.S.
JOHN PARRY.
W. H. HEYS.
W. C. WILLIAMSON, F.R.S., &c.
J. G. LYNDE.
J. WATSON.
G. H. GRINDON.
THOMAS COWARD.

Donation.

Dr. Alcock presented to the Section 24 slides of mounted Foraminifera from shore sand, coast of Galway.

A fine stuffed specimen of Osprey, shot at Rostherne Mere, was exhibited by Mr. Harrison.

PHOTOGRAPHIC SECTION.

April 6th, 1865.

H. E. Roscoe, B.A., Ph.D., F.R.S., F.C.S., Vice-President of the Section, in the Chair.

Mr. J. Sidebotham said that two of the chief points in his paper, read on the 2nd February, were, that lenses either very short or very long in focus did not give true transcripts of nature; in the long focus the objects in the distance looking too near, and in the short focus too distant; and that lenses of from seven to ten inches focus gave correct perspective If large pictures were wanted they should be enlarged from negatives taken with a short focus lens. The second point was, that in all photographs there should be introduced some standard of measure, so that the focus of the lens being known, objects could be approximately measured.

Mr. J. B. Dancer remarked that in comparing pictures taken by short and long focal lenses for the purpose of show-

ing the errors of perspective, it would be desirable to take into account the angle included in the field of view, the diameter of the stop, and the errors arising from the aberration in the lenses employed. In order to obtain a view of photographic pictures which should approach as nearly as possible to the view seen by the eye, they should be looked at through lenses of corresponding foci to the lenses with which they were taken.

Mr. JAMES MUDD read a paper entitled "A Photographer's Dream," in which he pointed out that a good picture is not due solely to the process or the apparatus used, but that more was dependent on the careful study of the laws of art, without which it was impossible, except by accident, to produce an artistic picture.

Annual Meeting, May 18th, 1865.

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

The SECRETARY read the annual report, congratulating the members on the success of their first session. The various Papers read at the different meetings were referred to; the funds were also stated to be in a satisfactory condition.

The report was unanimously adopted.

The following gentlemen were elected to hold office during the ensuing year :—

President.

THE RIGHT REV. THE LORD BISHOP OF MANCHESTER.

Vice-Presidents.

J. P. JOULE, LL.D., F.R.S., &c.
H. E. ROSCOE, B.A., Ph.D., F.R.S., F.C.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.

THOMAS H. NEVILL.

Secretary.

LESLIE J. MONTEFIORE.

Council.

J. B. DANCER, F.R.A.S.
E. C. BUXTON.
JOHN PARRY.
JOHN ROGERSON.
JOSEPH SIDEBOTHAM.
W. C. WILLIAMSON, F.R.S., &c.

Mr. GEORGE WARDLEY read a paper on "Glass Transparencies," in the course of which he detailed the various processes generally in use for printing glass transparencies, dwelling especially on the modification of the taupenot, in which, after coating the plate with collodion, instead of immersing it in the sensitive bath, it is thoroughly washed with water to get rid of the ether and alcohol, and is then coated with iodised albumen; the remainder of the process is then exactly as the taupenot. The tannin process answered well, but many prints were spoiled in consequence of halation. The best preservatives for printing transparencies were a mixture of tannin and honey, or tannin and raspberry syrup, the former giving brown and the latter black tones. The addition of the raspberry syrup to the tannin yielded pictures entirely free from halation.

Dr. J. P. JOULE, F.R.S., described some further improvements in his camera, by means of which glass only came in contact with the silver solution.

The following paper was read at a meeting of the Microscopical Section, held November 28th, 1864—see page 52:—

"Notes on Natural History Specimens lately received from Connemara," by THOMAS ALCOCK, M.D.

The series of specimens which I have now to lay before you is so extensive, and I believe so interesting, that parts of it might properly form the material for several distinct communications; but at present I propose to show them as a

whole, and, with the specimens, to hand in as complete lists as I can of the species in each class.

The richness of the coast of Galway is well known to every student of British Marine Zoology; for to whatever branch of the subject he devotes himself he finds alike that here some of his rarest treasures are to be obtained. It is not with the hope of making known to you much that is new that I am led to introduce this subject to your notice, but chiefly because I am convinced that natural history work amongst ourselves is best promoted by the formation of exact lists of the species which we actually know to have been found at some particular localities; and such lists of Connemara specimens, imperfect as they must necessarily be at first, I have now to lay before you. It is however no more than might be expected that in the course of careful examinations of so many objects some points have occurred to me which I think worthy of notice, and these I shall mention as I come to them in their natural order.

In the first place I have to show specimens of three species of Nullipore—namely, N. polymorpha, N. calcarea, and N. fasciculata; also N. calcarea var. depressa.

The list of *Foraminifera* is an extensive one, especially considering that all my specimens are from shore sand and from one locality. This sand is from Dogs Bay, Roundstone, and consists of many kinds of small shells of Mollusca, among which Rissoæ and Lacunæ are most noticeable at first sight, fragments of Lepraliæ and other Zoophytes, spines of Amphidotus, and sponge spicula, while the finer parts are made up entirely of Foraminifera. Of these I have found 58 species and named varieties, and also six very distinct forms which are not mentioned in Professor Williamson's Monograph on Recent British Foraminifera. Specimens of these, and of all the other forms contained in the list are mounted for inspection.

In the course of my frequent examinations of these objects

I have made a few observations on several of them, which may perhaps be interesting.

I find Orbulina universa common in the Dogs Bay sand; that is, I have picked out some hundreds of specimens. They vary greatly in size, the largest being four or five times the diameter of the smallest. They have the surface frosted with larger and smaller tubercles, arranged with a certain kind of regularity, but, though thus rough externally, the texture of the shell does not appear to be arenaceous as stated by Professor Williamson,—at least, if by that term is meant that it is formed of agglutinated grains of sand as is the case with some other species. When examined with a high power and transmitted light, the larger and smaller tubercles show black from their density and the spaces between them are partly occupied by objects like very transparent thin plates, of a uniform size and an imperfectly squared figure, the impression these convey being that they have been produced by a kind of crystallization of the material of the shell at the time of its original formation. I conclude that the colourless condition of my specimens depends on the perfect manner in which all animal matter has disappeared, and I think for an examination of the mere structure of the outer case this must be an advantage. It may be interesting to note that among the specimens are a few with one or more protuberances of parts of their surface, destroying the regular spherical figure, and indicating an incipient budding before the shell hardened; there is also one large and very handsome double specimen.

Besides the Orbulinas I have an example of another kind of spherical object, which for convenience I will mention here, though I do not suppose it to belong to the Foraminifera at all. It looks like a sphere of the most transparent glass, and is without colour or markings of any kind.

I have found all the forms of Lagena, excepting L. vulgaris typica and L. gracilis. Lagena striata and interrupta are

abundant; and these, with very few exceptions, have the costæ passing forward to the extremity of the neck, in which case it is only one half of the whole number which do so, each alternate one stopping short at the base. Specimens where the costæ wind spirally around the neck are equally common with those in which they take a straight course. These Lagenæ have the appearance of old coarse shells, but they do not seem to have suffered from attrition; they are scarcely ever found with the neck broken short, though it may perhaps be almost equally rare to meet with one absolutely perfect. The varieties—clavata, perlucida, semistriata, and substriata, are comparatively rare, and all of them have forms and characters very distinct from striata and interrupta, while the two latter agree perfectly excepting in the matter of the costæ, which are found in different specimens to be interrupted in a great variety of ways, those with the costæ perfectly continuous being the least common; so that the conclusion I am inclined to come to is, that they need not be separated even as varieties, and that, whatever doubts may remain as to some of the other named varieties, the great abundance of these two and the constancy of their general characters make it certain that together they will form a good species under the name of Lagena striata. A few specimens of this species have a mucro at the base, and deformed ones are not uncommon; these, besides having the body variously mis-shapen, often have the neck bent, sometimes even so much as to give the specimen the form of a retort.

The Dogs Bay sand contains many forms of Entosolenia, some of them agreeing with those described by Professor Williamson, but others distinct; and of these latter I have ventured to name two, which may be described as follows:—

1. *Entosolenia Williamsoni*, a very abundant form, might pass at first sight for Lagena striata with the neck broken away, but a close examination shows it is a perfect shell, the body like L. striata but rather less full in proportion to its

length than is usual in Connemara specimens, and the texture a little more glassy; its chief peculiarity however is in the neck, which is short and formed of two distinct portions, the first directly continuous with the body and having an outline similar to that of the lower part of the neck of Lagena abruptly cut short, and the second a cylindrical tube of comparatively small diameter continued from the middle of it. The first portion is ornamented with three circles of hexagonal reticulations, which are continuous below by their inferior angles with the longitudinal costæ of the body, and present an interesting combination of the superficial characters of E. costata and E. squamosa.—2. *Entosolenia Montagui* is a squamous form, but differs from the named varieties of E. squamosa in having its surface really covered with a pattern like scales instead of with raised reticulations. Well developed specimens are not at all flattened, though many are found as if crushed, and they then present an appearance resembling a dried fig.; the true shape however is a perfect oval, full and well rounded at the smaller end, and from the middle of this projects a short smooth cylindrical tube. With a low power of the microscope, the whole surface of the body appears to be made up of small almost square facets arranged in distinct longitudinal rows, but when these are more highly magnified each flattened surface is seen to rise a little anteriorly, and to have the front border rounded so as to give exactly the appearance of a covering of scales.

So far as I have yet seen the forms of Dentalina and Cristellaria are very rare in this sand, Nonionina Jeffreysii and elegans are also scarce, but Patellina corrugata, which is described as a rare species, is not very uncommon, and some remarkably fine specimens have been met with. All the forms of Rotalina occur excepting two, and there are several undescribed ones in addition; at present I have seen only one specimen of the rare species, R. inflata. There are two distinct varieties of Globigerina, one with the

chambers globular, the other having them considerably flattened, which gives quite a different character to the shell. Truncatulina lobata is by far the most abundant species, and with Miliolina seminulum, constitutes the chief bulk of the the sand. The two forms of Cassidulina are equally common, and specimens have not been met with presenting intermediate links. Polymorphina lactea occurs in profusion, and, though the forms which are distinguished as typica, oblonga, and communis are well marked, a considerable proportion of the whole number of specimens collected seem to indicate an absence of any definite plan in the arrangement of the segments, the chambers being evidently thrown together without order, and in some cases producing an irregular nodulated mass, with two, three, or more distinct and perfectly formed open mouths on different parts of the surface. I find also specimens consisting of nothing more than the primordial segment, and these might be mistaken for a form of Entosolenia globosa but for the peculiar texture of the shell and the radiating grooves around the mouth; they are worthy, I think, of particular notice, as possibly capable of furnishing some more reliable marks of distinction than are found in adult shells, though at present all I have seen are of one character.

The forms of Textularia are numerous, and among them are four which can readily be separated, but may still pass for varieties of T. cuneiformis; one of them however differs considerably in having the texture of the shell much finer, and the chambers full and rounded. Textularia conica is abundant, and its character, in these Connemara specimens, is so distinct from T. cuneiformis that it seems impossible to admit it as only a variety of that species. In many of the specimens the apex of the cone is broken, exposing always three chambers, which are arranged like a trefoil and are placed almost on the same plane.

An examination of the specimens before you of the two

forms of Biloculina, named respectively in Professor Williamson's Work,—B. ringens, typica and B. ringens, var. carinata, will suggest, I think, a doubt as to whether it is correct to throw them together as one species, the texture of the shells as well as the form of their mouths being very different.

All the named varieties of Miliolina occur in abundance, and among them are great numbers of evidently distorted and misshapen specimens which appear to me to give no help whatever in the way of supplying inosculating forms, but may prove useful by indicating facts bearing on the general development of the animals. Specimens with the last chamber, not broken but clearly left incomplete, are by no means uncommon.

Lepraliæ.—The specimens of many species of Lepraliæ which I have to show are from dead shells picked up from the shore of Dogs Bay, and from others dredged at the mouth of Birterbuy Bay. These promise a very rich harvest, and indications of a great number of species are further given by the specimens of detached cells and fragments picked out from the shell-sand, though the latter are generally too broken to serve for exact examination or description. At present I have done very little with the specimens, but cannot altogether omit mentioning them. Lepralia figularis is common, and the specimens are very beautiful ones. There are also many examples of a species nearly agreeing, but not quite identical, with Johnston's Lepralia ovalis; and Lepralia ciliata var. β of Johnston, Mr. Hassall's Lepralia insignis, is plentiful. There are many other striking forms which I have not yet had the means to identify, besides common ones about which there is no doubt; but at present I must pass over the subject without attempting to give a list.

Echinodermata.—The present list includes ten species, and on the table are very handsome specimens of four of these, namely, Uraster glacialis and U. violacea, Luidia fragillissima and Echinus lividus. The acquisition of the two

species of Uraster just mentioned, which were not before in the Museum collection, led me to compare them with Uraster rubens, and see if some more reliable distinctions might not be found than the slight differences of proportion and form on which Forbes lays the most stress. The result of this examination is that the strong spines with which the bodies of all three are beset offer at once a clear and simple means of distinction, their character in each species being decidedly different.

The spine of Uraster rubens is club-shaped, the base spreading, and irregularly nodulated, the short shaft cylindrical and smooth, and the head barrel-shaped and beset with longitudinal rows of thin triangular projecting points. That of U. violacea has the basal part rising in the form of a short wide cylinder, crowned with an irregular circle of short spinules, from the midst of which the spine is continued in a conical shape, and near its summit is armed with rows of triangular projecting points like those of U. rubens. While that of U. glacialis has the form of a large strong cone with a very wide base rising from a dense circular bed of small blunt spinules. The apex of the cone appears truncated, and its sides near the top are marked with a few slight serrations.

Entomostraca.—In the shell-sand from which the Foraminifera were obtained great numbers of the cases of entomostraca occur. Specimens have been picked out of Cythere albo-maculata, angustata, variabilis, flavida, convexa, impressa, pellucida, and quadridentata, the last mentioned species being rather common; and besides these I have to show mounted specimens of about thirty forms perfectly distinct from one another and from those above mentioned. At present, however, from want of knowledge of what has been recently done in this subject, I am obliged to pass them over.

Crustacea. The collection of crustacea before you forms

in itself a very handsome result of Mr. R. D. Darbishire's one day's dredging in Birterbuy Bay, and among the specimens are some choice species, as Eurynome aspera, Ebalia Bryeri and Cranchii and Atelecyclus heterodon. These and indeed all the other specimens, form valuable additions to the Museum collection, which promises soon to become really useful for study and reference. The plan adopted of showing the specimens dried, mounted on glass and enclosed in glass-lidded boxes, has been found quite satisfactory, preserving them both from dust and damage by handling, while by properly displaying several specimens of a species in different positions every part may be clearly seen.

Mollusca.—The list of Mollusca which I have next to present contains 138 species, including those dredged in Birterbuy Bay; the others are all beach specimens, collected by Mr. Glover and Mr. Darbishire, or picked out since from the shell sand.

The fact that 16 species of small land shells have been found in considerable numbers in the sand, along with the sea shells and foraminifera, may be worth notice; more or less of such an admixture is, I believe, not uncommon, but in the present case the proportions appear to be unusual, and the very great abundance of Helix pulchella is certainly remarkable. It should be mentioned, too, that Mr. Glover has collected from the sandy strip of ground separating Dogs Bay from Gorteen Bay many dead bleached specimens of Helix nemoralis and a few of H. aspersa remarkable for the great thickness and weight of the shells, in consequence of a very excessive deposition of calcareous matter in their substance; no similar shells in the living state have been found in the neighbourhood, though carefully looked for.

As to the sea shells, I shall mention only a few that have particularly attracted my notice. Otina otis was found on a mass of small living mussels brought by Mr. Glover. Cæcum glabrum is very abundant in the sand, and I have picked out

some scores of specimens of this shell in the young state with the spiral nucleus attached, besides great numbers of separate spirals; the intermediate length of tube between the young and the adult state increases rapidly in size, and has a decidedly conical shape, so that the whole length of the shell, if it were to remain entire, would not be so great as might be supposed from seeing only the young and the mature portions. Skenea rota occurs sparingly; I have seen about half-a-dozen specimens. The minute shells which have proved perhaps the most interesting are those of the fry of different species of limpet, four forms of which have been met with; they are exceedingly abundant in the shell sand, and three of the species have a distinct spiral cap surmounting the apex of the conical part. The series of mounted specimens shows very clearly the several stages of growth to the adult form, and it will be seen that an internal partition is formed by degrees until at last it completely divides the interior of the cone from that the larval cap, which then drops off, leaving a depressed oval scar near the apex. *Patella pellucida* is at once distinguished by its brilliant greenish-blue marks; it is smooth and has a semi-transparent horn colour, much like that of the adult, though it gives little promise of the elegant shape it afterwards grows to; the larval cap is large and its spiral character distinct. A second form has so much the appearance of *Patella vulgata* that I shall venture to call it so. The cap is smaller than in P. pellucida, and the conical shell is opaque and strong, and has radiating ribs and markings of colour resembling the adult P. vulgata; in this shell the scar left when the cap has fallen is much deeper and coarser than in P. pellucida. A third form, which is much the most abundant in the sand, has a smooth, delicate, colourless shell, sometimes quite transparent, but often partly or wholly opaque; the cap is always white and has the appearance of a drop of wax on the apex of the cone. In this species the conical shell from its first com-

mencement appears to grow uniformly and regularly around the margin of the larval shell, and thus at once becomes a complete cone, while in P. pellucida the growth is at first entirely on one side, forming as it were a very large and expanded outer lip. I refer the present shell, though without positive proof, to *Acmæa virginea*. The fourth limpet-like shell is quite different from those already mentioned, it forms a more depressed cone, and is strong, with a translucent flinty texture, and the surface is marked with numerous fine, equal, radiating ribs. There is no deciduous cap, but the apex of the cone is a little flattened and presents a circular area, which is smooth and glassy, and appears as though a small transparent button were sunk into the substance of the shell.

The occurrence of the shells of fry of great numbers of species both of univalve and bivalve mollusca in the sample of sand from Dogs Bay which I am examining, has led me to attempt their identification with the view of obtaining information as to the characters of some of the larval, or more properly speaking fœtal, forms, which have not hitherto been observed. In this inquiry it is necessary to give a more definite meaning to the term "Fry" than has generally been done, for it appears to have been used indifferently for the fœtal shell as it comes from the egg, and for the young which has grown considerably beyond that condition. Shells of the latter kind still show the fœtal shell perfectly, and at the same time display characters in the aftergrowth by which at all events the genus, and in some cases the species may be determined, and it is specimens with these double characters that I shall distinguish as fry. At present I will leave the microscopic univalve shells without further notice, excepting to remark that, while as a rule all fœtal shells are smooth and glassy, there are among the univalves some striking exceptions, where the shell is beautifully sculptured, as in

some exquisite little specimens apparently the fry of *Murex erinaceus*. The whole shell (fig. 1) is of a transparent horn brown colour, and is covered with raised reticulations most gracefully disposed; those on the nucleus are longitudinal and transverse, enclosing minute square depressions; while on the succeeding whorls they are diagonal in opposite directions, and form diamond-shaped areolæ.

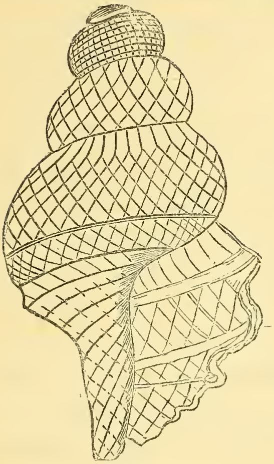


Fig. 1.

The bivalves have at present engaged most of my attention, and several of them will deserve a short notice. One of the most remarkable is a shell (fig. 2) which I refer to *Anomia*. It is transparent, and talclike in texture, and is very thin and delicate, so as to be almost always found more or less broken at the edges. Its substance is excavated by numerous tubes similar to those described by Dr. W. Carpenter in Anomia, and these tubes radiate from the edge of the nucleus in all directions to the margin of the shell. The nucleus itself is very glassy and transparent, and is full and rounded in shape, though still indistinctly triangular. But the singular feature in these specimens is that the growing shell, which completely surrounds the nucleus in the same way that it does in Ostrea, here leaves a small round hole on each side of the beak.—The fry of *Ostrea edulis* is common; the fœtal shell also occurs separate, and is readily distin-

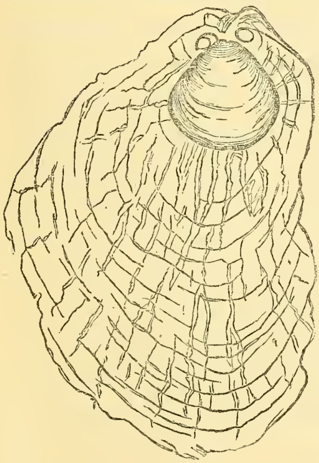


Fig. 2.

guished by having a notch about the middle of the ventral margin and by one of the valves being flatter than the other.—Shells having the form of *Pecten* are found in abundance, and about half a dozen kinds have been made out, though at present these cannot be referred to their species, the characters which distinguish them being of a different nature from those found in the adult shells; for, contrary to what is usually the case, the Pectens grow to a considerable microscopic size before the proper sculpture begins to be formed, and this intermediate growth resembles neither the fœtal shell nor the adult in structure. All the young Pectens which have been observed have a rounded shape like P. opercularis, and in all of them the nucleus is smooth, glassy, and transparent, with the valves ventricose and obscurely triangular, like those of Anomia. One form differs from all the others in having the intermediate growth entirely composed of cellular structure, which might lead to a suspicion that it is Avicula, but the general character is strictly that of Pecten. As might be expected from the conditions under which the specimens are found, none are double, and it may be worth mentioning that those with a cellular structure are flat, and, as far as has yet been observed, are all the same valve. The other forms are convex, and have the external surface pitted over with more or less numerous deeply excavated depressions of various forms, differently arranged so as to make so many distinct patterns.—*Lima subauriculata* is not uncommonly found of microscopic size, and it agrees with the characters given of the adult, excepting that the strong middle line is externally a furrow and internally a ridge, instead of the reverse. In this shell no distinct line of demarcation is seen between the fœtal shell and the aftergrowth. — *Crenella* presents an unusual case, as shown by the series of mounted specimens, in which the growth is seen to be continued to a considerable size on the plan of the original fœtal shell, and then suddenly changes to a totally different character, as has been already observed

by Mr. Jeffreys (*Brit. Conch*, vol. ii. p. 132.)—*Arca tetragona* shows a remarkable nucleus, which differs from almost all others I have seen in being amber-coloured or light brown, and it has, scattered over its surface, a few marks which appear like short deeply-implanted hairs. Two species of *Cardium* have been distinguished, namely C. edule and C. echinatum; both have the fœtal shell smooth, transparent, and globular, but that of C. echinatum is considerably the larger. Several species of *Venus* have been observed, and in all of them the fœtal shell is small, globular, and glassy. The fry of *Tapes* is characteristic, and the form of it which I have found appears to agree with T. virginea. I must pass over many other shells about which there is more or less doubt, and will at present mention only one more, namely *Saxicava rugosa*, which is the most abundant of all the species in the Dogs Bay sand. The nucleus is very large, glassy, and transparent, and resembles a Cyclas in shape. The fry of this species shows very well how completely the characters of the adult shell are dormant in the fœtal state, and how immediately they are sometimes assumed with the first growth beyond that condition.

In conclusion I have only to say that I trust the results of further examinations of this very rich shore deposit will justify me in having introduced it to your notice, although at present my observations are so slight and imperfect; and I will further venture to express the hope that the large collection of specimens before you may tempt other members of the Section to visit the coast of Galway and make additions to the subjoined lists.

LIST OF SPECIES FROM ROUNDSTONE.

1864.

Foraminifera.

Orbulina universa.	Globigerina bulloides.
Lagena vulgaris var. clavata.	Planorbulina vulgaris.
„ var. perlucida.	Truncatulina lobata.

Lagena vulgaris var. semistriata.
,, var. striata.
,, var. interrupta.
,, var. substriata.
Entosolenia globosa var. lineata.
,, costata.
,, marginata.
,, ,, var. lucida.
,, Williamsoni (N.S.)
,, squamosa, typica.
,, ,, var. scalariformis.
,, ,, var. catenulata.
,, ,, hexagona.
,, Montagui (N.S.)
Nodosaria radicula.
,, Pyrula.
Nonionina Barleeana.
,, Jeffreysii.
,, elegans.
Polystomella crispa.
,, umbilicatula.
Patellina corrugata.
Rotalina Beccarii.
,, inflata.
,, oblonga.
,, concamerata.
,, mamilla.
,, nitida.

Bulimina pupoides, typica.
,, ,, var. marginata.
Uvigerina angulosa.
Cassidulina lævigata.
,, obtusa.
Polymorphina lactea, typica.
,, ,, oblonga.
,, ,, communis.
,, myristiformis.
Textularia cuneiformis, typica.
,, ,, var. conica.
,, variabilis.
,, ,, var. spathulata.
,, ,, var. difformis.
Biloculina ringens, typica.
,, ,, var. carinata.
Spiroloculina depressa, typica.
,, ,, var. rotundata.
,, ,, var. cymbium.
Miliolina trigonula.
,, seminulum.
,, ,, var. oblonga.
,, ,, var. disciformis.
,, bicornis, typica.
,, ,, var. angulata.
Spirilina foliacea.
,, perforata.

Echinodermata.

Ophiura texturata.
Ophiocoma rosula.
Uraster glacialis.
,, violacea.
,, rubens
Asterina gibbosa.

Luidia fragillissima.
Echinus lividus.
Echinocyamus pusillus (in shell sand)
Amphidotus (spines in shell sand)

Crustacea.

Stenorhynchus phalangium.
Inachus Dorsettensis.
Hyas coarctatus.
Eurynome aspera.
Zantho florida.

Ebalia Pennantii.
,, Bryeri.
,, Cranchii.
Atelecyclus heterodon.
Pagurus Bernhardus.

Zantho rivulosa.
Cancer pagurus.
Carcinus mænas.
Portunus puber.
,, corrugatus.
,, depurator.
,, pusillus.
Gonoplax angulata.
Pagurus Prideauxii.
,, Cuanensis.
,, lævis.
Porcellana platycheles.
,, longicornis.
Galathea squamifera.
Crangon vulgaris.
Palæmon serratus.
Ligia oceanica.

Mollusca.

Vitrina pellucida.
Zonites cellarius.
,, crystallinus.
Helix ericetorum.
,, lamellata.
,, fulva.
,, pulchella.
,, rotundata.
Pupa umbilicata.
,, muscorum.
,, Venetzii.
Clausilia nigricans.
Zua lubrica.
Limnæus truncatulus.
Conovulus bidentatus.
Carychium minimum.
Murex erinaceus.
Nassa reticulata.
,, incrassata.
,, pygmæa.
Purpura lapillus.
Mangelia nebula.
,, costata.
,, rufa.
,, linearis.
,, attenuata.
,, turricula.
Cypræa Europæa.
Lamellaria perspicua.
Otina otis.
Cerithiopsis tubercularis.
Trochus cinerarius.
,, magus.
,, Montagui.
,, umbilicatus.
,, zizyphinus.
Phasianella pullus.
Ianthina exigua.
,, communis.
Puncturella Noachina.
Fissurella reticulata.
Acmæa virginea.
Patella vulgata.
,, athletica.
,, pellucida.
Dentalium entale.
Cylichna cylindracea.
,, truncata.
Pleurobranchus plumula.
Spirialis Flemingii.
Anomia aculeata.
,, ephippium.
Pecten maximus.
,, opercularis.
,, varius.
Lima Loscombii.
,, subauriculata.
Mytilus edulis
Arca tetragona.
Pectunculus glycimeris.
Nucula nucleus.
Cardium echinatum.

Chemnitzia elegantissima.
Eulima distorta.
Cerithium reticulatum.
,, adversum.
Turritella communis.
Cœcum glabrum.
Scalaria communis.
Skenea planorbis.
,, rota.
Rissoa cingillus.
,, labiosa.
,, inconspicua.
,, parva.
,, punctura.
,, rubra.
,, striata.
,, ulvæ.
Lacuna vincta.
,, puteolus.
,, pallidula.
Litorina litoralis.
,, litorea.
,, neritoides.
,, rudis.
,, saxatilis.

Cardium fasciatum.
,, Norvegicum.
,, pygmæum.
Lucina borealis.
Kellia suborbicularis.
Turtonia minuta.
Montacuta ferruginosa.
Venus casina.
,, fasciata.
,, ovata.
,, striatula.
,, verrucosa.
Artemis exoleta.
,, lincta.
Tapes aurea.
,, virginea.
Lutraria elliptica.
Tellina donacina.
Syndosmya alba.
Saxicava rugosa.
,, ,, var. arctica.
Lyonsia Norvegica.

ADDITIONAL LIST OF DREDGED SHELLS.

Gastrochœna modiolina.
Thracia convexa (valve.)
,, distorta (living.)
Solen siliqua.
Solecurtus candidus.
Psammobia vespertina.
,, ferroënsis.
Tellina crassa.
,, tenuis.
,, solidula.
Mactra solida.
Tapes decussata.
Circe minima (living.)
Lucina flexuosa.

Modiola modiolus.
Crenella marmorata.
Arca tetragona (living.)
Anomia patelliformis.
Chiton discrepans.
,, asellus.
,, cinerarius.
,, cancellatus.
Emarginula reticulata.
Trochus lineatus.
Rissoa Beanii.
Buccinum undatum.
Philine aperta.
,, catena.

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

VOL. V.

SESSION 1865-66.

MANCHESTER:

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

LONDON: H. BAILLIÈRE, 219, REGENT STREET.

1866.

26,855

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 3rd, 1865.

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

Dr. Roscoe exhibited an enlarged photograph of the moon, 21 inches in diameter, taken by Mr. L. M. Rutherford, of New York, on the 6th of March last. Mr. Brothers, Mr. Vernon, and other members, pronounced it to be the best lunar photograph they had seen, and decidedly superior to any yet produced in this country.

Mr. Dyer, referring to the breaking of the Atlantic Cable, expressed his surprise that no apparatus had been provided to seize and secure the end of the cable when the rupture took place, as contrivances for a similar purpose were in use in almost every cotton mill.

A paper was read "On the Internal Heat of the Earth as a Source of Motive Power," by Mr. George Greaves, M.R.C.S.

It has been very generally admitted that coal will not cease to be furnished because of the exhaustion of the stores of the mineral now existing in the coal measures; and further, that the obstacles to the continued working of the mines will not be engineering difficulties. The increased depth from which the coal will have to be brought may add to the cost, but at that increased cost it will still be for a long time obtainable. The author considered the real insurmountable obstacle to be the high temperature of the lower portions of the carboniferous strata. That temperature had been shown to be at a depth of 4,000 feet at least 120° Fahr., a degree of heat in which human beings cannot exist for any length of time, much less use any exertion. It had occurred to the author to inquire whether the very agency which will prevent the continued supply of fossil fuel might not be made the means of rendering that supply unnecessary—whether, in short, the internal heat of the earth might not to some extent be utilised. One or two modes of doing this had presented themselves to his mind. One of these might, he conceived, be the direct production of steam power by bringing a supply of water from the surface in contact with the heated strata by means of artesian borings or otherwise.

Mr. W. L. Dickinson read the following note on the eclipse of the sun which will take place on the 19th instant:

Calculation for Manchester (Royal Infirmary), Lat. N. 53° 29′, Long. W. 2° 14′.

At Manchester a partial eclipse is partly visible, and

	h.	m.	s.
Begins ....October 19th—	4	8	12
Greatest phase ...October 19th—	5	4	2

Greenwich mean time.

At Manchester the sun will set at 5h. 2m. Greenwich mean time.

Magnitude of the eclipse (sun's diameter = 1) 0·293, on the sun's southern limb.

Angle, from north pole, of first contact 120° } towards the
Angle, from vertex, of first contact 153° } west
for *direct* image.

The situation of the point of first contact may be familiarly illustrated in the following manner. If we suppose a Victoria shilling to represent the sun, the moon will appear first to touch it on the right side near the bottom, at the letter D.

PHOTOGRAPHICAL SECTION.

October 5th, 1865.

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

Messrs. Coote and Rogerson exhibited a series of very fine pantascopic photographs of scenes in Switzerland, taken by M. Adolphe Braun, of Dornach. Each picture was about twenty-one inches in length, and the angle of view was stated to be one hundred and twenty degrees.

Mr. A. Brothers, F.R.A.S., exhibited an interesting series of photographs, taken during the eclipse of the moon, on the evening of Wednesday, October 4th. Commencing at 8.45, when the moon was nearly full, the negatives, twenty in number, were taken at intervals of about 12 minutes until 12.45, and they show the progress of the eclipse throughout. The effect of the penumbral shadow of the earth is distinctly visible on the negative taken at 9.15, and also on the one taken at 12.32. An attempt was made during the middle of the eclipse to obtain the photographic image of the entire surface of the moon; but it was found that the portion

covered by the earth's shadow had no effect on the plate after an exposure of 15 seconds, although distinctly visible in the telescope. It was noticed that the southern limb of the moon showed the copper-coloured tint often seen during total lunar eclipses, and to this cause may be attributed the non-actinic effect on the sensitized plate. An exposure of about one or two tenths of a second gave the fully illuminated surface of the moon perfectly, but the parts covered by the penumbra were not defined, while an exposure of three seconds gave the outline of the earth's shadow with great distinctness, and an exposure of two seconds brought out some of the detail within the penumbra. Some of the negatives were obtained almost instantaneously.

The telescope with which these pictures of the moon were taken is an equatorial of 5 inches aperture and 6 feet focal length, driven by clockwork. This telescope gives the image of the moon about $\frac{11}{16}$ths of an inch in diameter, but by using a Barlow's lens this size is increased to $1\frac{1}{4}$ inch, and with this addition the eighteenth negative of the series was obtained in two seconds.

Dr. Joule, F.R.S., exhibited and explained the construction of a camera which he had contrived for outdoor work without a tent.

In this camera the operation was carried on by the successive introduction of the sensitizing and developing baths, the mode of the application of the baths being similar to that already described by the author. By a special arrangement the holders of the plate are preserved from contact with the developing solution.

Ordinary Meeting, October 17th, 1865.

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

Mr. Forrest stated that he had in his possession a very extensive collection of Shakespeare memorials, which he would gladly allow any member of the Society to inspect.

A paper was read entitled "Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes," by J. C. Dyer, V.P.

1*st.*—*Lace Making Machine.*

The bobbin net trade at Nottingham had been carried on by women working on cushions or lace frames until about the beginning of this century, when the process was superseded by the lace making machine, invented by the late Mr. John Heathcoat, M.P. Mr. Heathcoat commenced his experiments by stretching common packing threads across his room for the *warp*, and then passing, by common plyers, the weft threads between the cords, delivering them into other plyers on the opposite side, and then, after giving them a sideways motion, repassing the threads back between the next adjoining cords, and thus effecting the intersecting or tying of the meshes in the same way as they were formed on the cushions worked by hand. His next step was to provide thin metallic discs to be used as "bobbins" for conducting the thread back and forth through the warp. These discs being arranged in carrier frames, placed on each side of the

warps, were moved by suitable machinery so as to conduct the threads from side to side to form the lace. The limits of this abstract do not allow any detailed description of Mr. Heathcoat's beautiful invention; its effect, however, in superseding the hand work appears to have led to the Nottingham riots and the lace frame breaking, which took place about fifty years ago, when Mr. Heathcoat removed to Tiverton in Devonshire, where the patent lace making was re-established, and has been since carried on upon a large scale, and has thus proved an eminently successful invention.

The new principle of action conceived by Mr. Heathcoat was that of passing the threads of the weft through those of the warp and delivering them into conductors on the other side, to be repassed and delivered into the former conductors under mechanical control in place of hand working. He succeeded in working out this principle with marvellous perseverance and success, and this original conception of Mr. Heathcoat opened a new vista to other eminent mechanics, which led to the invention of many other valuable machines for widely different purposes, among which may be named that for passing back and forth the threads through fabrics for embroidering, as now exhibited in the beautiful machines for embroidering or ornamenting fabrics in the works of Messrs. Houldsworth. On the same principle is also based the cotton combing process now employed for separating the short and coarse from the long staples in fine cotton carding.

2nd.—*Wire Card Making Machine.*

About the beginning of this century Mr. Amos Whitimore, of Boston, commenced his experiments for making cards by machinery. His first step was to examine the movements required to form and set card teeth for hand carding. He was for a long time engaged with trial machines, and ultimately succeeded in performing the operations for making

and setting the card teeth by movements effected by eccentrics on a driving shaft, viz. (1) feeding the wire, (2) holding it, (3) cutting off and (4) bending the wires into staples, (5) piercing holes in the leather, (6) passing the staples through it, (7) pressing their crowns to the sheet, (8) crooking the teeth to the knee bend, and (9) advancing the leather to receive the next row of teeth. These complex and curious motions were effected by a series of cams or eccentric pieces fixed on the shaft and turned by a winch; therefore the invention of making wire cards by machinery was accomplished by Amos Whitimore, but it was many years thereafter before his machine could be made profitable in competing with the hand card makers.

The final development of this invention is explained at large in the paper, but cannot be given in this abstract. In this machine, as in that of Mr. Heathcoat's, a new principle of action was adopted to produce and govern the movements for making wire cards, viz., that of eccentric curves revolving on a driving shaft and guiding the motions of the traversing parts of the machines in their due order of succession for making and setting the card teeth as before stated. This application of curvilinear projections or cam pieces has since been extensively employed for giving intricate motions in many other machines invented during the last fifty years. Whence it appears that both Mr. Heathcoat and Mr. Whitimore became pioneers and guides to other able mechanicians in their several labours for the advance of mechanical science.

Among the inventions based upon that of the carding machine may be mentioned the machine for making the eyes or shanks of metal buttons, the machine for making wire reeds for weaving, and that for making pins. But without dwelling on other instances, it will suffice to say that the success of the lace machine and that of the wire card machine serve to spread the seeds of knowledge in practical mechanics, the germs of which, half a century ago, were

widely cast forth from the fertile geniuses of John Heathcoat and Amos Whitimore.

3rd.—Cutting Furs from Pelt.

In the year 1810 a model fur cutting machine was sent to me in London by a company in Boston, to be patented in England. It was stated to be the invention of a Mr. Bellows, who was unknown to me. The machine was adapted for shearing fibres from surfaces by the action of spiral cutters revolving and acting against a fixed straight cutter, so as to shear or cut fibres from the surfaces to which they are attached. I had a machine made and put into operation at a hat manufactory in the Borough; but the workpeople opposed its being used, which discouraged further attempts to bring it into use in that trade. The principle of it, however, was soon after patented for chopping straw, roots, &c., for which it was found valuable. Two or three patents were afterwards taken out for shearing the nap from cloth by the same action of spiral cutters revolving against a straight fixed edge, and many others have since appeared on the same principle, among which is that for mowing lawns.

PHYSICAL AND MATHEMATICAL SECTION.

October 12th, 1865.

ROBERT WORTHINGTON, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. DANCER, F.R.A.S., exhibited a small and cheap, but very effective induction coil, and a set of four Geissler's tubes, in which the stratification of the electrical light was very distinctly shown when a small battery of only one pair of elements was employed to produce the primary current.

Mr. BROTHERS, F.R.A.S., exhibited a beautiful series of enlarged photographs of the moon from negatives taken during the progress of the lunar eclipse on the night of the 4th instant (see *ante*, page 3).

Mr. DANCER stated, with reference to the eclipse, that he and his son, Mr. James Dancer, had noticed some irregularities on the border of the earth's shadow which, as they maintained their forms and relative positions whilst the shadow passed over the moon's disc, could not, he thought, be due to differences in the reflective power of different portions of the moon's surface.

Mr. BAXENDELL suggested that these irregularities might be owing to the prevalence of extended masses of clouds in certain portions of the earth's atmosphere and their absence in others.

The following table of rainfall at Eccles for 1864, was communicated to the Section by G. V. VERNON, F.R.A.S., at a meeting held April 13th, 1865 :—

RAINFALL AT ECCLES, NEAR MANCHESTER, FOR 1864.

By Thomas Mackereth, F.R.A.S., M.B.M.S.

Rain gauge 3 feet above the ground, 112 feet above the sea.

Days of Rain Fall, 1864.	Fall of Rain in inches for 1864.	Average Fall of Rain for 4 years at Eccles.	Average Fall of Rain for 71 years at Manchester	Difference from 4 years' fall at Eccles.	Difference from 71 years' fall at Manchester	
15	1·725	2·196	2·460	— ·471	— ·735	January.
13	3·160	1·887	2·379	+1·273	+ ·781	February.
15	2·375	3·144	2·310	— ·769	+ ·065	March.
12	1·722	1·977	2·038	— ·255	— ·316	April.
11	2·982	2·325	2·351	+ ·657	+ ·631	May.
20	2·878	3·390	2·913	— ·512	— ·035	June.
10	2·280	2·917	3·569	— ·637	—1·289	July.
15	2·462	2·959	3·592	— ·497	—1·130	August.
21	4·075	5·311	3·235	—1·236	+ ·840	September.
13	1·910	3·382	3·818	—1·472	—1·908	October.
18	3·299	3·104	3·473	+ ·195	— ·174	November.
17	2·006	2·815	3·260	— ·809	—1·254	December.
180	30·874	35·407	35·398	—4·533	—4·524	Total.
Jan. Feb. Mar.	7·260	7·227	7·149	+ ·033	+ ·111	
April May June	7·582	7·692	7·302	— ·110	+ ·280	
July Aug. Sept.	8·817	11·187	10·396	—2·370	—1·579	
Oct. Nov. Dec.	7·215	9·301	10.551	—2·086	—3·336	

The above table shows that the rainfall of 1864 was 4½ inches below the average fall at Manchester for 71 years, and the average fall at Eccles for four years. Both averages appear to show that the largest rainfall, in this district, takes place from July to December, which is also, to some extent, confirmed by the rainfall of last year, though there was so great a deficiency in the summer and autumn months.

Ordinary Meeting, October 31st, 1865.

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

The following communication from Sir J. F. W. Herschel, Bart., M.A., D.C.L., F.R.S., &c., Honorary Member of the Society, was read by Mr. Baxendell:—

Collingwood, October 18, 1865.

In the printed proceedings of the ordinary meeting of the society on the 3rd inst., I observe a notice of a paper by Mr. Greaves, "On the Internal Heat of the Earth as a Motive Power," in which the high temperature of the carboniferous strata, at the depth of 4,000 feet (120° Fahr.) is spoken of as likely to oppose an insuperable obstacle to the extraction of coal from that depth. On reading this it occurred to me that by employing condensed air, conveyed through conducting pipes, as a mode of working machinery at that depth—provided the air immediately on its condensation, and before its introduction into the pit, were drained of the heat developed in the act of condensation by leading it, in pipes exposing a large external surface, through a sufficiently large supply of cold water (or in winter time of snow) — the workings below might be sufficiently reduced in temperature by the re-expansion of the air on its escape, when given out below in the act of working the machinery, to admit of workmen remaining there in comfort; at the same time that ventilation would be supplied.

If you think that this suggestion would be worthy the notice of the author of the paper referred to, or of those members of the society who may have been present at its reading, or in

any other way available, it is quite at your service for that purpose.

P.S.—Water at 120° Fahr., or even much higher, would, I fear, afford but an inefficient moving power unless some means could be devised (without the expense of more power than the gain expected) of concentrating the heat of a large quantity of warm water into a smaller. This might perhaps be done through the intervention of air alternately rarefied and condensed.

Mr. Binney, F.R.S., F.G.S., said that at the present time little is known as to the difficulties we should experience in working coal mines at a depth of 4,000 feet from the surface. The exact increase of temperature in deep mines is not by any means well ascertained. All we can say is that no great difficulties have been found in working at a depth of 2,100 feet. It must always be borne in mind that the deeper a mine is the greater will be the natural ventilation, that is the current caused by the air of the mine, at say a temperature of 80° Fahr., ascending the upcast shaft, while the air at the surface, of 40°, descends by the downcast shaft. No doubt a mine might be cooled by the expansion of compressed air, but it could not, so far as at present known, be done economically. In most deep mines a considerable cooling of the air takes place by the expansion of the compressed gas (light carburetted hydrogen) as it escapes from the coal, where it has been long imprisoned under great pressure; and this has not always been allowed for by observers of temperature in such places. In newly-opened mines this pent-up gas forces off large pieces from the face of the coal, and it sometimes makes a noise like water rushing over a weir. In sinking a deep shaft at Wigan some years since the compressed gas in the coal forced up about four yards of strong bind and made its way through it into the shaft. The rising of the roof of the coal as the shaft approaches it is well known to sinkers in deep and newly-opened coal fields.

Mr. Edward Hull, F.G.S., exhibited some etchings of caves, fissures, and isolated rocks on the coast of Cantyre, intended to illustrate three classes of phenomena belonging to the raised beach and coast, known as "the 30-feet beach," from the fact that its mean elevation is about 30 feet above the present tides. This raised beach has been described by several authors, from Mr. Smith, of Jordan Hill (1836), downwards, and is part of the same beach which has been traced all along the western coast of Scotland, and the vestiges of which remain in a state of remarkable freshness to the present day.

Mr. T. Heelis, F.R.A.S., called attention to the proceedings of a scientific commission recently issued by his Highness the Viceroy of Egypt, who had succeeded in finding a tertiary coal basin in the valleys between Mount Olympus and the Bay of Oraniska, in the Gulf of Salonica, and also on the mainland of Asia Minor, near the Island of Samos; and mentioned some particulars of the coal there found, such as its specific gravity, in which it slightly exceeds the ordinary coal of the coal measures, and the results of experiments upon its combustion, which give twenty per cent of ash.

A paper "On Coresolvents." by the Honourable Chief Justice Cockle, M.A., &c., President of the Queensland Philosophical Society, was communicated by the Rev. Robert Harley, F.R.S., &c.

Whatever function X may be of x equation (3) of my last paper (*Proceedings*, vol. IV., pp. 37—40), is reducible to an equation with constant coefficients by changing the independent variable as indicated at p. 38 (*ibid.*), and the order of the symbolical factors of (3) is always indifferent.

Mr. Harley gives

$$3^2 x(2-x)\frac{d^2y}{dx^2} + 3^2(1-x)\frac{dy}{dx} + y = 1 \ . \ . \ . \ (\text{i})$$

as the differential resolvent of

$$y^3 - 3y^2 + 2x = 0 \;\ldots\ldots\; \text{(ii)}$$

The sinister of this resolvent is of the form of the sinister of (3) of my last paper, and the symbolical decomposition of (i) of this paper is (i denoting as usual $\pm\sqrt{-1}$)

$$\left(3\sqrt{x(2-x)}\frac{d}{dx}+i\right)\left(3\sqrt{x(2-x)}\frac{d}{dx}-i\right)y-1 \;\ldots\ldots\; \text{(iii)}$$

and the resolvent of this form of cubic, like that of the other, is soluble by change of the independent variable.

The evanescence of the dexter of a resolvent does not necessarily indicate that all its algebraic coresolvents are wanting in their second term. Thus, for instance,

$$3^2(1-x^2)\frac{d^2y}{dx^2} - 3^2x\frac{dy}{dx} + y = 0 \;\ldots\ldots\; \text{(iv)}$$

is the differential resolvent not only of

$$y^3 - 3y + 2x = 0 \;\ldots\ldots\; \text{(v)}$$

but of every algebraical equation whose roots are homogeneous linear functions of the roots of (v), that is to say of any two of them, for any root of (v) is such a function of the other two. Let α, β, and γ be roots of an equation whereof any one root is a linear and homogeneous function of any two other roots. Then we may put

$$\gamma = m\alpha + n\beta \;\ldots\ldots\; \text{(vi)}$$

and, from the identities

$$\alpha\left(m\frac{d\alpha}{dx} + n\frac{d\beta}{dx}\right) - \left(m\alpha + n\beta\right)\frac{d\alpha}{dx} = n\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right)$$

and

$$\left(m\alpha + n\beta\right)\frac{d\beta}{dx} - \left(m\frac{d\alpha}{dx} + n\frac{d\beta}{dx}\right)\beta = m\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right),$$

we infer that

$$\alpha\frac{d\gamma}{dx} - \gamma\frac{d\alpha}{dx} = n\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right)$$

and

$$\gamma\frac{d\beta}{dx} - \beta\frac{d\gamma}{dx} = m\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right);$$

and if $m=n=-1$, we have

$$\alpha\frac{d\beta}{dx}-\beta\frac{d\alpha}{dx}=\beta\frac{d\gamma}{dx}-\gamma\frac{d\beta}{dx}=\gamma\frac{d\alpha}{dx}-\alpha\frac{d\gamma}{dx},$$

relations which must be satisfied by the roots of (v) or of any cubic whereof the coefficient of the second term vanishes. (Compare art. 73 of my "Notes," &c., in the *Messenger of Mathematics* for November, 1864.) We have, indeed, generally

$$\left(m\alpha+n\beta\right)\left(p\frac{d\alpha}{dx}+q\frac{d\beta}{dx}\right)-\left(m\frac{d\alpha}{dx}+n\frac{d\beta}{dx}\right)\left(p\alpha+q\beta\right)=\left(mq-np\right)\left(\alpha\frac{d\beta}{dx}-\beta\frac{d\alpha}{dx}\right).$$

a relation which of course holds when we replace α and β respectively by a and b, two independent particular integrals of the differential resolvent.

"Oakwal," near Brisbane, Queensland, Australia,
August 1, 1865.

Mr. BAXENDELL drew attention to the auroral phenomena which occurred on the 19th and 26th instant, and showed sketches of the arches, &c., taken by Mr. R. P. Greg, F.G.S., at Prestwich.

Dr. JOULE, F.R.S., said he had observed the effect of the aurora on the former date, on his sensitive magnetic needle. The needle was violently agitated, as many as 36 changes of deflection, varying from 10″ to 1′ 40″ occurring per minute. The cause of the movements seemed to be instantaneous in its action. It was remarked that when the beams preponderated on the west of the magnetic north the needle took an easterly direction.

A paper was read entitled "Questions regarding the Life History of the Foraminifera, suggested by Examinations of their Dead Shells," by THOMAS ALCOCK, M.D.

The author said that though shore sand did not generally yield specimens in sufficient abundance or perfection to be worth examination, yet here and there very rich shore deposits are met with, and these ought to be specially noted when observed, both because of the exhaustless supply of specimens they afford for investigation, and the peculiarity of circumstances which must exist at these places to cause their accumulation. One such spot is at Dogs Bay, Roundstone, on the coast of Galway, where a great part of the shore is entirely composed of the remains of various microscopic creatures, by far the largest proportion consisting of shells of Foraminifera. Among other samples of sand from this place a very interesting one had been supplied by Mr. Parry, who had skimmed the material from the surface of pools left by the tide; it consisted entirely of fine and perfect specimens of the lighter kinds of Foraminifera, broken shells and the heavier sorts having sunk to the bottom. This naturally selected sample had furnished some very interesting results by supplying abundance of certain varieties comparatively rare in the rough sand, and examinations of some of these had led to the suggestions which he would now offer as to the formation of the shell in some kinds of Monothalamous Foraminifera. He considered in the first place that the soft body of the Foraminifer must act as a mould upon which the shell is formed, and that it must remain still and without change of shape while the process goes on; therefore that this, so far as the foundation layer of shell is concerned, will be a single act, and probably one requiring no great length of time. But if the shell be moulded on the surface of the animal, it will from the first be only just large enough to hold it, and consequently whatever growth afterwards takes place must be continued outside the shell, and must constantly increase the quantity of external sarcode which has been observed coating the shells of living Foraminifera, and which according to this view must be looked

upon as more than the mere result of a coalescence of the bases of the Pseudopodia. Orbulina universa and Globigerina bulloides are two forms found in great abundance in this Dogs Bay sand; they are interesting as being the prevailing shells in deep-sea dredgings and also as always occurring together, a fact which has been noticed by Professor Williamson, who has long entertained the belief that there is some very close relationship between them. The Orbulinæ are found of very different sizes, the largest being as much as six times the diameter of the smallest, with every possible intermediate gradation; and considering that the shell can only be made of the exact size of the body on which it is moulded, while that body will continue to grow, he could not avoid the conclusion that in this case at least, as often as a larger shell is required, the animal must withdraw itself entirely from the old one and cast it off. But a consequence of this view that the single-chambered Foraminifera cast their shells at intervals to form new ones is that they will occasionally be freed for certain periods from the restraint of the shell, and be in a condition to effect that spontaneous division which is so striking a feature in the Rhizopoda.

There are other specimens of Orbulina found occasionally in the Dogs Bay sand which have a very special interest, since they appear to show most clearly that Globigerina is merely a younger state of this species,—a fact which was first announced by Mr. L. F. Pourtales, (Ann. and Mag. Nat. Hist. 1858), illustrated by specimens obtained from dredgings in the Gulf Stream. Dr. Carpenter mentions the observations but expresses doubt as to their correctness. The Dogs Bay specimens however seem to corroborate them most fully, for they show the perfect Globigerina inside the sphere of the Orbulina.

MICROSCOPICAL SECTION.

October 16th, 1865.

A. G. Latham, Esq., President of the Section, in the Chair.

This being the first Meeting of the Session, the President delivered an address reviewing the past proceedings of the Section, and referring with satisfaction to the proposal to extend its objects to subjects of Natural History generally.

Mr. Sidebotham read "Notes on Atlantic Soundings."

He said that in the unsuccessful attempt made to raise the Atlantic Cable after it had unfortunately parted, the ropes and grapnels brought up from the bottom small portions of ooze or mud, some of which was scraped off and preserved, as stated at the time in the newspapers. Believing that a careful examination of this deposit might prove of considerable interest, he wrote on the subject to Dr. Fairbairn, who, after considerable trouble, obtained for him a fine sample, mounted specimens of which he now presented for the cabinet and to each member of the Section. In appearance the deposit resembles dirty chalk, and under the microscope reminds one much of the chalk from Dover, indeed it has all the appearance of being a bed of chalk in process of formation. It is composed entirely of organisms, chiefly in fragments. In the short examination he had made, he observed several forms which give promise of interesting results, and he thought it would be desirable to frame a complete list of the species found, which would be best accomplished by two or three members taking temporary

possession of all the slides, and preparing a report on their united observations. The sample now distributed was obtained at Dr. Fairbairn's request by Mr. Saward from Mr. Temple one of the Engineering Staff, who states that it was got in grappling for the Cable, August 11th, 1865, Lat. 51° 25′ 15″ N. Long. 38° 59′ W.

Mr. Sidebotham also read the following "Notes on Acherontia atropos":—

The Death's Head Moth, which was in former times an object of such dread that the appearance of a specimen of it was, like a comet, considered the precursor of some dreadful event, appears to be gradually becoming more common. If I remember rightly, it was Stothard the Artist who was so fortunate as to capture a specimen for his collection when a genuine British specimen was exceedingly rare. Even in my recollection a guinea or two was not considered too much to pay for a fine example. This season the insect has been unusually abundant, at least a score of larvæ having been found about my own neighbourhood. It has also been found at Bowdon, Middleton, Oldham, Strines, and other places round Manchester; in Middleton about 170 have occurred. Between Lytham and Blackpool it has been remarkably common; among those I obtained at Lytham was a very remarkable specimen, of which I made a rough drawing; it was so unlike the usual form that many who saw it fancied it must be some other species, but the same has been noticed by Stanton as occurring now and then, and Mr. Harrison obtained another somewhat similar at Bowdon. My specimen is still in the pupa state, and I shall carefully note whether or not the moth produced varies from the usual form.

Dr. Alcock read a paper entitled "Questions regarding

the Life History of the Foraminifera, suggested by Examinations of their Dead Shells."

This paper was afterwards read at the Ordinary Meeting of the Society, held on the 31st October. See page 15.

Dr. ALCOCK also exhibited specimens of Eozoon Canadense, from Canada, and also from Ireland, lent by Mr. H. B. Brady.

Mr. SYMONDS CLARK, of Adelaide, South Australia, exhibited a series of skulls, of small Marsupial animals, beautifully prepared by him; also the skins of several species.

Mr. SIDEBOTHAM recorded the discovery of Apion ononis, in the Isle of Anglesey, a species of Curculio, which he stated to be new to Britain.

Ordinary Meeting, November 14th, 1865.

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

Mr. Charles Bailey, and Mr. Thomas Barker, M.A., Professor of Mathematics, Owens College, were elected Ordinary Members of the Society.

The following extract of a letter from Thomas Ainsworth, Esq., of Cleator, near Whitehaven, Corresponding Member of the Society, accompanying a copy of his meteorological observations for October, was read by Mr. Baxendell:

The great peculiarity of the season has been the very heavy dews we have had—great luxuriance of pasture nourished by dews and not by rain. I have drawn Professor Simmond's attention to this as being a predisposing cause of the present cattle disease. Not that it really engenders the malady, but predisposes the animal to take this peculiar type of disease—my own experience from the readings of my instruments some twelve or fourteen years ago having shown that disease of the same type attacked my cattle and destroyed them, and each time when the high temperature of the day and the low temperature of night gave us such heavy dews as to render the herbage quite indigestible.

Mr. Baxendell considered it very probable that cattle would be injuriously affected by feeding on herbage which had not been well washed by occasional showers of rain. Dew had little or no washing effect, and it could not remove

the impurities which were deposited upon the leaves of plants during long periods of dry weather. The cattle plague is said to have had its origin in Central Asia, and in this region there is very little rain, and the daily range of temperature is very great. The herbage is therefore seldom well washed, and moreover, the cattle that feed upon it are exposed to frequent and violent changes of temperature. We have no report of any cattle plague breaking out among the herds on the pampas of South America, where rain falls more abundantly and the changes of temperature are much less violent.

The President said that the idea of deriving the cattle plague or any similar epidemic from the organic matter brought down by dew was at least in harmony with much that we had learnt. The dews and fogs of evening over certain lands were known to produce colds, agues, or fevers which could be avoided by rising to a certain height from the ground. There seems little doubt that the moisture in such cases is not the cause of disease, but only the means of conveyance. These diseases were produced by breathing the impure air. We know less of the effect when the matter is condensed and conveyed into the stomach, but the effect of impure water made this use of it also suspicious. He was not aware that it could be shown that in aggravated cases another class of disease might not be produced. In Manchester we can see the accumulation of matter taking place in the fog to such an extent that it lies like a cap over the whole town, and so increases that every sense is affected, whilst the lungs and eyes suffer severely. The matter in solution in this case is not putrefactive, although injurious, or it would probably sweep us off instantly. Probably no accumulation of putrefactive matter equal in amount ever occurred in the natural atmosphere. It illustrates, however, the mode by which the emanations of the soil are collected in the atmosphere and presented in a concentrated form for

us to breathe. He had for many weeks collected dew on a grass lawn in a garden, and from it had obtained organic matters unquestionably collected from surrounding objects, as it was known on one occasion to smell of flowers. If this entered into putrefaction it would of course be unwholesome, but what kind of disturbance of health it would cause it must be for others to find. The evening air of a rainy country like this is less dangerous than that of some other climates where there is more both evaporated and condensed, and neither wind nor rain to remove it. Notwithstanding this, he believed that more than the dew was required, especially in northern climes.

The President also said, that when sitting in a railway carriage with his friend, Mr. James Young, of Bathgate, that gentleman observed that the particles of dust which floated in the air seemed to shine with a metallic lustre. Dr. S. immediately collected some, and found that the larger class were in reality rolled plates of iron which seemed to have been heavily pressed and torn up from the surface. Another and smaller class were less brilliant, and when looked at with a considerable power shewed many inequalities of surface which would be interesting to study. Probably these were the particles which were not torn up but rubbed off. The dust enters the mouth and lungs, and has to be taken as one of the evils of railway travelling, although we do not know that these small particles are worse than those of sand. At any rate, it is clear that some kind of iron will wear down more readily than others, and we ought to have that which will wear down least. By observing what takes place in the carriages on a dusty day, every man may to some extent compare the iron of different railways. Those which give off the largest pieces in greatest quantities, are to that extent the worst, as regards health.

A paper was read entitled "An Attempt to refer some Phenomena attending the Emission of Light to Mechanical Principles," by R. B. Clifton, M.A., Professor of Natural Philosophy in Owens College.

The author attempted to show, by analogical arguments, that it is possible to give some account of certain phenomena attending the Emission of Light, by assuming principles closely resembling, if not identical with, those adopted by Professor Clausius in his well known paper on "The Nature of the Motion which we call Heat."*

Matter is assumed in all cases to have its atoms grouped together into *molecules*, an assumption which seems necessary when the different allotropic states of certain substances are considered.

These molecules are assumed to be in motion, and also the atoms to be vibrating in the molecules; for, whatever may be the laws of the forces which bind together the atoms in the molecules, it is impossible to imagine the molecules to be in motion, and to be subject to mutual actions, without causing motion of the component atoms.

In *solids* and *liquids* the molecules are supposed to remain continually within the spheres of action of neighbouring molecules, so that the internal motion in a molecule is never due to the undisturbed action of the interatomic forces—the only difference between solids and liquids being that in the former the same molecules are constantly neighbours, while in the latter a molecule may completely change its place in the liquid, and also that in liquids a molecule may perform complete rotations round axes through its centre of gravity, while in solids this is not generally possible.

In a *perfect gas* a molecule is supposed to be under the action of other molecules, only for a portion of time indefinitely small with respect to the whole time of motion, and its centre of gravity describes a polygonal path, only

* *Phil. Mag.*, vol. xiv., for 1857.

changing its direction of motion upon the near approach of the molecule to another molecule, or to a containing vessel, which may be considered as equivalent to an impact.

In an *imperfect gas* a molecule is supposed to be under the action of other molecules during a finite portion of the whole time of motion, this portion increasing as the gas approaches its state of saturation.

Between the molecules of a body, and the atoms of a molecule, the *luminiferous ether* is supposed to exist.

The vibrations in the ether which constitute radiant heat and light, are considered due to the vibrations of the atoms in the molecule, and not to the motion of the molecule as a whole; the latter bearing some such relation to the ether, as a bell or a stretched string does to the air, the internal vibrations only in the two cases causing the vibrations in the surrounding media, which give rise respectively to light and sound.

It appears obvious that as the motion of a molecule of a body as a whole increases, that is, as the temperature rises, the internal motion in the molecule also increases, considering that the action of one molecule upon another must be due to the mutual action of atoms, or to the interatomic forces, it seems probable that the internal vis viva in a molecule, to which the light is due, is proportional to the vis viva of the molecule as a whole, to which heat is to be referred. Thus, as the temperature of a body rises, the internal vis viva in the molecules increases, and the vis viva communicated to the ether also increases; hence the intensity of the vibration in the ether increases, and at the same time the period of vibration diminishes, or waves of shorter length are continually produced with increasing intensity.

Hence as the temperature of a body rises radiant heat is given off, the intensity corresponding to a given wave length constantly increasing, at last then vibrations in the ether, with wave lengths corresponding to the extreme red of the

spectrum, will be caused with sufficient intensity to be visible, and thus the body will begin at first to glow with red light; as the temperature still rises, and vibrations of shorter and shorter wave lengths become of visible intensity, the light emitted will gradually change from red to white.

From Draper's Law, that all bodies become incandescent simultaneously, as well as from other considerations, it seems probable that in *all bodies* the internal vis viva in the molecules bears the same ratio to the vis viva of the molecule as a whole.

In solid and liquid bodies, the molecules being constantly under their mutual actions, and these actions being subject to constant change from the varying relative positions of the molecules, the atoms cannot assume any definite periods of vibration, but are constantly changing the time of vibration; hence the vibrations in the ether will be constantly, and with extreme rapidity, changing their periods. This change having apparently no limit, and the effect upon the eye continuing for a finite time, light of all wave lengths will appear to be given off simultaneously by such bodies when the temperature is sufficiently high; in other words, incandescent solids and liquids will appear to give off white light, which when analysed by a prism will yield a continuous spectrum.

In the case of an incandescent Gas or Vapour sufficiently removed from a state of saturation to be considered perfect, the atoms will be left to vibrate under the action of the interatomic forces only, and will thus assume periods of vibration all included in a certain set; these vibrations will consequently cause vibrations in the ether corresponding only to certain definite wave lengths. Hence the spectra of such incandescent vapours will be broken, and consist only of a series of fine lines.

With imperfect gases, or vapours not far removed from their points of saturation, the intermediate phenomenon of spectra broken, but consisting of bands, is to be expected:

when however the temperature of such vapours is sufficiently increased, a change from spectra consisting of bands to spectra consisting of fine lines is to be looked for. This change has been observed in many cases.

When a solid body is incandescent, the light emitted so as nearly to graze the surface may be considered due mainly to the surface molecules, but these being free on the side of the surface, but affected by other molecules on all other sides, the internal vibrations in these surface molecules will have a bias in a direction perpendicular to the surface. Thus the vibrations caused in the ether, which are propagated nearly grazing the surface, will preponderate in a direction perpendicular to the surface, or considering the vibrations in plane polarised light to be perpendicular to the plane of polarisation, the light emitted by such a body, so as to pass close to its surface, will be partially plane polarised, the plane of polarisation being parallel to the tangent plane to the surface of the body at the point of emission.

In the case of an incandescent gas, the surface molecules are continually changed, and as a molecule may arrive at the surface in any position and is equally free on all sides, all trace of polarisation in this light will be destroyed.

The fact that incandescent metallic plates do emit partially plane polarised light in directions nearly grazing the surface, the plane of polarisation being parallel to the surface, and that incandescent gases emit unpolarised light, has been observed by Arago.

As the molecules at or near the surface of solids or liquids can cause vibrations in the ether, giving rise to emitted light, it is to be expected that, in some cases at least, it will be possible for light, if of sufficient intensity, when incident upon a body, to cause vibrations in the atoms constituting the molecules near the surface, but considering the difference of mass of the atoms of the body and of those of the ether, that the atoms of the body will vibrate slower than those of

the ether, the actual times of vibration depending however upon the molecular forces in the body. As these atomic vibrations will again affect the ether, such bodies will or may become luminous, the wave lengths of the emitted light being however longer than those of the incident light which causes the disturbance in the body.

This emitted light will necessarily last some time after the incident light is removed, for the vibrations in the body will not cease as soon as the cause of disturbance is removed, but in general it is to be expected that this emitted light will speedily disappear, though cases may occur in which it will continue for a considerable time.

These probable deductions from the assumed principles coincide exactly with the phenomena of Fluorescence and Phosphorescence (not including in this term cases in which light is emitted by bodies undergoing slow combustion), all Fluorescent bodies being Phosphorescent for times of different, though in all cases at present observed, of very short duration.

PHYSICAL AND MATHEMATICAL SECTION.

November 9th, 1865.

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

Mr. Baxendell, F.R.A.S., read the following "Note on the Variable Star S Delphini."

Since the discovery of this variable in October, 1863, it has gone through two complete periods of change, and my observations have enabled me to fix with tolerable exactness the dates of three maxima, and to determine the form of the light-curve for that portion of the period during which the

star is visible with telescopes of moderate power. Comparing the light-curves for 1864 and 1865 with the curve laid down from the observations made in 1863 after the discovery of the variable, I conclude that a maximum occurred about the 14th of October, 1863, the magnitude being 8·5. The next maximum occurred on the 12th of September, 1864—magnitude, 8·3; and the third on the 9th of August, 1865—magnitude, 8·9. The interval between the first and second maxima is 334 days, and that between the second and third, 331 days. The mean period is therefore about 332 days. In the Astronomische Nachrichten, No. 1523, Dr. Schönfeld states that this star was observed on the 8th of September, 1855, in zone 724 of the Bonn "Durchmusterung," and estimated to be of the ninth magnitude, but was invisible to Professor Argelander with the telescope of the Bonn meridian circle on the 9th, and with the heliometer on the 20th of November following, and was therefore not inserted in the "Bonner Sternverzeichniss," its existence evidently being considered doubtful. Guided by the course of the star's light-curve, I conclude from the Bonn observations that a maximum occurred about the 12th of August, 1855. The interval between this date and that of the last maximum, August 9th, 1865, is 3,650 days, during which time the star passed through eleven complete periods of change. We find therefore that the value of the mean period is 331·8 days, a result agreeing very closely with that derived from my own observations alone.

An inspection of the diagrams which accompany this communication will show that S Delphini increases in brightness much more rapidly than it diminishes, and that the course of its light-curve is more irregular after than before a maximum. During the last apparition it rose from the thirteenth magnitude to a maximum in 48 days, but was 89 days in descending again to the same magnitude. When at minimum it is below the 13½ magnitude, and it remains

invisible with telescopes of ordinary power for more than the half of its entire period.

Mr. G. Knott, F.R.A.S., of the Woodcroft Observatory, Cuckfield, Sussex, has kindly favoured me with a copy of his observations of S Delphini, made during the present year, and of the light-curve laid down from them. He has obtained for the date of maximum August 11, 1865—magnitude, 8·8. Considering the nature of the observations, and the form of the light-curve at its maximum, this result agrees very fairly with that derived from my own observations. It will however be seen that there is a slight difference in our modes of drawing a curve through the points laid down; Mr. Knott evidently regarding apparent irregularities as being principally owing to errors of observation, while, on the other hand, I have regarded them as being mainly due to actual changes in the brightness of the star. Treating his observations precisely as I have treated my own, I do in fact, obtain the same date of maximum, August 9, 1865.

The colour of S Delphini is decidedly "reddish," and my observations seem to indicate that this colour becomes sensibly more intense as the star approaches its minimum. The difference of 0·1 between my own and Mr. Knott's estimations of magnitude at the last maximum is doubtless due to the colour of the star, and to the fact that while my observations have been made with a telescope of five inches aperture, Mr. Knott's have been made with one having an aperture of seven and one-third inches.

ERRATUM.—Page 20, last paragraph, for *apion ononis*, read *apion ononidis*.

Ordinary Meeting, November 28th, 1865.

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

Mr. Francis Hampson, Solicitor, was elected an Ordinary Member of the Society.

Mr. Dancer, F.R.A.S., said that in a paper "On the Illumination of Opaque Objects under the high powers of the Microscope," read before the Microscopical Section of this Society, November 20th, he had described a method of employing the oblique body of the binocular microscope with Wenham's prism, for illumination of opaque objects, and he had also exhibited an instrument fitted up for this purpose, giving the members present a practical demonstration of the advantages which this mode of illumination afforded under certain circumstances. He wished now to describe another method of illuminating opaque objects, and as it is equally applicable to monocular and binocular microscopes, it appears worthy of some consideration.

In the method of Mr H. L. Smith, of Kenyon College, (which was briefly described in the paper before mentioned), and also in the use of the Wenham's prism there is a considerable loss of angular aperture, (which is a very important consideration): it occurred to the author that by modifying Mr. Smith's contrivance this loss might be diminished in some degree; this has been attempted in the following manner.

Instead of placing the mirror immediately over the opening at the back of the object glass, a small speculum $\frac{1}{6}$ of an inch in diameter is introduced into the front of the body of the microscope, $2\frac{1}{2}$ inches above the top of the objective. A lateral opening is made in the body at right angles to the speculum, for the admission of light to be reflected down through the objective to the object below.

The interposition of the small speculum does not produce any disagreeable effect in the field of view, and in the examination of objects it is easy to use that portion of the field which is between the centre and the edge. With proper manipulation very good definition can be obtained by this method, when the speculum is of the proper curvature. This contrivance can always remain attached to the microscope without interfering with the general appearance of the instrument, and when the use of the speculum is not required, it can be withdrawn or turned aside out of the field of view, and the aperture at the side of the body may be closed by a small shutter. It is obvious that the use of the binocular body is not interfered with by this arrangement.

A binocular and a monocular microscope with this arrangement were exhibited to the members at the close of the meeting.

Mr. Hardy (on behalf of Edward Ross, Esq.) exhibited a large cetacean vertebra which had been found in the valley of the Don, about two miles from Tinsley and four from Rotherham, in Yorkshire, on a line of railway now in course of construction. The bone was met with in excavating, at a depth of 14 feet below the surface of the ground, in a bed of gravel overlaid by the alluvium of the valley. In answer to questions put by Messrs. Binney and Hull, Mr. Hardy described the bone as one of the lumbar vertebræ of a species of whale, probably identical in genus with the balæna of the present seas. The bone measured on its largest diameter a

little over ten inches, in thickness seven inches, and in circumference about three feet. It presented every appearance of having lain in the earth for a very considerable length of time; but as it only reached Manchester on the day previous, the geological character of the gravel in which it was found had not been ascertained.

Dr. ROBERTS drew attention to the injurions effects produced by burning Pharaoh's serpents in close rooms, and gave the particulars of a case which had lately come under his notice.

Dr. ROSCOE stated that in his opinion persons could not be too careful respecting the inhaling of even small quantities of mercury vapour, and he alluded in support of this opinion to the fact that two German gentlemen who were engaged in a London laboratory, in the preparation, for a scientific purpose, of volatile organic mercury compounds, had recently been poisoned by the accidental absorption through the lungs or skin of very small quantities of the vapours of these substances. The symptoms characteristic of this form of mercurial poisoning are of the most painful and distressing kind, the first patient dying in a state of mania shortly after his admission into the hospital, and the second, on whom the effect became first perceptible three months after he had ceased to work with the substance, now lying in a hopeless state of idiocy.

A paper was read "On the Amount of Carbonic Acid contained in the Air above the Irish Sea," by Mr. T. E. THORPE, Assistant in the Private Laboratory, Owens College, communicated by Professor H. E. ROSCOE, F.R.S.

The determination of the amount of carbonic acid contained in the atmosphere over the land has been made the subject of investigation by many experimenters, and from the results obtained by Theodore de Saussure, Brunner, Boussingault, Angus Smith, and others, we are acquainted with the exact

proportion of this gas contained in the atmosphere under varying circumstances of situation and weather.

But hitherto, the influence which, *a priori*, must necessarily be exercised by large bodies of water on the proportion of carbonic acid in the atmosphere has scarcely been sufficiently studied. The fact that a considerable influence is exercised has certainly been noticed, but beyond the incomplete results of one or two observers, we have no numerical data from which to judge of the extent of this influence, and we therefore know but little of the changes in the comparative amount of the atmospheric carbonic acid as effected by the waters of the ocean.

Dr. Roscoe therefore suggested that I should undertake some experiments on this subject, and kindly placed the necessary time and apparatus at my disposal. I may here be allowed to express my thanks for this kindness, and for the advice I have received from him during the prosecution of these experiments.

It appeared from the observations of Vogel on the air of the Baltic and of the Channel that the sea abstracts to a very considerable extent the carbonic acid from the atmosphere; and this conclusion was apparently confirmed by the experiments of Emmet on the air over the Atlantic and at Bermuda, and by the determinations of Watson at Bolton, made on air blowing from the seaward.

These experiments were however, for the most part, merely qualitative, and the circumstances under which they were made, together with the inaccurate nature of the methods employed, render such a conclusion premature. In fact, the experiments of Lewy and Morren on the nature of the gases which sea water holds in solution at different periods of the day and during various seasons of the year would appear to show that the sea may possibly act in quite the opposite direction, and cause a sensible *increase* in the comparative amount of atmospheric carbonic acid.

The air contained in sea water consists of variable quantities of free carbonic acid, oxygen, and nitrogen, and Morren and Lewy have shown that the changes in the relative proportion of these gases depend:—(1) upon alteration of temperature affecting the relative amounts of the dissolved gases in accordance with the laws of gaseous absorption; and (2) upon the variations in intensity of direct and diffused solar light, producing a corresponding effect upon the vitality of sea plants and animals, and hence altering the composition of the dissolved gases.

Some further experiments by Lewy, on the composition of the atmosphere above the Atlantic ocean in the tropics, tend to confirm the above supposition of the possible increase in carbonic acid in the atmosphere above the sea. In fact, if it is possible that the composition of the air above the sea in our latitudes can be sensibly altered by this phenomenon of the variation in the nature of the gases in solution in sea water, as Lewy and Morren assert, we might expect that the atmosphere above the tropical oceans would manifest to a much larger extent variations in the relative amounts of carbonic acid and oxygen, since infusoria exist, as is well known, in enormous quantities in these oceans, and the composition of the air in their waters must necessarily undergo rapid variation, and a considerable evolution of the dissolved gases must consequently occur. At the instance of the French Academy Lewy collected air at different times during a voyage from Havre to Santa Marta, and on subsequent analysis not only did it appear that the mean quantity of carbonic acid was sensibly greater in the air of the Atlantic ocean in the tropics than in the air of the land, but also that the air of the day was appreciably richer in carbonic acid and oxygen than air collected in the night.

On comparing the means of each series we have, in 10,000 volumes of air, for the

	Day (mean of 7 expts.)	Night (mean of 4 expts.)
Carbonic acid......	5·299	3·459
Oxygen	2105·801	2097·412

and this variation appeared to increase in proportion as the middle of the ocean was approached.

This remarkable phenomenon, of the variation in composition of the air above the tropical oceans, may doubtless be accounted for, without any reference to the direct action of infusoria, by the heating effect of the sun on the sea water and the consequent disengagement during the day of gas proportionately rich in carbonic acid and oxygen. During the night, on the other hand, as this source of action is removed, the disengagement may be assumed not to occur; and, following Lewy, one may perceive that this difference would become more appreciable and easier to trace in air at great distances from any continent than in air collected nearer the coasts, and consequently, liable to be mixed with the air of the land.

Although the precision of these results is certainly remarkable, they still require confirmation. The air was collected in glass tubes of about 100 cbc., and analysed eighteen or twenty months after in the eudiometric apparatus of Regnault and Reiset. The fact pointed out by Regnault that air which has remained for any great length of time in glass tubes invariably exhibits notable diminutions in the amount of carbonic acid, since the glass absorbs a portion of this gas; and the difficulty generally experienced in accurately noting contractions so minute as the absorption of the carbonic acid from a small volume of atmospheric air, are circumstances which may possibly influence the reliability of the results.

The kind permission of the Honorable Board of Trinity House has enabled me during the vacation of last summer to make some additional experiments in this direction on board the "Bahama Bank" Light-vessel, situated in the Irish sea, latitude 54° 21′ and longitude 4° 11′, seven miles W.N.W. of

Ramsey, Isle of Man, and consequently nearly equidistant from the nearest shores of England, Scotland, and Ireland. The ship is placed to mark the proximity of a dangerous bank, by which, for the greater part of the day, a strong current, setting in from the southward, flows through the North Channel and thence into the Atlantic.

These experiments were made in the early part of August, at the same periods of the twenty-four hours, namely about 4 a.m. and 4 p.m, or nearly the times of minimum and maximum temperature.

Pettenkofer's method of analysis was adopted, with the improvements in the practical details suggested by Angus Smith. This method is in principle similar to the one adopted by Watson and Emmet, but admits of far more delicacy and precision in practice. Baryta is substituted for lime water, and oxalic for sulphuric acid. The solution of oxalic acid for these experiments was made so that one cubic centimetre of it corresponded to one milligramme of carbonic acid; it thus contained 2·864 grms. of pure crystallised oxalic acid per litre. Twenty-five cubic centimetres of the baryta solution were originally made to correspond to about twenty-eight of oxalic acid, but of course the exact strength of the baryta water was ascertained previous to each experiment. The bottles were generally filled with the air by means of the bellows, but sometimes when the wind was strong it sufficed to hold them up for a minute or two in such a manner that the air could circulate freely within. The baryta water remained in contact with the enclosed air for three quarters of an hour to one hour, during which time the bottles were frequently agitated. Although even this is longer perhaps than is actually required for the complete absorption of the carbonic acid, still, for the sake of conclusiveness, in experiment 4 the bottles were allowed to stand for three hours, and in experiment 13 for six hours, before the solutions were tested. The capacities of the two bottles which served for all the ex-

periments were 4815 cbc. and 4960 cbc. The burette was Mohr's modification, for which a table of calibration had been constructed by weighing and interpolating in the ordinary way.

The fact that the various meteorological changes influence to such a remarkable extent the nature and amount of the gases dissolved in sea-water renders it necessary, in any investigation on the constitution of the atmosphere over the sea, to take particular account of these changes. Accordingly the temperature, pressure, and degree of humidity of the air; direction and force (estimated—Beaufort's system) of wind; amount (estimated—overcast=10) and nature of clouds, and general appearance of the day, together with the temperature of the sea water and amount of sea disturbance (1 to 9), were noted at the time of each experiment.

The following table shows the results of these observations, together with the amount, in volumes, of the carbonic acid in 10,000 volumes of air. All the experiments which were made are here given. The hours of observation, as before stated, were 4 a.m. and 4 p.m.

TABLE OF RESULTS.

No.	Aug. 1865.	Night or Day.	Bar. Mm.	Temp. of Air.		Temp. of Sea.	Direction and Force of Wind.	Sea Distrb. (1 to 9)	Amount and Nature of Clouds. (Overcast =10.)		Carbonic Acid.		Remarks.
				D. Bulb.	W. Bulb.						1st Ex.	2d Ex.	
1	4th	D.	762·5	16·4° C.	11·1° C.	16·0° C.	N.W. by W. v. light	calm	cirrus	1	2·66	3·07	Day very fine and clear.
2	4th	N.	762·0	13·9	12·9	15·0	S.W. by S. light br.	1	cirro-cumuli	9	2·92	3·05	
3	5th	D.	761·2	16·1	14·4	15·0	S.W. by S. light br.	1	cirro-cumuli	9	3·08	3·21	Not much sun.
4	6th	N.	753·4	14·2	13·3	15·0	S.W. by W. light	1	cirro-cumuli	9	3·30	3·22	Baryta water exposed 3 h.
5	7th	D.	757·5	17·2	15·1	15·6	N.W. light	1	cirro-cumuli	6	3·20	3·15	Sunny, very fine.
6	7th	N.	760·2	13·6	12·2	15·0	N.N.W. moderate	3	cirro-cumuli	8	3·06	3·19	
7	8th	D.	761·0	18·3	13·1	16·0	N.N.W. light breeze	1	cirrus	1	3·32	3·02	Fine and sunny.
8	8th	N.	758·7	13·3	12·2	15·0	S.W. by W. light br.	1	cirrus	7	2·93	3·10	
9	9th	D.	756·4	15·0	—	15·0	S. by W. moderate	1	cirro-cumuli	7	3·09	3·23	Rain.
10	10th	D.	749·3	15·0	13·9	14·5	S. by W. fresh	4	nimbus	9	3·11	3·11	Very wet, rain all day.
11	11th	N.	750·5	13·4	11·9	14·5	S.W. by W. strong	5	nimb. & cirro-cum.	7	3·09	3·10	Very windy and much rain from the 11th to the 16th.
12	16th	D.	752·3	14·7	12·8	15·0	N.W. by W. light	2	cirro-stratus	8	2·93	2·95	
13	16th	N.	753·1	13·9	12·8	15·0	W.S.W. fresh	4	cirro-stratus	9	3·12	2·94	Baryta water exposed 6 h.

Day Mean of 14 Determinations .... 3·086

Night „ 12 „ .... 3·085

In comparing these results with the following determinations of the carbonic acid contained in land air, it is seen that the air of the Irish Sea contains a much smaller proportion of carbonic acid than the air of the neighbouring land. The most extensive observations on the land air have given as means:—

Observer.	*Locality.*	*No. of Expts.*	*Vols. in 10,000 of air.*
Th. de Saussure,	Chambeisy,	104	4·15
Boussingault,	Paris,	142	3·97
Verver,	Groningen,	90	4·20
Roscoe, 1st series,	London & Manchester,	108	3·97
„ 2nd series,	Manchester,	53	3·92
Smith,	ditto.	200	4·03
General mean of land air			4·04
Mean of 26 expts. on sea air			3·086

It would also appear that no difference is discernible in the amount of carbonic acid in the air of day and night over the Irish Sea. On the other hand, from Saussure's observations a decided difference may be traced between day and night air on the land—a conclusion subsequently confirmed by several experimenters.

In noting the above mean 3·08, and the apparent identity in the amount of carbonic acid in the air of day and night over the sea, it should be borne in mind that July and August are, in general, the hottest periods of the year, (these months were unusually hot this year, 1865) and that, consequently, all the influences may be supposed at work which would tend to increase the relative amount of carbonic acid, and render appreciable any difference in the air of night and day.

The conclusions therefore to be drawn from these experiments are:—

1. That the influence of the sea in our latitudes in abstracting the carbonic acid from the atmosphere is not so great as the old experiments of Vogel and others would lead us to suppose.

2. That the sea in our latitudes does not act in increasing the amount of carbonic acid in the air above the ocean, as found by Lewy over the Atlantic near the equator.

3. That the differences observed in the air of night and day by Lewy on the Atlantic, are not perceptible in the air above the Irish Sea.

4. That in the month of August 1865, the mean quantity of carbonic acid in the atmosphere of the Irish Sea was 3·08 in 10,000 volumes of air.

In conclusion, I beg to acknowledge the kind attention which I received from Captain Temple, and from his crew during my stay on board his ship.

MICROSCOPICAL SECTION.

November 20th, 1865.

A. G. Latham, Esq., President of the Section, in the Chair.

Mr. Parry read a paper on "Collecting Foraminifera on the west coast of Ireland." He said that in June last he visited the coast of Connemara, for the purpose of collecting Foraminifera, more especially at Dogs Bay; he was accompanied by Mr. Burns, of Doohulla Lodge, who gave him much assistance. After he had procured a considerable quantity of the shell-sand in the usual way, he noticed some white floating material on the surface of the advancing tide; he collected a quantity of it by means of a muslin net, and on examination found it nearly all composed of perfect dead shells of Foraminifera. On a second visit to the bay Mr. Burns discovered a pool near high water mark, covered with the floating shells, and of these Mr. Parry collected a large quantity, portions of which he had since distributed to members of the Section. He observed that the underside of the rocks forming the pool were covered with foraminifera, and he therefore concluded that these minute creatures live there, and from what he saw he was led to believe that Dogs Bay is a breeding ground for them, and that they may also be found living in "Burns' Pool."

Mr. Dancer, F.R.A.S., read a paper "On the Illumination of Opaque Objects under the high powers of the Microscope."

The Author's attention was drawn to a paper on this subject, which appeared in the Scientific American, and was copied into the Mechanics' Magazine of October 20th, 1865.

Mr. H. L. Smith of Kenyon College, had contrived a plan for the illumination of opaque objects, by placing a small mirror in a rectangular box, which could be attached to any ordinary microscope, this mirror was made adjustable immediately over the opening of the back of the objective, a light was placed at the side of the box and reflected down through the objective on to the object. In this manner the object could be illuminated when the high powers were used.

Mr. Hurst suggested that a discussion on this subject would be of interest to the members of the Microscopical Section. The Author not having time to make one of Mr. Smith's apparatus, thought it possible to arrive at similar results by the employment of the binocular microscope, an instrument which is now more common than a monocular instrument. The trial quite answered his expectations. The simplest method and one which gave good results, is to remove the eye piece from the oblique body and fix a reflector on the top of the body in such a manner as to throw the rays of light down to the Wenham's prism, and thence through the object glass on to the object.

If a plane mirror is employed, a lens of suitable focal length should be placed in the body in order to get the field of view entirely illuminated.

A concave mirror or lenticular prism can also be used for the same purpose, providing the focal length is adapted to the length of the body and object glasses.

Various modifications can be adapted so as to vary the character of the illumination to suit the particular object to be viewed. In some cases the Wenham's prism may be withdrawn a little, to produce the proper effect.

Uncovered objects only can be seen to advantage, owing to the light reflected from the surface of the covering glass. The surface on which the objects are mounted should reflect as little as possible, and be a marked contrast in colour to the object.

Ordinary Meeting, December 12th, 1865.

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

Mr. J. Bottomley said that a recent paper upon the employment of the internal heat of the earth led him to consider what might be the condition of the atmosphere when coal, lignite, anthracite, and all other forms of vegetable fuel should be so exhausted that the human race would be compelled to resort to this source of heat. The numbers obtained led him to the conclusion that the exhaustion of the coal fields implied more than the depriving of the human race of a ready source of warmth, namely, the alteration of the atmosphere to an extent that would ultimately prove fatal. As the latter assumption seemed to him to be repugnant to reason, he would infer that long before the exhaustion of the coal fields, the carbonic acid in the atmosphere beyond the limits of safety to life, would have been decomposed by vegetation; moreover, as plants decomposed water, there would always be some combustible compound of carbon and hydrogen; in other words, there will and must be abundance of fuel in the world in all ages, if not of so dense a character as anthracite and coal, yet of some nature intermediate between those fuels and vegetable tissue, the origin of all varieties. The effect of vegetation in maintaining the purity of the atmosphere has long been known. The assumption that this agency is sufficient to furnish an abundant and perpetual supply of fuel to mankind involves no new principle, but it tends to establish a new inference upon principles already

acknowledged. Liebig states, in his "Chemistry in its Application to Agriculture and Physiology," (3rd edition), that the quantity of carbon existing in the atmosphere amounts to more than the weight of all the plants and of all the strata of mineral and brown coal existing on the earth. This would seem to favour the notion that the mineral and brown coal available for combustion would not affect the atmosphere to any serious extent if consumed. He has assumed more carbonic gas to exist in the atmosphere ($\frac{1}{1000}$ by weight) than many authorities would allow. Moreover, since the book was written enormous deposits of fuel have been discovered. In another passage Baron Liebig seems to favour an opposite view, for he states—"In former ages, therefore, the atmosphere must have contained less oxygen, but a much larger proportion of carbonic acid, than it does at the present time, a circumstance which accounts for the richness and luxuriance of the earlier vegetation." Dumas and Boussingault say, in their book on the chemical and physiological balance of organic nature, "If we suppose, then, that the whole of the carbon was diffused through the atmosphere in the shape of carbonic acid prior to the creation of organised beings we shall see that the atmosphere, instead of containing less than the one-thousand part of its bulk of carbonic acid as at present, must have contained a quantity which it is not easy to estimate, but which was perhaps in the proportion of 3, 4, 5, 6, and even 8 per cent." Mr. Hull, in his "Coal Fields of Great Britain," taking 4,000 feet as the depth capable of being worked, estimates the supply from the English and Welsh coal fields at 60,000,000 tons for 1,000 years. In the same book it is stated that the American coal fields are 72 times greater than the English and Welsh.

In the reports furnished to the Admiralty some years back, by Dr. L. Playfair and Sir H. De la Beche, there is given a table showing the average composition of Welsh, Newcastle,

Lancashire, Scotch, and Derbyshire coal. The mean of these numbers is as follows:—Carbon, 80·40; oxygen, 7·16; hydrogen, 5·19. Subtracting from the amount of hydrogen the quantity corresponding to 7·16 of oxygen there remain as combustible material in one part of coal—carbon, 0·8040; hydrogen, 0·0421. For combustion it requires 2·4828 parts of oxygen, and produces 2·9480 parts of carbonic acid. Sir J. Herschel, in his "Meteorology," takes as the approximate weight of the atmosphere 11×10^{18} pounds. If we take as the amount of oxygen in the atmosphere 23·04 per cent by weight, and as the amount of carbonic acid ·05 per cent by weight, the following numbers are obtained (assuming 1×10^{14} pounds as the unit of measurement):—Weight of atmosphere 110000; oxygen contained 25344, carbonic acid contained 55; weight of coal, 98·112; oxygen required for combustion, 243·59; carbonic acid from combustion of coal, 289·23; total carbonic acid, 344·23; ratio of oxygen to carbonic acid at present, 460·8 to 1; ratio after combustion of assumed quantity of fuel, 72·9 to 1.

The last ratio can of course only be regarded as an approximation, but when we take into account all the available fuel in the world—wood, peat, lignite, coal, anthracite, also the quantity of carbonic acid evolved from volcanic districts—and remembering the opinion of a member of this society, that we know little about the difficulties likely to be encountered in mining operations at a depth of 4,000 feet—there seems little reason to doubt that the ratio of the oxygen to carbonic acid would be reduced considerably below the number above stated, and that the quantity of carbonic acid in the atmosphere would reach a point much beyond that at which it becomes deleterious to human life. It seems then more reasonable to take the alternative and maintain that the carbonic acid will be de-oxidized, and that there will always be an abundance of fuel.

A paper was read entitled "Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes." Part II. By J. C. Dyer, V.P.

The Employment of Steel for Transferring Engravings.

At the beginning of this century, upon the death of Washington, medals to commemorate that event being called for, Mr. Jacob Perkins (then a silversmith at Newbury Port, near Boston) undertook to supply them, and, as they were required in large numbers speedily, he devised a summary process of transferring the engraved design, from prepared steel dies or stamps, by which he obtained several from one original die, and thus a vast number of medals were rapidly produced. Shortly after Mr. Perkins applied the same principle of transferring engravings for bank notes, on which very elaborate designs were printed to prevent or render their being forged very difficult by the hand of the engraver. To effect this he procured cast steel plates, and decarbonated their surfaces to the depth of about one-sixteenth of an inch, which were thus converted into *very soft* and *pure iron;* the letters and designs for the notes being then engraved upon them they were case hardened and tempered for *use,* but in lieu of printing from these plates they were used as dies for making others to print with. His next process was to prepare a cast steel cylinder, which in like manner was decarbonated at the surface, and then under a strong traversing pressure it was rolled over the letters and figures engraved on the hardened plate, and these engravings were taken up in relief on the surface of the soft cylinder. This cylinder being then hardened and tempered, was used to transfer, by means of the same traversing pressure, the entire work upon its surface, to any number of copper or soft steel plates for printing with.

The adoption of this plan by several banks, for having very elaborate engravings on their notes, turned the counterfeiters

upon other banks whose notes would be so much more readily forged, which led to an extended demand by the other banks.

In the year 1809 Mr. Perkins communicated to me the details of his process of transferring engravings, with a view to having the invention patented in England for our joint account. From the success of his plan in America its adoption here was anticipated, and still further development of it looked for from the higher state of the graphic arts in London. With this view I took out patents, and minutely specified "the method of carrying the invention into effect." A very beautiful design was then obtained from the classic pencil of the late Sir R. Smirk, R.A., which was engraved by Reimback, on prepared steel, for printing bank notes. But I could not succeed at that time to induce the Bank of England or any other bank to adopt the plan, nor could the booksellers then be made to perceive the importance of the transferring system for illustrating books, for which it has since been so extensively used. The time had not arrived when public attention could be drawn to the bank note forgeries as a national evil and the disgrace of hanging men for a feat so readily performed as that of forging the one pound notes then in general circulation. If any excuse can be offered for this apathy, it may be said that the passions and interests connected with the war, together with those yet more embarrassing that arose from the transitions from war to peace, caused such disturbances in the circulating medium and in the general interests of commerce and industry, that it became very difficult to awaken public attention to the great scandal of relying solely upon the gallows for preventing forgeries.

It has been above shown that Perkins' invention was not for engraving on steel plates for printing, nor for engraving upon steel at all, but rather for engraving on soft iron of homogeneous structure. It was found that all wrought iron

is more or less fibrous and unfit to receive delicate engravings, and that by decarbonating the surface of cast steel a pure iron surface was obtained, and this being engraved on, was case hardened and used for transferring and printing as before stated. This should be kept in view, because many persons have supposed that the invention of Perkins was merely the substitution of steel in the place of copper for engraving upon; such a substitution of the one metal for the other would not be an invention in any fair sense of the word. But his method of obtaining soft iron surfaces to receive the work, converting these surfaces back into steel, and then transferring the engravings to other plates for printing, comprised together a series of novel processes which confer lasting honour upon the inventor.

After the transition period, having better hopes of success, I recommended Mr. Perkins to come over himself to explain his system and aid the artists here in putting it into operation. Accordingly in the year 1820 Mr. Perkins came to England, and being over sanguine, brought a large staff of able artists, mechanics, &c., but he could not bring any money to aid in establishing his intended works in London. He had assumed that capital could always be obtained in England for conducting any safe and profitable schemes. Now the matter of proving his to be such was not easy to establish with the *monied class;* so to me alone, not of that class, he had to look for the entire expenses of his mission, and this I could only bear for a few months. After some time the late Mr. Charles Heath, the eminent engraver, was induced to join Mr. Perkins and become a partner in the engraving works which were then commenced in Fleet-street, and are still continued by their successors.

Besides the printing on paper, Mr. Perkins' system of transferring has been since very extensively employed for calico printing, and in later years we have also seen his process employed to a vast extent in many other departments of

the graphic art, such as post office and receipt stamps, and other prints that are required in greater numbers than could be produced by other than steel plates or stamps. His system of engraving on steel has at length become a great artistic power, the wide-spread increase of which has given employment to labour and capital to a vast extent in the several branches of art before stated, and from which I believe many large fortunes have been made, but little other than "toil and trouble" ever accrued to the inventor of them.

When any important discoveries in physical science are made they never die, whatever may chance to their authors. The new facts brought before the public go forth like seeds cast upon a fertile soil, yielding the fruits of continual progress among the families of men who seek improvement. It seems only just then that each generation should transmit to the next some record of the names of those contemporaries to whose genius and talents all nations are indebted for such discoveries. Wherefore, in addition to the four distinguished inventors brought to the notice of this Society in my former papers, I have in the present one aimed to place that of Jacob Perkins as a worthy contributor to the advance of those branches of art to which his inventions have been applied.

Appendix I.

On the Compression of Water.

In tracing the progress of steel engraving I had no thought of giving a general account of Mr. Perkins' researches in physical science, yet it may not be out of place to notice one or two other of his discoveries.

(1) His experiments on the compressibility of water (made some time before he left America) were to test the correctness of the doctrine founded on the Florentine experiments, that water was a non-elastic body, which was then generally taught in the schools and elementary works. At that time

Mr. Perkins had never heard of the experiments of Canton, (made some fifty years before) which had established the compressibility of water. Although by Mr. Perkins' experiments its discovery was not *strictly new* yet they were of high scientific value, because of the widely different compressing forces employed by him and by Canton, the latter having applied the pressure from half an atmosphere to two atmospheres, say from seven and a half to thirty pounds a square inch, whilst that employed by Perkins was from fifty to four hundred atmospheres, or from 750 lbs. to 6,000 lbs. per inch. The same rate of compression appeared in all his experiments, which corresponded with that shown by Canton's experiments, and in all of which the water was compressed in volume directly as the compressing forces.

The apparatus employed by Perkins was first a cast iron cylinder, about three inches thick, with a movable top of equal strength; this, filled with water, had a force pump (as in the hydraulic press) to measure the pressure within by the leverage and size of the induction pipe. 2nd. A small brass cylinder, with a piston to slide in it, water tight, about three quarters of an inch diameter, and to have a column of water ten inches long under the piston. The piston rod, graduated to divisions of a hundred to the inch, had a sliding ring on it, to be pressed upon the rod as it was forced down upon the enclosed water, thus marking in the hundredths of an inch the descent of the rod; the brass cylinder, being under the same pressure inside and out, was not subject to any strain to alter its capacity. 3rd. When the external pressure was removed, the water in the brass cylinder expanding to its original length and raising the piston rod, would of course mark the greatest compression effected. In each experiment the diminished bulk of water was directly as the pressure applied, and under the pressure of 100 atmospheres the bulk of the water was reduced one part in a hundred, and, as before mentioned, this rate proved to be the

same as that shown by the experiments of Canton. Some time after Professor Oersted made similar experiments by employing a stout glass vessel and using a column of mercury to give the pressure, having the like inside cylinder as that of Perkins to mark the result, which confirmed the same rate as that shown by Perkins and Canton.

Appendix II.

On Perkins' Steam Gun.

Mr. Perkins conceived the idea of employing steam at a very high pressure for discharging projectiles with greater rapidity and effect than could be done by the common use of gunpowder. To effect this object he devised a plan for heating water more intensely than could be done by any boilers then known, viz.—that of employing a great number of iron or copper tubes, with their ends fastened into plates, with chambers or cavities for receiving the water at one end and emitting the steam at the other.

This apparatus was placed in the midst of a furnace for the heat to act directly on the water in the tubes, and thus, as Mr. Perkins phrased it, the water could be made red hot and flash into steam with a force exceeding that of gunpowder. Then a gun barrel, with its breech opposite the valve opening from the steam chamber, and an apparatus for conducting the balls into the space between the breech of the barrel and the outlet of the steam, and, the valve opening at the same time, the steam issued and propelled the balls through the gun in rapid succession with a force about equal to common powder, which could be continued as long as the heat of the furnace could keep up the pressure. He found that from fifty to a hundred balls per minute were shot forth to a target, about one hundred yards, with a force nearly equal to that of a common musket, and of course by having ten such guns fixed to the same furnace, from 500 to 1,000 balls might be discharged per minute. His experiments were witnessed by the

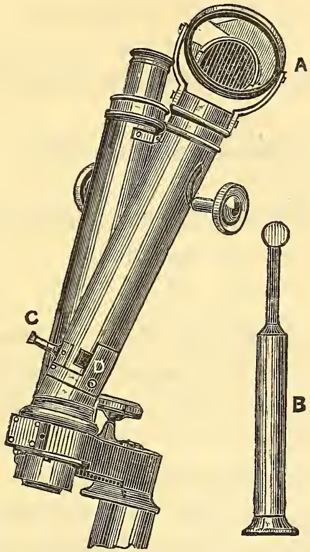
Duke of Wellington and many other eminent men, who took much interest in them.

The non-adoption of his system arose from several radical defects in it—first, the danger from fire of having such a cumbrous furnace on a ship; second, the time required to get up the steam in case of sudden encounter with an enemy, which might lead to a surrender before a shot could be thrown from the steam battery; third, by unequal heating the tubes sometimes gave way, allowing the water to escape, and though the quantity being small would not cause explosions, leaks would deaden the fire and stop the action of the guns, and any suspension in the midst of action must be fatal to the ship using such a weapon. Still the after interest attached to the plan of the tubular boilers came from reversing the scheme used by Perkins, viz.—employing the tubes as flues for the fire, to convey the heat through the tubes to the water surrounding, then, in an outer boiler, by this method, without damaging the tubes, it is found that high pressure steam can be employed with safety and advantage, so that Mr. Perkins' invention was not barren to the outer world, since his tubular boilers led to their extended employment in railway and steamboat engines, and were, I believe, first employed by Stephenson a few years after the steam gun experiments had been put "hors de combat."

NOTE.

Although it is needless to describe the process of case hardening, so generally known, it may be well to explain that of decarbonising the steel plates for engraving. This process is as follows:—The prepared steel plates are placed in a cast-iron box, and covered about an inch deep with an oxide of iron, prepared by subjecting iron filings to alternate wetting and drying until they are mostly converted into red oxide. Over this covering a clay luting is placed so as to exclude the air, and the box is then placed in a furnace and kept at a red heat for about sixty hours, when the oxide in contact with the steel will have taken up the carbon from its surface to the depth of about a sixteenth of an inch, and thus convert the surface into pure iron, as mentioned in the text.

Engraving of Mr. J. B. Dancer's method of illuminating opaque objects under the high powers of the microscope.



At A is shown a concave mirror, having a vertical and horizontal movement, mounted over the oblique body of a binocular microscope. By this mirror the light is reflected down to the Wenham's prism, and thence through the objective to the object.

Another method.

B is a representation of a small speculum fixed at the end of a brass wire. This is inserted into the vertical body just over the fine motion tube at C. The speculum receives light at the side of the microscope at D, and reflects it down through the objective to the object. If there is any obstacle in the way of attaching the small speculum in the vertical body as shown at C, it could be fitted to an adapting ring between the body and the objective.

PHYSICAL AND MATHEMATICAL SECTION.

December 7th, 1865.

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

A paper was read "On the November Meteors, as observed at Woodcroft, Cuckfield, Sussex, November 12–13, 1865," by George Knott, Esq., F.R.A.S., communicated by Joseph Baxendell, F.R.A.S.

The night of November 12th being fine, Mrs. Knott and myself were enabled to watch under favourable circumstances for the meteor-shower, of which warning had been given at the last meeting of the Royal Astronomical Society.

An occasional examination of the sky during the earlier part of the night did not reveal a single meteor, but as a systematic watch was not commenced before midnight, too much weight must not be attached to this circumstance. Our station commanded a clear view of the southern half of the horizon, but towards the north the view was obstructed by the house. Between 12h. and 1h. a.m. we counted 39 meteors, giving an average of rather more than 0·6 per minute; the next 55m. added 61 to the number, giving an average of 1·1 per minute. After half an hour's interval we resumed our watch at 2h. 25m. a.m., and between that hour and 3h. 5m., when we ceased observing, we noted 55 meteors, showing that the average had risen to 1·4 per minute. The observations of the last 40 minutes showed clearly that the *radiant point* was in the immediate vicinity of the star ζ Leonis,

or, perhaps, between that star and ε and μ of the same constellation, the neighbourhood in fact of what the Rev. C. Pritchard happily terms the "*apex of the earth's way.*" The paths of a few meteors seemed to suggest a second *radiant point* in the neighbourhood of β Tauri, but the observed flights were too few to afford satisfactory evidence on the point.

We remarked a strong tendency of the meteors *to occur in groups,* four or five, and in some cases more, appearing one after the other in quick succession, followed by a lull, during which none were seen. We did not notice any of *very remarkable brilliancy,* they ranged for the most part from that of stars of the first magnitude downwards, in the majority of instances leaving a train behind them, which in several cases remained visible for some little time after the main body of the meteor had disappeared.

Among such numbers it would hardly be possible, in all likelihood, to identify individuals; I may just notice, however, that at 2h. 42m. 30s. a bright meteor passed precisely over α Orionis, leaving a train which remained visible for a few seconds, on which the star had the curious appearance of being *threaded.* The meteor passed in the direction of γ Orionis, its time of flight being about one second, and the length of its path, of which α Orionis was about the middle point, 10° or 12°. Of course, in the case of an unpractised observer, these data must of necessity be very rough, and I much regret that I was not at the time acquainted with Mr. Herschel's ingenious alphabetic chronometer.

In the last number of the Abbe Moigno's serial "Les Mondes," I find the following observations by M. Coulvier-Gravier;—"Night of the 12–13 November. First hours of the night up to 4 a.m., 237 meteors observed, or about 29·7 per hour; from 4h. to 5h., 96 meteors, or on the average 1·6 per minute; from 5h. to 6h., 43 meteors, or 0·7 per minute." A comparison of these results with those of our own obser-

vations, would seem to indicate not only that there was no very material increase in the average number of meteors per minute as the morning advanced, but also that the display was already on the decline before daylight put an end to observation. At the same time it is to be noticed that, according to the observations of M. Coulvier-Gravier, meteors in more than average numbers, indeed very considerably so, were to be seen on the night of the 13–14 November; he observed, "although the sky was almost constantly clouded, 72 meteors, of which 36 appeared between the hours of 4 and 6 a.m., when only 0·2 of the sky was clear." But in any case, so far as present accounts are concerned, it would appear that the display this year was by no means a very extraordinary one.

The position of Mr. Knott's Observatory is Lat. 51° 0′ 41″ N., Long. 0°. 0′. 34″. W.

In the discussion which ensued Mr. Thomas Heelis, F.R.A.S., pointed out the advantage first suggested by Quetelet, in 1841, of recording not only the position of the radiant point but also the mean distance from such point at which the meteors became visible; and Mr. Baxendell concurred in this, stating that he had himself, in the display of 1833, noted that the meteors had apparently lain in at least two strata, each stratum having its peculiar mean distance of apparition from the radiant point.

Mr. Heelis also called attention to the danger which appeared to him to exist of forming a theory of meteors from observations taken from a one-sided point of view, and urged that as the observations of M. Coulvier-Gravier, in France, had led him to the conclusion that all meteors were atmospheric, whilst the form adopted by the British Association had been framed by a committee the members of which regarded all meteors as cosmical, each form should in the interests of truth be so far altered as to inclose, when

practicable, not only the particulars already noted, but also some of the atmospheric conditions, both at the time of observation and also at a given time afterwards, which Mr. Baxendell suggested should not be less than 24 hours.

Exception was likewise taken to the exclusion, by the members of the committee, of small meteors from their catalogue.

Mr. Baxendell added that to his mind one of the greatest objections to the cosmical theory was that the constant deposit during thousands of years of the remains of meteors upon the surface of the earth, had not altered the time of diurnal rotation.

The general opinion of the section appeared to be that no one origin ought to be exclusively assigned to meteors, and that both the cosmical and atmospheric theories, if pushed to the exclusion of each other, were wrong.

Mr. Baxendell referring to his paper "On the Variable Star S Delphini" (see page 28) read the following extract of a letter he had received from Mr. Knott:—

"I was much interested in your remarks on our different modes of drawing the light-curves of variable stars. You were quite correct in your statement that I have been in the habit of regarding deviations from an even curve as due mainly to errors of observation. I have, however, recently felt that this hypothesis was not quite a satisfactory one, and have been on the point of broaching the question to you once or twice; and the recent projection of observations of R Vulpeculæ has strongly confirmed my suspicions. I enclose a projection of my observations of this star for the last maximum and minimum. You will at once see the marked dislocation in the ascending curve. An even curve would give an apparent error of observation on August 24 amounting to about half a magnitude; yet my light estimates on that day were R Vulpeculæ$=e+2=f=g-3$, the magnitudes of the comparison stars being 9·5, 9·7, and 10·0, and

the resulting values of the magnitude of R for the night being therefore 9·7, 9·7, 9·7. It is difficult to imagine I could have made an error of half a magnitude here! The descending curve is, you will see, very regular. It seems to me that this is rather an interesting point in variable star observations, and worthy of some study."

The projection of the observations of R Vulpeculæ referred to in this extract was exhibited to the meeting, and the general impression of the members present seemed to be that the irregularity in the ascending part of the curve could not fairly be attributed to errors of observation.

Ordinary Meeting, December 26th, 1865.

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

Mr. Henry Simpson, M.D., was elected an Ordinary Member of the Society.

Mr. Binney F.R.S., F.G.S., exhibited some singular calcareous nodules found in the lower coal seams of Lancashire and Yorkshire, full of beautiful specimens of fossil wood, showing structure even to the smallest striæ of the tubes. These nodules were found in several seams of coal, but were always associated, so far as yet known, with beds of fossil shells lying immediately above them.

In one instance the beds occurred in the following descending order, namely : —

	Ft.	In.
1. Black shale full of shells of the genera *Aviculopecten*, *Goniatites*, *Posidonia*, &c., and containing calcareous concretions enclosing similar shells	1	6
1. Seam of caking coal with the nodules containing the fossil plants...............................	2	0
3. Floor of fire clay and gannister, full of *Stigmaria ficoides*	2	6

The fossil wood is found in nodules dispersed throughout the coal, some being spherical, and others elongated and flattened ovals, varying in size from the bulk of a common pea to eight and ten inches in diameter. In some portions of the seam of coal the nodules are so numerous as to render it

utterly useless, and they will occur over a space of several acres, and then for the most part disappear and again occur as numerous as ever. For a distance of twenty-five to thirty miles the nodules occur in this seam of coal in more or less abundance, but always, so far as yet known, containing the same plants. Fossil shells are rarely met with in the nodules found in the coal, but they occur abundantly in the large calcareous concretions found in the roof of the mine, and are there associated with *Dadoxylon* containing *Stembergia* piths, which plant had not been noticed in the coal, and *Lepidostrobus*. So far as his experience extended, the nodules in the coal were always found associated with the occurrence of fossil shells in the roof, and were probably owing to the presence of mineral matter held in solution in water and precipitated upon or aggregated around certain centres in the mass of the vegetable matter now forming coal before the bituminization of such vegetables took place. No doubt such nodules contain a fair sample of the plants of which the seams of coal in which they are found were formed, and their calcification was most probably in a great measure due to the abundance of shells afterwards accumulated in the soft mud now forming the shale overlying the coal. These shells, on their decomposition, would yield most of the minerals now found in the fossil wood, whilst the surrounding salt water and vegetables would supply the remainder.

The specimen of *Sigillaria vascularis* exhibited was of an irregular oval shape, one foot three inches in circumference, had the ribs and furrows well shown on the outside of the decorticated stem, and afforded evidence of the structure of the original plant from the centre to the circumference. In the middle was a light coloured cylinder of about an inch in diameter, which appeared to be composed of carbonate of lime and carbonate of magnesia. The remainder of the specimen was of a much darker colour. By the kindness of our President an analysis was made in his laboratory, by Mr. Browning,

of a fair sample of the bulk of the dark part of the specimen. This gave

Sulphates of potash and soda	1·62
Carbonate of lime	45·61
Carbonate of magnesia	26·91
Bisulphide of iron	11·65
Oxides of iron	13·578
Silica	0·23
Moisture	0.402

The minutest vessels of the central axis and the internal radiating cylinder of the plant, with their finely striated sides, were preserved nearly as perfectly as in the living plant, without affording evidence of disarrangement from pressure or chemical change.

From the position where the calcareous nodules occur, namely in the middle of the seam of coal, they must have have been formed when the coal was in a soft and pulpy state and in the same shape and condition in which they are now found, something similar to such nodules in a peat bog of the present day. Instances have been known of hazel nuts placed in a damp calcareous deposit having had all their kernels removed and replaced by carbonate of lime while the woody portion of the nutshell remained little altered, but in this case the form of the starchy granules and original cellular tissue had not been preserved.

From the analysis previously given it is evident that the waters in which the nodules were formed contained a considerable amount of sulphuric acid, probably as much as would act on the cellular tissue and woody fibre of the vegetables so as to convert them into colloids. If this be assumed to be the case, then we might by the laws of liquid diffusion given by Mr. Thomas Graham, F.R.S., Master of the Mint, in his valuable paper printed in the *Philosophical Transactions* for 1861, suppose that the crystalloids now forming the light coloured cylinder in the middle of the specimen could have a

free passage from the circumference to the centre, and replace molecule by molecule the particles of the original vegetable, and all its beautiful and delicate structure just as we now see it preserved in the stone. However, before dialysis could be held to account satisfactorily for the phenomena above stated, a good many experiments on recent woods would have to be made, and more attention devoted to the subject than he (Mr. Binney) would be able to give.

The specimens exhibited were a portion of those described in a paper in the *Philosophical Transactions* of this year.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

December 18th, 1865.

J. B. Dancer, F.R.A.S., in the Chair.

Mr. Parry exhibited some sections of fossil wood and Echinus spines, most beautifully cut by Mr. John Butterworth, of Oldham, and presented some of the slides to the Section.

Mr. Parry also presented to the meeting, for distribution among the members, mounted slides of the contents of a shark's stomach, from the Madras coast, consisting almost entirely of Diatomaceæ.

Mr. Hurst then made a few remarks on late improvements in illuminating opaque objects under the higher powers of the microscope. He said they consisted of three different methods. Firstly, that of H. E. Smith, of Kenyon College, America, described in the "English" *Mechanics' Magazine* of the 20th October, 1865, in an extract from the American *Journal of Science and Arts.* This gentleman employed a box, or adaptor, between the object glass and the

Wenham's Prism of the binocular, with a side perforation opposite to which was a small silver reflector or a common thin glass cover, acting as a mirror and capable of adjustment to any angle—thus enabling it to throw the rays of light admitted by the side aperture through the object glass down on to the object itself.

The disadvantage of this method is that all adaptors cause unsteadiness, and however skilfully constructed injure the accurate centering of the object glass, and while on the one hand the thin glass cover appears to produce some distortion of the image, the reflector so near the object necessarily casts off a number of the rays proceeding from it. This plan also seems to require lamp light and the use of a condenser. Messrs. Smith and Beck appear to have patented the use of the thin glass cover.

Secondly, a modification of the foregoing by Mr. Dancer, of this Section, who places the thin glass or reflector between the eyepiece and the Wenham prism, cutting an aperture in the body of the microscope to admit the light. This dispenses with the objection inherent to adaptors, and theoretically seems the most perfect of these new methods; but Mr. Hurst's experience in its use was as yet too limited to form an opinion. He hoped however to report on the subject at the next meeting.

Thirdly, that invented by Mr. Dancer, who places a circular mirror over the oblique tube of the microscope, previously removing the eye piece: the light is thrown down to the Wenham's prism, and thence through the objective on to the object. The only disadvantage of this method was that of not admitting of binocular vision; otherwise its simplicity, cheapness, and great facility of adjustment render it far preferable to the others, while its effects are fully equal to theirs. It answers moreover equally well by day or lamp light, and does not require a condenser. to be used. Mr. Hurst thought every binocular microscope

would be fitted with it when their owners had seen its working.

Mr. Hurst wished meanwhile to draw the particular attention of the members to the extraordinary beauty and clearness with which opaque objects—hitherto the despair of microscopists—were displayed by these methods of illumination, some being shown as clearly as if enlarged into a relatively gigantic model and viewed by the naked eye. Another peculiarity connected with them is, that as the object glass itself acts as a condenser, the amount of light is increased with the magnifying power of the object glass, contrary to the effect of other modes of illumination.

Mr. Hurst thought the subject was in its infancy and that great improvements would yet be made, but that the idea of Mr. H. E. Smith, of making the object glass its own illuminator, would prove to be one of the greatest steps in modern microscopic science, and, as improved upon by Mr. Dancer, it was one so costless in price and rapid in its adjustment that every microscopist, however economical either of time or money, could readily avail of its assistance.

Mr. Coward then exhibited some interesting plants from India, illustrating abnormal forms of different natural families, especially of Leguminoseæ.

Ordinary Meeting, January 9th, 1866.

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

Mr. Eddowes Bowman, M.A., gave an extended series of striking illustrations of the principal phenomena of polarised light.

PHOTOGRAPHICAL SECTION.

December 14th, 1865.

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

Mr. Joseph Sidebotham read a paper "On the Application of Measuring Rods to Photographic Pictures."

The author referred to a paper on this subject read last Session (*Proceedings*, vol. iv., p. 113), in which he advocated the use of graduated rods being placed in certain positions in buildings or landscapes of which photographs were to be taken, whereby correct measurements might be afterwards made on the finished picture. He now briefly alluded to certain corrections which would be necessary before these measurements could be considered perfectly accurate.

Professor C. Piazzi Smyth had made great use of this plan during his investigations at the great pyramid last winter, and had kindly allowed a selection of forty of his pictures to be exhibited to the members as illustrations. The pictures were exhibited by the oxyhydrogen light on the screen, and were admirable photographs. They consisted of a series of views of the exterior and interior of the tomb of King Shafra, recently excavated near the great sphynx. A series of four of these, taken to show the correct orientation of one of the passages, are very remarkable, having been taken two minutes before twelve, twelve o'clock, and two minutes past, true astronomical time. Views were also exhibited of the entrance to the great pyramid, the socket in which the corner stone had rested, also views of the niche in Queen's Chamber, and the mysterious coffer in the so-called King's Chamber, the object of so much interest and speculation. These interior views were taken by the aid of the magnesium light, and, considering the many difficulties to be overcome, are very good photographs. The divisions on the measuring rods surrounding the coffer are exceedingly plain, and by the application of a pair of compasses a tolerably correct measurement may be obtained.

A paper was then read "On Celestial Photography," by A. Brothers, F.R.A.S.

The credit of having produced the first photograph of a celestial object is generally given to the late Mr. Bond, of Cambridge, U.S.; but it appears from a paper by Professor H. Draper, of New York, published in April, 1864, that in the year 1840 his father, Dr. J. W. Draper, was the first who succeeded in photographing the moon. Dr. Draper states that at the time named (1840), "it was generally supposed the moon's light contained no actinic rays, and was entirely without effect on the sensitive silver compounds used in Daguerreotyping." With a telescope of five inches

aperture Dr. Draper obtained pictures on silver plates, and presented them to the Lyceum of Natural History of New York. Daguerre is stated to have made an unsuccessful attempt to photograph the moon, but I have been unable to ascertain when this experiment was made.

Mr. Bond's photographs of the moon were made in 1850. The telescope used by him was the Cambridge (U.S.) refractor of fifteen inches aperture, which gave an image of the moon at the focus of the object glass two inches in diameter. Daguerreotypes and pictures on glass mounted for the stereoscope were thus obtained, and some of them were shown at the Great Exhibition of 1851, in London. Mr. Bond also proved the advantage to be derived from photographs of double stars, and found that their distances could be measured on the plate with results agreeing well with those obtained by direct measurement with the micrometer.

Between the years 1850 and 1857 we find the names of Father Secchi in Rome, and MM. Berch and Arnauld in France; and in England, Professor Phillips, Mr. Hartnup, Mr. Crookes, Mr. De la Rue, Mr. Fry, and Mr. Huggins. To these may be added the name of Mr. Dancer, of Manchester, who, in February, 1852, made some negatives of the moon with a four and a-quarter inch object glass. They were small, but of such excellence that they would bear examination under the microscope with a three-inch objective, and they are believed to be the first ever taken in this country. Mr. Baxendell and Mr. Williamson, also of Manchester, were engaged about the same time in producing photographs of the moon.

The first detailed account of experiments in celestial photography which I have met with is by Professor Phillips, who read a paper on the subject at the meeting of the British Association at Hull in 1853. Professor Phillips says:—"If photography can ever succeed in portraying as much of the moon as the eye can see and discriminate, we shall be able to

leave to future times monuments by which the secular changes of the moon's physical aspect may be determined. And if this be impracticable—if the utmost success of the photographer should only produce a picture of the larger features of the moon, this will be a gift of the highest value, since it will be a basis, an accurate and practical foundation of the minuter details, which, with such aid, the artist may confidently sketch." The pictures of the moon taken by Professor Phillips were made with a six and a-quarter inch refractor by Cooke. It is of eleven feet focus, and produces a negative of one and a-quarter inches diameter, in thirty seconds. Professor Phillips does not enter very minutely into the photographic part of the subject, but he gives some very useful details of calculations as to what may be expected to be seen in photographs taken with such a splendid instrument as that of Lord Rosse. It is assumed that an image of the moon may be obtained *direct* of twelve inches diameter, and this when again magnified sufficiently would show "black bands twelve yards across." What may be done remains to be seen, but up to the present time the Professor's anticipations have not been realised.

We have next, from the pen of Mr. Crookes, a paper communicated to the Royal Society of London, in December, 1856, but which was not read before that society until February in the following year. Mr. Crookes appears to have obtained good results as early as 1855, and, assisted by a grant from the Donation Fund of the Royal Society, he was enabled to give attention to the subject during the greater part of the year following. The details of the process employed are given in the paper with much minuteness. The telescope used was the equatorial refractor at the Liverpool Observatory, of eight inches aperture, and twelve and a-half feet focal length, producing an image of the moon 1·35 inch diameter. *The body of a small camera* was fixed in the place of the eyepiece, so that the image of the moon was received

in the usual way on the ground glass. The chemical focus of the object glass was found to be eight-tenths of an inch beyond the optical focus, being over-corrected for the actinic rays. Although a good clock movement driven by water power is applied to the telescope, it was found necessary to follow the moon's motions by means of the slow-motion handles attached to the right ascension and declination circles, and this was effected by using an eyepiece, with a power of 200 on the finder, keeping the cross-wires steadily on one spot. With this instrument Mr. Hartnup had taken a large number of negatives, but owing to the long exposure required he was not successful; but with more suitable collodion and chemical solutions, and although the temperature of the observatory was below the freezing point, Mr. Crookes obtained dense negatives in about four seconds. Mr. Crookes afterwards enlarged his negatives twenty diameters, and he expresses his opinion that the magnifying should be conducted simultaneously with the photography by having a proper arrangement of lenses, so as to throw an enlarged image of the moon at once on the collodion plate; and he states that the want of light could be no objection, as an exposure of from two to ten *minutes* would not be "too severe a tax upon a steady and skilful hand and eye."

In an appendix to his paper Mr. Crookes gives some particulars as to the time required to obtain negatives of the moon with different telescopes, from which it appears that the time varied from six minutes to six seconds. The different results named must, I conclude, have been caused not so much by the differences in the instruments as in the various processes employed, and in the manipulation. I must observe, also, that it is not stated whether all the experiments were tried upon the *full* moon, a point materially affecting the time.

Mr. Grubb read a paper on this subject before the Dublin Photographic Society on May 6th, 1857. After referring to

the fact that he found the actinic focus of his object glass to be longer than the visual (thus agreeing with Mr. Crookes) he states it to be generally understood that in a compound object-glass made as nearly achromatic as possible, the actinic focus is shorter than the visual. The most valuable portion of Mr. Grubb's paper is the suggestion for a piece of apparatus to be attached to the part connected with the telescope for holding the dark frame, which he proposes may be so arranged as to follow the moon's motion in declination; and he gives the following description of a contrivance used by Lord Rosse, and which is suitable for telescopes not equatorially mounted:—"On a flat surface attached to the telescope, and parallel to the plane of the image is attached a sliding plate, the slide being capable of adjustment to the direction of the moon's path at the time of operating. The slide is actuated by a screw moved by clockwork, and having a governor or regulator of peculiar construction, which acts equally well in all positions. The clockwork being once adjusted requires no change; but the inclination of the slide must be effected by trial for the moon's path at the time of taking the photograph." This idea originated with Mr. De la Rue, Lord Rosse's share in it arose from his having applied a clock motion to the apparatus.

The telescope used by Mr. Grubb is $12\frac{1}{10}$ inches aperture and twenty feet focus, giving an image $2\frac{1}{16}$ inches diameter in from ten to forty seconds.

The next contribution on this subject is by Mr. Fry, who, in 1857, commenced his experiments on the moon with an equatorial telescope, the property of Mr. Howell, of Brighton. The object glass of this instrument is eight and a-half inches diameter and eleven feet focus, and gave an image of the full moon in about three seconds, but under very favourable circumstances a negative was made in a single second. The size of the image is not stated, but it must have been about one and a-quarter inches diameter. Mr. Fry appears to have

removed the eyepiece of the telescope, and in its place a board was fixed having a screw adjustment, so that a plate-holder could be moved backwards and forwards on the board (graduated to tenths of an inch) for the purpose of finding the actinic focus, which was three quarters of an inch beyond the visual. He found that this position of the chemical focus was variable, owing, as he thought, to the varying distance of the moon from the earth, but, as suggested by Mr. De la Rue, it might arise from the length of the telescope tube having altered through change of temperature.

In 1858 Mr. De la Rue read an important paper before the Royal Astronomical Society, from which it appears that the light of the moon is from two to three times brighter than Jupiter, while its actinic power is only as six to five, or six to four. On December 7th, 1857, Jupiter was photographed in five seconds and Saturn in one minute, and on another occasion the moon and Saturn were photographed just after an occultation of the planet in fifteen seconds.

The report of the council of the Royal Astronomical Society for 1858 contains the following remarks:—

"A very curious result, since to some extent confirmed by Professor Secchi, has been pointed out by Mr. De la Rue, namely, that those portions of the moon's surface which are illuminated by a very oblique ray from the sun possesses so little photogenic power, that, although to the eye they appear as bright as other portions of the moon illuminated by a more direct ray, the latter will produce the effect called by photographers, solarisation, before the former (the obliquely-illuminated portions) can produce the faintest image."

And the report also suggests that the moon may have a comparatively dense atmosphere, and that there may be vegetation on those parts called seas.

At the meeting of the British Association at Aberdeen, in 1859, Mr. De la Rue read a very valuable paper on celestial photography. An abstract of this paper was published at the

time in The British Journal of Photography, and in August and September of the following year further details of Mr. De la Rue's method of working were given in the same Journal. The processes and machinery employed are so minutely described that it is unnecessary here to say more than that Mr. De la Rue commenced his experiments about the end of 1852, and that he used a reflecting telescope of his own manufacture of thirteen inches aperture and ten feet focal length, which gives a negative of the moon averaging about $1\frac{1}{10}$th of an inch in diameter. The photographs were at first taken at the side of the tube after the image had been twice reflected. This was afterwards altered so as to allow the image to pass direct to the collodion plate, but the advantage gained by this method was not so satisfactory as was expected. In taking pictures at the side of the tube, *a small camera box* was fixed in the place of the eyepiece, and at the back a small compound microscope was attached, so that the edge of a broad wire was always kept in contact with one of the craters on the moon's surface, the image being seen through the collodion film at the same time with the wire in the focus of the microscope. This ingenious contrivance in the absence of a driving clock was found to be very effectual, and some very sharp and beautiful negatives were thus obtained. Mr. De la Rue afterwards applied a clockwork motion to the telescope, and his negatives taken with the same instrument are as yet the best ever obtained in this country.

The advantage of the reflecting over the refracting telescope is very great, owing to the coincidence of the visual and actinic foci, but it will presently appear that the refractor can be made to equal if not excel the work of the reflector.

Mr. De la Rue's paper (as published in the report of the British Association,) contains some extremely interesting particulars as to the mode of obtaining stereoscopic pictures of the moon, and diagrams are given showing the effects of

the moon's libration. The most beautiful stereoscopic prints of the moon are those by Mr. De la Rue. Mr. Fry also was very successful in this branch of celestial photography.

In this brief history of the subject of celestial photography, I have not referred to anything which has been done in making photographs of the solar spots, but the matter must not be altogether passed over. The first step in this direction appears to have been taken in France, in 1845, by MM. Fizeau and Foucault, but it is chiefly due to the efforts of Mr. De la Rue that so much useful work has been done in heliography. In 1860 Mr. De la Rue and his staff of assistants performed one of the greatest feats yet recorded in this branch of the art of photography, having succeeded in obtaining several beautiful negatives of the various phenomena seen only during total eclipses of the sun, and two negatives were obtained during the totality. One question of much interest to astronomers was determined by this great experiment. The red prominences or flames generally seen as issuing from the edge of the moon were proved to belong to the sun. Photographs of the sun are taken daily when the weather is favourable at the Kew Observatory, and also by Professor Selwyn, at Ely. With the Kew photoheliograph pictures of the sun spots have been made on the scale of three feet to the sun's diameter. Much, however, remains to be done. The light of thesun is much in excess of what is required to obtain a collodion picture, so that the loss of light consequent on the necessary interposition of lenses and the distance of the plate from the instrument can be no objection; and for these reasons I have very little doubt that with apparatus suitably arranged photographs of spots and groups of spots will be obtained of very much larger diameter than any yet taken.

The *Quarterly Journal of Science* for April, 1864, contains the next important paper on celestial photography. It is by Dr. Henry Draper, one of the Professors at the New York

University. On his return to America, after paying a visit to Parsonstown, where he had the advantage not only of making some observations with Lord Rosse's large reflector, but also of seeing the method there pursued in grinding and polishing mirrors, stimulated by what he had seën, it was determined to build an observatory, and to construct an instrument to be devoted solely to celestial photography. The speculum used by Dr. Draper is fifteen and a-half inches in diameter, and twelve and a-half feet focal length; but this was afterwards superseded by one of glass on Foucault's principle. The great labour involved in a work of this character may be judged of by the fact that Dr. Draper ground and polished more than 100 mirrors, varying in diameter from nineteen inches to a quarter of an inch; but he appears at last to have secured a good instrument. The chief points to be noticed in this article are, that instead of driving the telescope in the usual way by means of a clock, the frame carrying the glass plate was made to move on the plan previously referred to. Instead of clockwork to effect this motion, an instrument called a "clepsydra" was used. It has a weight and a piston rod, which fits into the cylinder filled with water, which is allowed to escape by means of a stopcock, and can be regulated with great exactness, so as to follow the object. The large number of 1,500 negatives are stated to have been taken at this observatory, some of which would bear magnifying twenty-five diameters (the paper says *times*, but I assume this to be an error, as a negative must be very bad if it will not bear more than five diameters, or twenty-five times.) As the average size of the negatives was $1\frac{4}{10}$ inch, an increase of twenty-five diameters would give an image of the moon nearly three feet in diameter. I have not seen the prints from these negatives, and have never heard anything of the quality of the work produced by this telescope; but it may be stated that Dr. Draper writes as if the negatives were of the best quality, and encourages others to follow his example.

Nearly a quarter of a century has elapsed since the moon was first photographed in America, and our friends on that side of the Atlantic have not been idle in the interval. To an American gentleman we are indebted for the best pictures of our satellite yet produced, and it is difficult to conceive that anything superior can ever be obtained; and yet with the fact before us that Mr. De la Rue's are better than any others taken in this country, so it may prove that even the marvellous pictures by Mr. Rutherford may be surpassed.

Mr. Rutherford appears, from a paper in the *American Journal of Science* for May of the present year, to have begun his work in lunar photography in 1858 with an equatorial of eleven-and-a-quarter inches aperture, and fourteen feet focal length, and corrected in the usual way for the visual focus only. The actinic focus was found to be seven-tenths of an inch longer than the visual. The instrument gave pictures of the moon, and of the stars down to the fifth magnitude, satisfactory when compared with what had previously been done, but did not satisfy Mr. Rutherford, who, after trying to correct for the photographic ray by working with combinations of lenses inserted in the tube between the object glass and sensitive plate, commenced some experiments in 1861 with a silvered mirror of thirteen inches diameter, which was mounted in a frame and strapped to the tube of the refractor. Mr. Rutherford enumerates several objections to the reflector for this kind of work, but admits the advantage of the coincidence of foci. The reflector was abandoned and a refractor specially constructed of the same size as the first one, and nearly of the same focal length, but corrected only for the chemical ray. This glass was completed in December last, but it was not until March 6th of the present year that a sufficiently clear atmosphere occurred, and on that night the negative was taken from which the prints were made, and through the kindness of Dr. Roscoe I now have the pleasure of showing them to you.

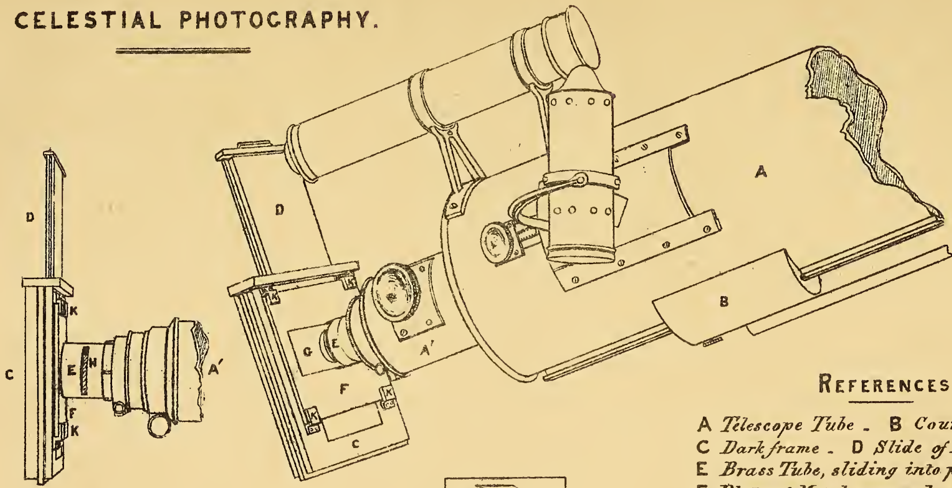
I have entered somewhat minutely into particulars of what has been done in this branch of our favourite art, in order that we may have before us a kind of summary or index of the work done up to the present time, so that those who desire further information may at once refer to the authorities quoted. It may be asked why it was thought necessary to draw up this paper, as Mr. De la Rue and others have said almost all that is necessary to enable anyone to take up the subject and to pursue it sucessfully.

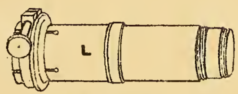
It is true there are very elaborate papers, and from their perusal I have derived much useful information, but at the same time it must be confessed their very elaborateness deterred me, for a long time after I possessed the necessary apparatus, from commencing the experiments which have since afforded me so much enjoyment.

Every writer on this subject speaks of the difficulties encountered from optical, instrumental, and atmospheric causes; and to this may be attributed the fact that we have so few of our amateur astronomers giving their attention to the subject. Another reason may be that comparatively few of those who possess telescopes may have the necessary photographic knowledge; but surely some friend having this knowledge might be found who would be willing to spare a few hours occasionally to assist in taking negatives of the stars, planets, or of the moon. The reason, then, why this subject is brought before your notice this evening is, that it is believed that the apparatus I use is, in some particulars, more simple than any heretofore described, and as it can be used with any kind of telescope, a greater number of amateurs than are now engaged in it may be induced to follow this fascinating branch of photography.

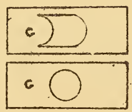
It will have been noticed that when particulars of the apparatus have been given the writer has spoken of a *small camera*, which has been fixed at the eye piece end of the telescope. As to how this was effected I have seen no description,

CELESTIAL PHOTOGRAPHY.





BARLOW'S LENS.



DIAPHRAGMS.

REFERENCES.

A *Telescope Tube.* – B *Counterpoise Weight.*
C *Dark frame.* – D *Slide of Dark frame.*
E *Brass Tube, sliding into place of Eye-piece.*
F *Plate of Metal screwed to* E. – G *Diaphragm.*
H *Opening in tube for Diaphragm.* – K *Clips for fixing* C *to* E. – L *Barlow's Lens, sliding into place of Eye-piece.*

Drawn & Photolithographed by
A. BROTHERS, *Manchester.*

and as no camera box is required I need not enter into any supposition as to the mode in which it may be done. Before deciding what was necessary to be done, it occurred to me that the telescope tube itself is the camera, and all that was required was the means of fixing the dark frame or plate holder. If the telescope be pointed to the moon, the eye piece removed, and a piece of ground glass held between the eye and the aperture, the image will be seen on the glass, and we require then the means of holding the sensitive plate steadily near the same place. All that is needed is a brass tube about four or five inches long, of the size exactly fitting the tube of the telescope in the place of the eyepiece. In some cases the sliding tube of the eyepiece may be unscrewed and used for this purpose. At one end of this tube a thread is cut and is made to screw into a piece of metal plate (in the centre of which is a circular aperture of the same size as the tube), of the same dimensions as the dark frame. Attached to the plate holder are clips accurately fitting the brass plate, but so that the frame will easly slide off and on without disturbing the telescope. This is all the additional apparatus required to enable photographs of the moon or any other celestial object to be made. A separate frame for the ground glass is not necessary: it must be cut to fit the dark frame, and while in use can be held by slight springs fixed inside the frame at the sides.

The accompanying photo-lithographed copy of a drawing shows the arrangement of the apparatus when in its place for taking a negative, and renders further description unnecessary. The method for ascertaining the actinic focus may be stated in a few words. With the rack motion adjust the focus for distinct vision on the ground glass, and then mark the tube E, and also the sliding part of the telescope. Although very unlikely to be of the slightest use, unless taken with a reflecting telescope, a picture may now be taken; it will at least serve to give some idea of the proper exposure.

If the chemical and visual foci are not coincident the image will have a blurred appearance. Before exposing the next plate, turn the adjusting screw so as to lengthen the tube about the 16th of an inch, and so proceed until, by the greater distinctness of the image, it is seen that the chemical focus is found. At every change of the focus a slight mark should be made on the tube, and when the true focus is satisfactorily determined the marks should be made distinctly visible; and in all future experiments with the same instrument the focus will be always at or very near the same place. Should it be found that the indistinctness increases, it will of course be necessary to try in the other direction.

The appearances arising from atmospheric disturbances are very much the same as when the object is out of focus: experience alone will enable the operator to determine from which cause the defect proceeds.

It is assumed that the telescope is provided with a driving clock; when such is the case every care should be taken that all the parts are clean, and when necessary oiled or greased, so that the motions may be as smooth as possible.

In photographs of the moon in the phases, prior to and after the full, the side opposite to the sun is always too light, or "burnt up," while those parts near the terminator are often so dark that only the tops of the craters and peaks are visible, although in the telescope a clear and bright image can be seen. The cause of this must be that the exposure, if continued long enough to bring out all the eye can see on the darker side, would entirely obliterate the details on the brightly illuminated portions of the moon's surface. Mr. De la Rue's suggestion as to why the dark side of the moon has so little actinic effect, has been already referred to. I would suggest that, as the light of the full moon is 100,000 times weaker than that of the sun, the *twilight** on the moon's

* In the absence of an atmosphere on the moon there can be no *twilight* such as we have on the earth. The meaning, therefore, of this sentence may

surface must be very much less, and consequently the actinic effect of the light is lessened in the same way as at a corresponding time on the earth.

The question, then, of photographing the terminator is only one of time, and in order to remedy the defect spoken of to some extent, I have used diaphragms such as are shown in the drawing. In the tube E openings are made on opposite sides, and wide enough to admit the diaphragms to be used without touching the tube. The diaphragm must be of the proper length and width to shut off the moon's light until the plate is ready for exposure. The shape of the diaphragm will suggest which form should be used according to the moon's age. The exposure should be made with the full aperture for as many seconds as previous experiments have proved to be necessary for the bright side, and the diaphragm then gently moved and kept in motion, gradually approaching the darkened side. By this means the exposure may be regulated, and the great differences in the light and dark sides of the moon may be modified.

As to the processes employed, each experimenter must adopt the one he finds in his hands gives the best result. It seldom happens that two operators can produce the same effects with, *apparently*, the same chemicals. Experience has shown me that the ordinary patent plate glass (carefully selected, so as to be free from scratches and other defects,) is preferable to the white patent plate, having found that after a time the surface becomes covered with a kind of dew or "sweat," as it is termed, owing to the decomposition of some of the salts used in the manufacture. The collodion used was made for me by Messrs. Huggon and Co. of Leeds; it is very quick, free from structure, and suitable for iron development.

be misunderstood. It was intended to say that on those parts of the moon, enlightened only by the oblique rays of the sun, the light is so diminished that the actinic effect is lessened as it is on the earth shortly before sunset and during twilight, when it is well known that a much longer time is required to obtain a photograph.

I prefer to develope with an iron solution, using only sufficient to cover the plate; and with this developer and collodion, when the plate has been properly exposed, a negative can be obtained which will not require intensifying afterwards. Not having had sufficient experience with pyrogallic acid I cannot speak with confidence as to any advantage it may possess in giving fine texture to the negative. With the bath and collodion exactly in the proper state, there is no doubt that with this acid negatives may be had as quickly as with iron; but it is extremely difficult to have everything constantly in the best working order. Unless the greatest attention be given to this matter, the time of exposure is so much increased that iron, for this reason, must have the preference.

Upon the character of the image after development entirely depends the value of the enlargement to be made from it, and in this direction there is much room for improvement. Even in the best negatives yet made defects from this cause are very apparent. The microscopic photographs by Mr. Dancer have the finest texture, and will consequently bear greater magnifying than any other photographs I have ever seen, but the process by which they are made is not published.

The weather in this conntry is so very uncertain, and success in this branch of photography is so entirely dependent on the state of the atmosphere, that it is necessary to be always prepared to take advantage of a favourable night. I have a small cupboard placed in a convenient part of my house where there is a supply of water, and the temperature is always much above the air outside. This cupboard is just large enough to hold a small glass bath fixed at the proper angle ready for use, also the bottles for collodion, bath, developing and fixing solutions, and other little requisites. This arrangement is so convenient that when there is a prospect of getting a negative I can set the telescope, prepare the plate, and take a negative in less than ten minutes. But when there is a chance of two or three hours' work an assistant is desirable, as the best results can only be obtained

when one's attention is chiefly devoted to the careful adjustment of the apparatus connected with the telescope.

The convenience of the plan adopted may be judged of by the fact that on the evening of the partial eclipse of the moon, the 4th October last, in four hours I succeeded with the help of two assistants in taking no less than twenty negatives, and the telescope was several times disturbed to oblige friends who desired to see the progress of the eclipse through the instrument, but the apparatus was quickly re-adjusted, although possibly in some cases with slight loss of definition in the negative, through haste. At a previous meeting, I described how these negatives were made, but it may be interesting to refer to the fact that while the fifteenth of the series was taken the telescope was at rest. The clock had been disconnected for re-adjustment and it was forgotten when the plate was ready for exposure, consequently the moon had moved partly off the plate, and the negative shows a portion only; but the exposure was so short that the eye fails to detect any difference in the sharpness of this and the others, which were all taken when the clock had been watched and carefully regulated for the moon's motion. This fact is, I think, of some interest, as it shows that about the time of full moon when the light is of the greatest intensity, pictures may be made with telescopes not equatorially mounted.

My telescope is a refractor of five inches aperture and six feet focal length, giving an image of the moon averaging about eleven sixteenths of an inch in diameter. The actinic focus is one tenth of an inch longer than the visual. The object glass is of Munich manufacture, and is mounted by Mr. Dancer on the Sisson's or English plan with double polar axis. The hour circle is twenty-six inches in diameter, and is used also as the driving wheel, having teeth cut in the edge, in which a screw works connected with the driving clock by a rod, and which can be instantly disconnected by means of a cam. The object glass is an excellent one, and the mounting is everything that can be desired.

The negative taken direct in the telescope is but one step towards what we require, that is, the enlarged copy on paper. From the small negative a positive on glass must be made, say of twice or three times the diameter of the original. It will be quite unnecessary here to explain how the enlargement is to be made; but I may remark that the negative should be placed with the film side towards the copying lens, and the resulting positive copy must also be placed in the same way. The enlarged copy or negative will then give the true telescopic appearance of the moon. In the print of the full moon by Mr. Rutherford a mistake has been made, arising from the negative or positive copy having been placed the wrong way, and consequently the moon looks as if it had been photographed from the opposite side. The print is a very beautiful one in other particulars, but entirely worthless as a picture of the moon, as the eye can never see it as there represented.

I have sometimes taken two negatives on the same plate. It will be seen in the drawing that the dark slide is not quite central with the telescope, so that by reversing the plate after one exposure a second picture can be taken. In photographing the planets Mr. De la Rue has allowed the object to move on for a few seconds, the telescope meanwhile being at rest, and thus four or five negatives can be taken in a very short time on the same plate. It has occurred to me that by having a frame made "landscape way" instead of upright, and in place of having four clips such as K, there might be a kind of groove at top and bottom, so that after taking the first negative, and the light shut off, by moving the plate about an inch, at least three negatives might be taken on the same plate—or a "shifting back" might be adapted. The advantages of this plan are that different exposures might be tried, and the development continued for the one or two which promised the best results. This method would effect a great saving of time, which on a fine night is of much importance.

With the Barlow lens I have made some negatives which

have shown that when the same care has been taken to find the actinic focus negatives of a much larger size may be made, and in a very short time. The image is increased from eleven-sixteenths to one and a quarter inch, and the time of exposure at full moon was two seconds. The fittings of the lens are so arranged that three different sized negatives may be taken.

There are several other matters which it might have been desirable to refer to had time permitted, but they must stand over to a future occasion.

Mr. BAXENDELL said that some years ago Dr. Joule had obtained several beautiful negatives of the sun. The time of exposure was about $\frac{1}{200}$ of a second, and the apparatus for admitting and cutting off the light was not attached to the telescope, as in the Kew heliograph, but was mounted on a separate stand.

PHYSICAL AND MATHEMATICAL SECTION.

January 4th, 1866.

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

Mr. G. V. VERNON, F.R.A.S., M.B.M.S., was elected Honorary Secretary of the Section.

Mr. VERNON communicated the following "Remarks on the Barometric Disturbances during the Months of October, November, and December, 1865."

At the last meeting of the Physical Section attention was called to the great barometric disturbances in October and November of this year; and in order that their character may be seen at a glance I have drawn out my observations made twice daily in the subjoined curves.

The amount of oscillation in October was not remarkable for its excessive amount, but for the very sudden changes which took place towards the end of the month, say from the 23rd to the 31st.

For November the reading was high until the 15th, when a sudden change commenced, the great depression of the 22nd showing the extremely low reading of 28·240 inches. The amount of oscillation during the first half of the month only amounted to about half an inch per week, leaving more than six inches for the amount of oscillation during the last half of the month, against an average of 5·851 inches for the last seventeen years for the entire month.

The amount of oscillation for the two months of October and November for the last five years, in continuation of the values given in my paper in vol. i., 3rd series, of the Memoirs of the Society, are as follows: —

	OCTOBER.			NOVEMBER.		
Year.	Number of Oscillations	Amount of Oscillation.	Rainfall.	Number of Oscillations	Amount of Oscillation.	Rainfall.
		Inches.	Inches.		Inches.	Inches.
1861	13	5·087	1·230	15	7·491	3·878
1862	18	6·569	5·035	13	5·323	1·685
1863	12	4·648	6·242	14	7·496	2·904
1864	14	5·110	1·898	20	8·504	3·255
1865	14	6·361	5·005	15	7·390	2·770
Means of 17 Years..	15·5	5·851		13·8	5·804	

In the paper last alluded to I pointed out at page 3 that an excessive amount of rainfall was accompanied also by a large excess of barometric oscillation in every month but October, stating also that perhaps a longer period of observation might remove this apparent anomaly. The last five years, however, give exactly the same abnormal values for October, as reference to the small table annexed will show. October, 1863, with the least barometric oscillation, had the largest rainfall out of the five years. November conforms to

the law, which applies to all the months but October, the table showing that generally the rainfall increased with the amount of oscillation. Of course leaving 1863 out of the question would make October conform to the rule also; but it is these very abnormal months which are so difficult to account for, and which seem to bear out Mr. Baxendell's suggestion of some secondary disturbing cause operating during the month of October especially. (Vide Mr. Baxendell's paper on "Periodic Disturbances of Atmospheric Pressure," Memoirs, 3rd series, vol. i. p. 263.)

The amount of oscillation in December was 5·468 inches, the oscillations being thirteen in number. This is somewhat below the average, although the month was marked by such wide barometric readings as shown by the diagram.

The following Table of Rainfall for 1865 was also communicated by Mr. Vernon.

Old Trafford, Manchester.

Rain gauge 3 feet above the ground, and 106 feet above the sea.

Quarterly Periods. 1864.	Quarterly Periods. 1865.	1865.	Fall in Inches.	Average of 72 Years.	Differences.	No. of Days' Rain-fall in 1865.	Quarterly Periods. 1865.	Quarterly Periods. 1864.	Quarterly Periods for 72 Years.
Days.	Days.		Inches.	Inches.	Inches.		Inches.	Inches.	Inches.
		Jan. ...	3·112	2·469	+0·643	18			
38	48	Feb. ...	2·357	2·379	—0·022	17	7·143	7·722	7·149
		March.	1·674	2·301	—0·627	13			
		April...	1·082	2·025	—0·943	8			
39	36	May ...	3·187	2·368	+0·819	21	5·226	7·722	7·279
		June ...	0·957	2·886	—1·829	7			
		July ...	2·996	3·561	—0·565	15			
45	36	Aug....	3·840	3·596	+0·244	18	7·502	8·063	10·357
		Sept ...	0·666	3·200	—2·534	3			
		Oct. ...	5·005	3·834	+1·171	20			
49	44	Nov....	2·770	3·464	—0·694	16	8·518	7·133	10·523
		Dec. ...	0·743	3·225	—2·482	8			
171	164		28·389	35·308	—6·919	153	28·389	30·640	35·308

The rainfall for 1865 has been 6·919 inches below the average of the last seventy-two years, and 2·251 inches

below the fall for 1864, which was also comparatively a dry year. Although the first three months had about an average fall, the second three months had a fall greatly below the average. The same remark applies to the third and fourth quarters of the year. The fall of rain was remarkably small in June, September, and December; and it will also be seen that with the exception of the four months of January, May, August, and October, every month had a rainfall below the average. As compared with any other month, the rainfall in October was excessive. The number of days upon which rain fell was also greatly below that of many previous years.

Mr. Baxendell, F.R.A.S., read the following "Note on the Variable Star T Aquilæ."

In the communication made to this Section on the 12th of November, 1863 (*Proceedings*. vol, iii. p. 195), announcing the discovery, at Mr. Worthington's observatory, of the variability of T Aquilæ, I gave the times of one minimum and one maximum, but was unable to give a trustworthy value of the length of the mean period. The observations which I have since made have however enabled me to determine with tolerable exactness the times of three more maxima and two minima, thus affording the means of ascertaining the value of the mean period within very moderate limits of error.

The observed times of maximum and minimum and the corresponding magnitudes are:—

Maxima.

1863—Oct. 25.	8·9	magnitude.
1864—Aug. 23.	8·4	ditto
1865—June 30.	10·1	ditto
1865—Nov. 22	9·4	ditto

Minima.

1863—Aug. 24	11·3	magnitude.
1864—June 24	11·2	ditto
1865—Aug. 31	11·5	ditto

The times of minima appear to be more irregular than those of maxima; but the magnitudes at maximum vary more than those at minimum. As the times of maxima are evidently better suited for the determination of the mean period than those of minima, I have treated them in the usual way, and have obtained the following elements:—

Period = 152·4 days.
Epoch = 1865, January 24·1.

Calculating the times of maxima from these elements, and comparing them with the observed times, we have the following differences:—

C.—O.
days
−0·1
+1·7
−4·5
+2·9

The greatest difference is only 4·5 days, or about one *thirty-fourth* of the entire period, and the mean difference is 2·3 days, or one *sixty-sixth* of the whole period. T Aquilæ therefore belongs to the class of variables whose periods are moderately regular.

The diagram which accompanies this communication shows the mean form of the light-curve of T Aquilæ as derived from all the observations I have yet made. An examination of this curve has yielded the following results:—

Mean magnitude at maximum	= 9·20
Mean magnitude at minimum	= 11·20
Mean range of variation......	= 2·00 magnitudes
Mean magnitude...................................	= 10·40
Interval from minimum to maximum	= 64·0 days
Interval from maximum to minimum	= 88·4 days
Interval from minimum to mean magnitude...	= 43·0 days
Interval from mean magnitude to maximum...	= 21·0 days

Interval from maximum to mean magnitude... = 40·4 days
Interval from mean magnitude to minimum... = 48·0 days
Interval from mean magnitude before to mean magnitude after maximum .... = 61·4 days
Interval from mean magnitude before to mean magnitude after minimum .... = 91·0 days

A faint secondary maximum is indicated at 63 days after the principal maximum.

Mr. Knott, F.R.A.S., has kindly favoured me with the results of his observations of T Aquilæ for the past year, and from the following comparison it will be seen that, with the exception of the time of the August minimum, they are very closely accordant with my own:—

Maxima.

Baxendell.	Knott.
June 30—10·1 mag.	July 2—10·1 mag.
Nov. 22—9·4 mag.	Nov. 20—9·4 mag.

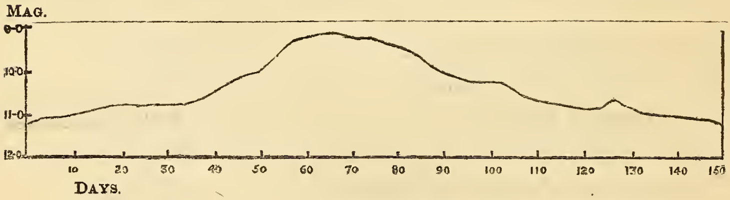
Minimum.

Aug. 31—11·5 mag.	Aug. 22—11·4 mag.

The place of T Aquilæ for 1865 is $20^h\ 5^m\ 25{\cdot}4^s + 15°\ 13{\cdot}4'$, and the calculated times of its maxima for 1866 are April 26·3 and September 25·7.

MEAN LIGHT-CURVE OF T AQUILÆ,

As derived from all the Observations made at Mr. Worthington's Observatory during the years 1863–5.



January 23rd, 1866.

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

M. le Marquis Anatole de Caligny, of Versailles, Civil Engineer, was elected a Corresponding Member of the Society.

Thomas Graham, F.R.S., &c., Master of the Mint; A. W. Hofmann, F.R.S., &c.; Joseph Prestwich, F.R.S , &c.; and Andrew Crombie Ramsay, F.R.S., &c., were elected Honorary Members of the Society.

A conversation took place respecting the cattle plague, in the course of which Mr. Baxendell stated that the results of inquiries he had made had led him to believe that the total mortality among cattle from plague and all other diseases, during the past year, had been very little, if at all, above the average rate of the last ten years; thus indicating that the plague had, to a great extent, displaced pleuro-pneumonia and other dangerous diseases, and that therefore no just cause at present existed for the feeling of alarm which prevailed throughout the country.

A paper was read entitled "Notes on a Section of Chat Moss, near Astley Station," by W. Brockbank, Esq.

By the kindness of my friend Henry Mere Ormerod, Esq., I have had the opportunity of examining a section of the strata at Chat Moss, exposed in the excavations at present being made to obtain marl for the reclamation of the moss.

A large area has been excavated to a depth of 45 feet from the surface, down to the red rock, giving the following thicknesses of the beds, viz.—

a.	Peat Moss...........................	17ft. 0in.
b.	Sandy Clay or Loam	1ft. 6in.
c.	Boulder Clay or Till..............	26ft. 6in.
d.	Soft Red Rock	——

Commencing with the red rock, the following particulars appeared worth recording:—

d. The soft red rock forming the base of the section is the lower member of the trias. The surface of the rock was covered with water, which prevented an examination of it, but a large detached fragment was found lying upon it, which was not at all worn.

c. The boulder clay or till had a thickness of 26ft. 6in., and contained boulders in abundance. It had no parting of sand or gravel, but small patches of red sand occurred about the middle, and of small gravel near the base. Small cubical fragments of coal were very plentiful, and many pieces of black shale. The following list of boulders was made from those found in the excavations, viz.—

Greenstone. One very large boulder, much striated, and many smaller ones. The largest boulders were of this rock.

Granites. Four varieties, all much waterworn; the grey and green granites decomposed and very friable.

Limestones. Dark limestone boulders plentiful, one containing a good specimen of "Productus Gigantens" on the unworn surface, the other side being deeply striated and much worn.

Light grey limestones, much worn.

Reddish limestones (permian), very little worn, and pronounced by the workmen to be "Bedford limestone," because worked at Bedford, in the neighbourhood. One fragment of this limestone occurred within four feet of the surface, and was very little worn, the corners quite sharp.

Red Marlstone Rock (permian), little worn.

Hœmatite Iron Ores (probably permian), worn into round lumps. These are eagerly sought by the workmen for "red raddle." They resemble the richest specimens of iron ore as found at the Patricroft workings, but are apparently much purer, and more like the hœmatites of North Lancashire.

Coal and Coal Shales. Abundant. The coal in small cubical fragments, but in great quantity. One fine specimen of coal shale with "Anthracosia robusta," well preserved, and one fine frond of "Neuropleris." Oval nodule of pyrites or "Brass lump" from the coal measures.

Sandstones. White sandstone boulders very frequent, containing fossils, probably calamites and sigillariæ, and chiefly resembling the white sandstones of the middle coal measures.

Ironstone. Nodules of claybound ironstones frequent; one very good example with fine septaria in centre.

The above list of boulders possesses some features of considerable interest. The granites and greenstones are very much the same as those found in the clays around Manchester, and which are generally supposed to have come from Cumberland and Westmoreland. These are much worn and have deep striæ.

The limestones are such as occur in the Pendle district, and their presence confirms the author in his belief that the current which denuded the coal of the high-lying seams of Lancashire and as far north as Ingleborough, and scattered the boulders which we find in this clay, came from the northwards, and probably from the neighbourhood of Ingleborough. The quantity of coal in small fragments was very remarkable, and evidences very considerable denudation of the coal measures, as also the presence of so many boulders of coal measure sandstones and shales, such as occur throughout the district lying to the north of Chat Moss. The unworn fragments of the permian limestones and marlstones is an

additional evidence, their outcrop being in the neighbourhood immediately to the northwards.

Mr. Ormerod informs me that the surface of the boulder clay is in ridges or long undulations, and that it frequently sinks into deep hollows, making the thickness of its moss covering very variable.

b. The clay is capped by a seam 1ft. 6in. thick of loamy sand, which contains the roots and stumps of oak trees, birches, and hazels, the stumps being broken off about a foot from the surface, and the trees lying near, embedded in the moss. Leaves, branches, and hazel nuts are found in abundance in this thin stratum, upon which rests the moss.

In an area of about a quarter of an acre laid bare by Mr. Ormerod's excavations were found at least 200 cubic feet of oak timber, which was carted away for fuel, forming six cart loads. Some of the trees were two feet in diameter, the roots *in situ* in the loam.

On one of the trees was found a fine specimen of a fungus attached to the stem at a foot or two above the root. It is a fungus now found growing on oak trees, "Polyporus igniarius," and is in beautiful preservation, the pores being quite visible to the naked eye.

It would appear as if the level valley plain on which Chat Moss now lies was formerly covered over with a forest of oaks with a dense undergrowth of birch, hazel, and other brushwood. How these trees were thrown down, and the forest became covered up by moss, is subject for speculation. It was probably a gradual decay, owing to the lowlying, undrained soil, which favoured the growth of sphagnum and of such fungi as we have seen attached to the oaks now found lying in the moss.

The occurrence of this thin stratum of loam containing the roots of trees *in situ,* underlying the moss, is strikingly like the underclays containing roots of the stigmaria in the coal measures.

a. Peat Moss. The general thickness of the moss, as exposed in the excavations and drains made by Mr. Ormerod, is from 17 to 25 feet. In some parts of Chat Moss the thickness is much greater, owing to the irregularities in the surface of the boulder clay or other stratum forming the base of the moss. In one place a depth of at least 180 feet has been reached with boring rods, which would place it below sea level, and would point to the existence of water—possibly an inland arm of the sea—since filled up by moss.

When the water is drained off, the moss is very considerably reduced in thickness. We measured it at two places where drains had been cut through to the loam, and where the moss had originally been 17 feet thick. It is now reduced to 8 feet, having sunk above one half. Mr. Ormerod believes he has thus lowered an area of 300 acres more than 3 feet on an average. It is quite evident from this circumstance that the bulk of the moss is very greatly influenced by the water contained in it, and it is probably from this cause that mosses are usually highest at the centres. The water can get away from the margins, but it remains in the centres and swells up the bulk.

The existence of the boulder clay under Chat Moss affords a ready means for the reclamation of the surface for agricultural purposes. Mr. Ormerod proceeds in the first place by carrying forward deep drains down to the loam, having connection with the lowest outfall. These are connected by cross drains, which gradually draw off the water and leave a firm surface. The moss is then turned over by spade labour, after which it is covered with clay brought up from below at the extensive excavation which forms the subject of this paper. The land is next manured with town manure, and, being sown with clover and oats, is found to bear an excellent crop of oats the first year, and clover and grass crops the two years following. In this way it is expected the whole of Chat Moss will shortly be reclaimed.

PHOTOGRAPHICAL SECTION.

January 11th, 1866.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

A note from Mr. Joseph Sidebotham was read, regretting his inability to attend the meeting, and giving particulars respecting some photographs which he had recently taken with Dallmeyer's new wide-angle lenses of five and seven inches focus. These prints were exhibited, and also one taken with an ordinary lens from the same spot, a comparison of the two showing the great advantage of the new over the old form of lens. The time of exposure was stated to be from two minutes to thirty seconds for collodio-albumen plates at this season of the year.

Professor Roscoe explained the method of meteorological registration of the chemical action of light, as described in his paper in the *Philosophical Transactions* of the Royal Society, which has been chosen as the Bakerian lecture for 1865. Dr. Roscoe exhibited the apparatus needed, and showed the method of manipulation adopted in order to obtain curves of daily chemical intensity. He also detailed the results which have been obtained by the employment of his method at the British Association's observatory at Kew, during nine months of the year 1865, under the superintendence of the Director, Mr. Balfour Stewart, F.R.S.

Dr. Joule observed that he considered Dr. Roscoe's investigations on this subject to be equal in importance to the

systematic registration of the variations of temperature, as the growth of corn, &c., depended as much on the chemical effect of light as on the temperature of the air.

Mr. Baxendell regarded the subject as a new department in meteorology, and one likely to yield results of considerable interest and importance.

Mr. Parry suggested that it was desirable to have observations made at more than one station, and at a distance from Kew.

Dr. Roscoe stated that he hoped in a short time to perfect apparatus for effecting the registration of the chemical changes in the light by means of self-acting apparatus, so as to lessen the labour of making the observations.

It was suggested that a simple photometer would be useful to photographers working with dry plates, and Dr. Roscoe said he would endeavour to carry out the idea.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

January 17th, 1866.

A. G. Latham, Esq., President of the Section, in the Chair.

The following donations were announced:—

Roper's Catalogue of Microscopic Works, by the Author.
Kölliker's Manual of Human Microscopic Anatomy, by the Executors of the late George Mosley, Esq.
Beck on the Microscope, by the Secretary.
Six slides of seeds and fungi, by the Secretary.
Several slides of sections of a Cidaris from the Indian Ocean, by Mr. Parry.

The Secretary reported that he had made a catalogue of the collection of microscopical objects belonging to the Section.

Mr. Sidebotham remarked on the best cement to use in forming cells for fluid preparations, and stated that gold size appeared to prevent the entrance of air bubbles better than Japan varnish or Brunswick black, which latter in time became porous, and also, from the evaporation of its turpentine, brittle. He said he and Mr. Thwaite were perhaps the first to use this method of mounting objects, and that he possessed slides of gold size cells made in 1844, which were still quite perfect, while those he mounted with Japan black in 1850 were most of them spoiled, and that he had again reverted to the use of gold size for the formation of the cell, using Japan varnish for its final closing only.

The SECRETARY said gold size remained viscid for a long time, and that if the cells formed of it were not well dried for a considerable period, or even baked in an oven, the size was very liable to "run in" and spoil the preparation. He had re-varnished the Section's collection with a mixture of Japan varnish and gold size, and thought the gold size would prevent the Japan varnish from becoming brittle or porous, while the latter would prevent the gold size from running in; but he strongly recommended that all collections should be re-varnished every five years, and deprecated the use of covering papers on slides of fluid preparations, as it prevented this.

Mr. LATHAM recommended the addition of a solution of india-rubber, and Mr. PARRY of wax, to Japan varnish, to obviate its tendency to become porous and brittle.

Mr. HEYS showed a well mounted specimen of the exuvium of the larva of a Dragon Fly, and stated he found these insects were easily brought to cast off their skins by changing the water in which they were kept—if soft, to hard, and *vice versa*, or if muddy, to fresh.

Mr. PARRY exhibited mounted specimens of an Ammonite.

Dr. ALCOCK said that among Foraminifera from Dogs Bay which he had lately mounted, he thought there were some slides likely to interest the members. Many of the deformed specimens of Lagena striata (Williamson) were very curious, and a double one, having the neck as well as the body double, deserved particular notice. He said that he was quite convinced the striated Lagena with a mucro at the base is not a mere sub-variety of Lagena striata, but is very distinct from it; there were many specimens of it, all agreeing in their peculiar characters, and he proposed for it the varietal name of L. mucronata. The Lagena with a collar at the base of the

neck, described by him in a previous paper, was undoubtedly distinct from any of the named forms, and he proposed to call it Lagena antiqua. In his examinations of the Dogs Bay sand one specimen only of Lagena vulgaris typica (Williamson) had occurred, though L. clavata was comparatively common. Perhaps the most interesting find was a perfect and characteristic specimen of Lagena crenata, a form lately described and figured by Parker and Jónes, from Australia, but he believed not hitherto observed as British in the recent state. The very magnificent specimens of Entosolenia Melo also deserved notice, and the curious specimens of Truncatulina lobata with the later chambers "run wide," and various monstrous forms of Miliolina, would be examined with interest.

Ordinary Meeting, February 6th, 1866.

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

The Chairman adverted to the loss the Society had experienced by the unexpected death of Mr. Parry, who from the period of his election in 1833 had constantly promoted the success of the meetings by his intelligence and kindly intercourse with the members.

Mr. Baxendell said that it might interest some of the members of the Society, especially those who had commercial relations with India, to hear that the system of forecasting the weather, and particularly the occurrence of cyclone storms, had been introduced into that country by Mr. Pogson, the government astronomer at the Royal Observatory, Madras. The first trial was made on the 25th November last, when the indications of a distant but approaching cyclone induced Mr. Pogson to send a notice to the Governor, Sir William Dennison, who promptly despatched a number of mounted sepoys to convey the information to various officials and direct them to prepare for the coming storm. Next day, uprooted trees on shore and disabled ships in the roadstead showed that the indications had been rightly interpreted. On the 4th December the weather had again a very

threatening appearance, and the officials of the Marine department ordered all the vessels in the roadstead to put to sea; but Mr. Pogson, guided by the principles which had served him so well on the 25th November, concluded that a cyclone was passing far to the south, but would not approach sufficiently near to cause a severe gale at Madras, and he therefore ventured to send the message, "No need, the storm will not come here," and the result fully justified the confidence he had placed in his conclusions, as the storm passed without the wind at Madras attaining a dangerous force, and unusual precautions proved therefore to be unnecessary.

A paper was read entitled "Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes." Part III. By J. C. Dyer, Esq., V.P.

On Nail Making by Machinery.

About the beginning of this century the great consumption of nails in America, in wood buildings, and the high rate of skilled labour, had led to many attempts to make nails by a more summary process than that of the hammer and anvil, used for making those imported from England. Those called "cut nails," were made by cutting angular slips from iron plates by the common shears, and the heads were made by a separate process of the die and hammer, worked by hand; and to unite these two operations in one machine was the object sought. For this purpose a machine was invented and patented by Mr. J. Odiorne, and another machine by Mr. Jacob Perkins, about the year 1806. These two patents, though for the same object, differed so far in construction

that they were held to be distinct inventions. A third machine was invented by a Mr. Reed, which, however, was a combination of the former two, and I believe was not held valid as a patent. In 1810, Messrs. Wells and Co., of the Charles River Iron Works, near Boston, having arranged with the patentees, sent out models of the nail machine to me in London, to be patented for our joint account. The operations of the machines were as follow:—

1st. Feeding-plates, of the width and thickness to form the nails, are pushed endwise over a fixed cutter, against a stop under the traversing cutter, and at such angle with the line of the cutters as to give the severed nail the head and point ends, and the plates turned over between each successive cut.

2nd. A fixed gripping die is placed just under the fixed cutter, the face of it and the cutter being in the same line, and the counter die moves forward to bring both together, so as to hold the nails firmly, a portion of the large end standing out beyond the dies to form the heads of the nails.

3rd. The heading die then advances and presses the end into the "rose," "clasp," or "clout" heads.

Success or failure often depends upon slight changes in power-driven machines. These nail machines answered very well for large nails, but failed as applied to very small ones, but as a means of saving the labour applied to making tacks and very small nails, it became important to adapt the machines to them also. This object, after some time, I succeeded in accomplishing, and thereby rendered the patent nail manufactory, established under the patents for the original invention and the improvements on it, a success.

For a full description of these inventions I must refer to the specifications of the patents, and to the paper in *extenso,* as they could not be made intelligible within the limits of this abstract. The average rate of working was about 100 per minute for nails and 120 per minute for tacks. In after practise the tack machines averaged about 80,000 a day, whilst the best hand-workers made about 1,200 to 1,400 a day. Each machine was tended by a youth, much like those employed in hand-making, so that one hand with the machine turned out as many tacks per day as would require about sixty hands working with hammer and anvil. I have been informed that the speed of these machines has been greatly increased of late years. Some of the movements in these nail machines have since been adopted and found very efficient in rivetting machines and others of recent invention.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

January 29th, 1866.

J. SIDEBOTHAM, Esq., in the Chair.

The following specimens were exhibited:—

Two figures of the gills of fungi on glass, made by the dropping of their spores, by Sir J. Herschel, exhibited by Mr. Sidebotham; also similar ones prepared by Mr. Sidebotham.

Small cyclophorus-like shells, with a considerable part of the later growth free and strangely contorted, from Borneo, Mr. Sidebotham.

Leaves of some Indian plants, mounted for the microscope, Mr. Hurst.

Mounted Foraminifera, from shore sand, Port Adelaide, South Australia, Dr. Alcock.

Mr. LINTON said that the abundant appearance last year of the humming bird hawk moth had afforded many collectors and admirers of nature excellent opportunities of watching this interesting and amusing insect in pursuit of its food, and of observing its remarkable agility, darting from flower to flower, and then poising itself, apparently stationary, while its long proboscis is put out with almost instantaneous velocity. It is very shy, and when disturbed darts away to a great height in the air, but it returns after a time and often to the same spot. He had observed one in his garden, at Old Trafford, about a clump of sweetwilliams,

and, after two unsuccessful attempts in one day, he succeeded on the following day in taking it. During the time when the insect was abundant, many letters referring to it appeared in the *Times* newspaper, and in these it was constantly stated that the insect visits only scarlet or bright-coloured flowers, such as the scarlet geranium and verbena; but if closely watched it will be found not to confine itself to any particular flower, and in the case just mentioned it chose the sweetwilliams, though these were at the time quite on the decline, and there was abundance both of geraniums and verbenas in the garden. While staying at Pensarn he observed one of these moths on two successive days resting during the hottest part of the day on a newly whitewashed wall. It might be supposed that the most likely place to find the moth would be hovering over the plants on which the larvæ feed, which are Galium verum and G. molluga; but, while in Wales in August, he visited a spot daily, for a fortnight, where these plants were growing in great luxuriance, and never once saw the moth at that place, though he found the larva and succeeded in rearing it. The pupa cannot be said to bury itself in the earth, for it is found with only a few leaves drawn together over it.

Mr. Linton also announced having bred the butterfly, Grapta C. album, from the larva taken by himself at Abergele.

PHYSICAL AND MATHEMATICAL SECTION.

February 1st, 1866.

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

The following "Results of Rain-gauge and Anemometer Observations made at Eccles, near Manchester, during the year 1865," were communicated by Mr. THOMAS MACKERETH, F.R.A.S., M.B.M.S.:—

During the past year I have been able to register the rainfall at Eccles, from four different gauges; two I have placed three feet above the ground, thirty-four feet from any dwelling-house, and fourteen feet from my astronomical observatory, which stands ten feet higher than the receivers of these gauges. This ten feet measures to the apex of the revolving roof of the equatorial room. The receivers of the rain gauges on the ground are of different shapes, one being round, 10in. diameter, the other 5in. square. The other two gauges are placed four feet above the ridge of my house, quite above the chimneys and free from the influence of any erections whatever. One of the gauges is 5in. square, like the 5in. one that is three feet above the ground, on Glaisher's plan, with an edge inclined inwards, as is usual with rain gauges. The other is also 5in. square, but with an edge inclined outwards. These gauges are thirty-two feet from the ground. The water falls into glass vessels, into which the receivers are made to fit, exactly like the 5in. gauge three feet from the ground, so that there is no tube required for it to flow through and thus to cause loss by evaporation. Below I

present the monthly amounts of rain that fell into each gauge, and the number of miles of horizontal movement of the air.

1865.	Rainfall in 5in. square gauge with edge inclined outwards, 32ft. from ground.	Rainfall in 5in. square gauge with edge inclined inwards, 32ft. from ground.	Rainfall in 5in. square gauge with edge inclined inwards, 3ft. from ground.	Rainfall in 10in. round gauge with edge inclined inwards, 3ft. from ground.	Amount of horizontal movement of air in miles.
January	2·238	2·486	2·956	2·952	5,250
February	1·872	2·137	2·582	2·489	4,309
March	1·320	1·730	1·700	1·721	4,480
April	0·993	1·025	1·083	1·102	4,724
May	2·585	2·605	2·840	2·884	3,499
June	0·830	0·840	0·943	0·943	3,215
July	1·883	1·992	2·043	2·085	3,784
August	4·808	4·832	4·967	4·977	3,615
September	0·481	0·548	0·563	0·571	3,089
October	4·353	4·621	4·491	4·550	4,286
November	2·596	2·830	2·744	2·818	4,237
December	0·569	0·589	0·673	0·717	4,263
Totals	24·528	26·235	27·585	27·809	48·751

The next table shows the fall of rain in each gauge under the same general direction of the wind, and the corresponding daily amount of horizontal movement of the air.

General direction of the wind	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
Average daily horizontal movement of the air in miles	79	124	143	156	178	162	183	149
Fall of rain in inches in 5in. gauge with edge inclined outwards 32ft. from ground	1·414	2·011	0·259	4·060	5·246	6·738	2·099	2·701
In gauge with edge inclined inwards 32ft. from ground	1·468	2·054	0·271	4·285	5·866	7·168	2·280	2·840
In 5in. gauge with edge inclined inwards 3ft. from ground	1·472	2·425	0·317	4·550	6·000	7·258	2·515	2·949
In ordinary 10in. gauge 3ft. feet from ground	1·501	2·415	0·327	4·610	6·069	7·363	2·516	3·006
Number of days	6	27	6	35	22	43	15	23

From the above table it appears that the least amount of rain falls with an east wind, and the greatest with a south-

westerly wind. The strongest wind with rain is the west wind, and it is known that the strongest gales are from winds a little north of west. This shows, though the above table is framed from the observations of one year only, that the strongest winds are from nearly the same quarter, even when rain falls with them.

The following table shows the average daily rainfall in each kind of gauge when the velocity of the wind has ranged between the number of miles indicated in the first column.

Daily Movement of Wind.	32 feet from ground.		3 feet from ground.	
	5in. square gauge with edge inclined outwards.	5in. square gauge with edge inclined inwards.	5in. square gauge with edge inclined inwards.	10in. ordinary round gauge.
0 to 50 miles	·029	·030	·042	·045
50 to 100 „	·103	·105	·110	·114
100 to 150 „	·220	·232	·231	·233
150 to 200 „	·108	·118	·122	·124
200 to 250 „	·151	·160	·174	·175
250 to 300 „	·127	·138	·154	·158
Above 300 „	·174	·204	·197	·211

The President remarked with reference to the difference between the two elevated gauges that a Glaisher's gauge, with the rim inclined inwards, would register more than the true amount, as the spray from drops striking the rim on the windward side would be carried over into the gauge; while, on the other hand, a gauge with a rim inclining outwards would give less than the true amount, because the spray from drops falling on the rim on the lee side would be blown away and lost.

Mr. Baxendell, F.R.A.S., read the following "Note on the Variable Star S Coronæ."

This variable was discovered on the 5th of August, 1860, by Dr. Hencke, of Driesen, and a notice of it inserted in the *Astronomische Nachrichten*, No. 1281. So far, however, as I am aware, no account of any observations made since, has

yet been published, and I have therefore thought it might be desirable to communicate to the Section the results of two series of observations which I have made at Mr. Worthington's observatory, and which have enabled me to determine its approximate mean period and epoch of maximum. The first series commenced on the 26th of May, and ended on the 31st of October, 1864. A projection of the observations shows that a maximum occurred on the 12th of August, 1864, the magnitude being 6·7. The second series began on the 2nd of April and was continued till the 12th of November, 1865, the star during this time rising from the 11·9 magnitude on April 2nd to a maximum, 6·6 magnitude, on July 17th, and afterwards declining to the 8·6 magnitude on November 12th. An inspection of the light-curves which accompany this note will show that only one period could have elapsed between the dates of these two maxima; and as Dr. Hencke's observations in 1860 indicate that a maximum occurred about the 1st of September in that year, we have the following data for the determination of the star's elements:—

OBSERVED MAXIMA.		INTERVAL IN DAYS.	NUMBER OF PERIODS.
1860.	September 1.	1441	 4
1864.	August 12.	339	 1
1865.	July 17.		....

Equating, and treating by the method of least squares, we have

Mean Period............... = 357·2 days.

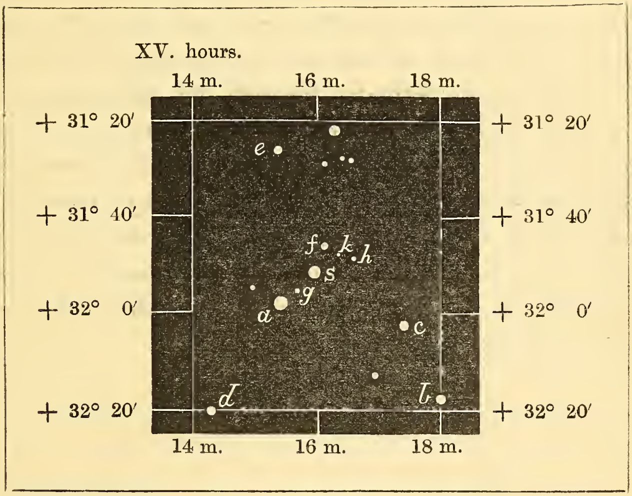
Epoch of Maximum...... = 1863, August 10·6.

The times of maxima calculated from these elements, compared with the observed times, give the following differences:—

C – O
days
+ 2·0
– 10·2
+ 8·0

I have not observed S Coronæ when at its minimum brightness, and cannot therefore give the lower limit of its range of variation, nor the interval from minimum to maximum. My observations show, however, that it belongs to the list of variables which increase in brightness more rapidly than they diminish. Thus in 1864 it rose through 4·5 magnitudes in 78 days, but fell through only 1·5 magnitudes in 80 days; and in 1865 it increased 5·3 magnitudes in 106 days, but diminished only 2·0 magnitudes in 118 days.

The place of S Coronæ for 1865·0 is 15 h. 15 m. 54·6 s. +31° 51·2′. The calculated time of its next maximum is 1866, July 17; and for the convenience of observers who may be disposed to watch its changes and record their observations, I append to this communication a small chart, and a list of comparison stars with their magnitudes.



MAGNITUDES OF COMPARISON STARS.

a = 6·8	*d* = 8·6	*g* = 11·3
b = 7·9	*e* = 8·8	*h* = 11·9
c = 8·2	*f* = 10·2	*k* = 12·6

Mr. BAXENDELL also read the following communication "On the Determination of the Mean Form of the Light-curve of a Variable Star."

Since the publication in the last number of the Society's *Proceedings* of my "Note on T Aquilæ," I have been requested to give an explanation of the method I employ to obtain the mean light-curve of a variable star. Many years ago I adopted a method which I afterwards found was very similar to, if not identical with, the one used by Professor Argelander for the same purpose; but I have recently employed another plan which appears to be decidedly preferable, as it saves time, renders unnecessary a great deal of tedious computation, and gives results of greater weight and value than those obtained by the former method. It is simply this: Having determined the elements of a variable star, calculate the times of all the maxima—or minima, as the case may be—that have occurred during the period over which the observations extend; then, taking the original light-curve, read off the values of the ordinates for each day, and arrange them in a table having as many columns as there are days in the star's mean period, or, in many cases, it will be sufficient to take every second, or even third day, always reckoning, of course, from the last preceding maximum or minimum, and then adding together the values in each column, and taking the means, we have the numbers from which to lay down the star's mean light-curve.

Ordinary Meeting, February 20th, 1866.

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

Mr. E. W. BINNEY, F.R.S., said that in the calcareous nodules found in the Upper Foot coal, a mine lying about fifteen yards above the Gannister coal at Moorside and other places near Oldham, he had met with a small stem of fossil wood showing structure in a very perfect state. It evidently belonged to the genus *Pinites* of Witham, since changed by Endlicher and Brongniart into *Dadoxylon;* but after comparing it with the species figured and described by those authors, and more lately by Professor Schimper of Strasbourg, he was of opinion that it was a new one. The specimen was also more complete than any other with which he was acquainted, although it was but of diminutive size.

It has generally been supposed that the coniferous woods found in the coal measures were only to be met with in sandstone rocks, and not in seams of coal or beds of shale, and had been drifted from high and dry lands into the waters in which such deposits had been formed, and had not grown on the places where they were discovered like *Sigillaria* and its root *Stigmaria.* The specimen of *Dadoxylon* now described, however, had equal claim to be supposed to have grown on the spot where it was found as any *Sigillaria* met with in the same seam of coal.

The stem is nearly cylindrical and enveloped in a matrix of limestone, so that we cannot see its external characters. Its diameter is about one half of an inch.

On examination of a transverse section of the stem we find a medullary axis composed of irregular polygonal cells full of dark carbonaceous matter separated by intervening spaces

vertically, and thus forming a kind of discoid pith somewhat similar to that noticed by the late Professor Corda, and afterwards by Professor W. C. Williamson, F.R.S., in a paper published in vol. ix., second series, of the Society's memoirs. This pith in the present specimen appears to be separated from certain lunette-shaped bundles of hexagonal tubes of large size, arranged in a convex form towards the pith inwards, and lessening in size as they pass outwards into wedge-shaped masses of four-sided subhexagonal cellules arranged in radiating series and divided by large medullary rays or bundles which appear to originate in the lunette-shaped masses. On the outside of this internal radiating cylinder are other lunette-shaped bundles similar to those in the inside previously described and also with a convex outside. Then comes a narrow zone of lax tissue which has been a good deal disarranged. Outside this are some thin wedge-shaped bundles of cellules full of dark carbonaceous matter, and arranged in radiating series of varying sizes separated by lax tissue probably representing the bark of the tree.

In the longitudinal sections the cellules are seen to be greatly elongated and divided with oblique and transverse dissepiments placed at great distances. Two of the walls, namely those facing the medullary rays, are regularly reticulated with six, seven, and eight series of hexagonal areolæ arranged contiguously but not in a line.

In the tangential section the walls of the cellules also show a reticulated appearance something like that previously noticed, but not in so marked and distinct a manner, and the medullary rays or bundles in their section show numerous irregular series of small cellules of one to four, and more rarely much larger cellules.

The arrangement of the lunette-shaped bundles next the pith reminds us of similar masses described by Brongniart as occurring in his *Sigillaria elegans*, but the cellules in my specimen are much more irregular in size than those of

Sigillaria, and there are no traces of striæ upon their sides. The same may be said of somewhat similar lunette-shaped masses appearing on the outside of the internal radiating cylinder.

The areolæ on the walls of the cellules are more numerous than in any species of *Pinites* or *Dadoxylon* which have hitherto come under my notice, the *P. medullaris* of Witham having the walls of its elongated cellules reticulated with two, three, and four series of contiguous areolæ, and those only on the walls parallel to the medullary rays, while in my specimen they are reticulated with six, seven, and eight, and not only on such walls, but also on the walls at right angles to the medullary rays. The lunette-shaped bundles of cellules both in the inside and outside of the internal radiating cylinder are different from those seen by me in any other specimens of *Dadoxylon.* For the purpose of distinguishing it the name *D. Oldhamium* has been given to it.

A paper was read "On Air from off the Atlantic, and from some London Law Courts," by R. Angus Smith, Ph.D., F.R.S., &c., President.

The specimens of air collected by Mr. Fryer when on his way to West Indies, and those collected in Antigua, are worth remarking, as the first agrees with the figures obtained previously when examining air on the sea shore and open heaths of Scotland, where the highest average was obtained, and the second agrees with the numbers obtained in more inhabited but not closely inhabited places.

Those from a law court are interesting; they are the most deficient in oxygen of any specimens found by me during the day in inhabited places above ground. The first is almost exactly the same as the average found in the currents of galleries in metaliferous mines; that from the lantern is nearly the same as the specimens found close to the shafts of the same mines, meaning of course the average of many

specimens. I have not known any mills or workshops so deficient in air. I consider a room bad when it loses 1,000, and workshops very bad when they lose 2,000 of oxygen out of a million parts; here the loss is actually 5,000 less than the parks of London. The circumstance is strange, and I hope unusual. A scientific friend happened to call my attention to it and wished me to examine the air. The moisture from the window was collected and there were several ounces obtained, and more might have been easily found. It was perspiration in great part, the smell of it was distinct. It is putrefying, and decolorises more permanganate now than it did at first. Mere change of air will not purify a room like this—a current must pass through it for a long time until complete oxidation takes place.

Oxygen per cent in some Specimens of Air.

18ft. above water. Fine day. 2 30 p.m. Lat. 43·05, W. 17·12.	St. John's, Antigua. April 11th, 1865. 9 a.m. Showery morning.
21·0100	20·9600
21·0000	20·9100
20·9700	21·0000
Mean 20·9900*	Mean 20·9500

Law Court, Feb. 2nd, 1866.	Law Court, from the lantern, 4 30 p.m. just as the court was closing.
20·6400	20·5000
20·6700	20·4800
Mean 20·6500	Mean 20·4900

* May be read 209,900 in a million, and so with the others.

Ordinary Meeting, March 6th, 1866.

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

Mr. E. Sonstadt communicated the following "Note on the Purification of Platinum:"—

The tendency of platinum to alloy with other metals, at a temperature far below its fusing point, is sufficiently well known to every user of platinum crucibles. It is equally well known that iron, &c., which has been absorbed by platinum cannot be removed, except superficially, by the action of hydrochloric acid for instance, nor even by heating in acid sulphate of potassium. Stas, in his memoir on the atomic weight of silver, &c., states that he purified his platinum vessels from iron, by causing them to come in contact, at a red heat, with the vapour of chloride of ammonium. The process had to be repeated as often as any yellow sublimate was formed. This process is less effectual, or less conveniently and speedily effectual, than the modification of it that I have to propose; because, if the vapour of the sal ammoniac is generated from the solid salt in the vessel to be purified, the heat absorbed in the vaporization of the salt tends to keep the vessel at a temperature below that at which volatile metal chlorides are most readily formed. Instead of chloride of ammonium, I put dry double chloride of ammonium and magnesium in the platinum vessel intended for purification. The vessel is then heated to about the fusing point of cast iron for about an hour. I find a Gore's furnace convenient for this purpose. In this process, not

only is chloride of ammonium vapour given off for a long while with the double salt, at a temperature much above that at which chloride of ammonium alone volatilizes, but when that salt is completely expelled, the chloride of magnesium remaining is perpetually being decomposed with evolution of free chlorine, and, frequently, the formation of a crystalline crust of periclase lining the crucible. Platinum thus purified is softer and whiter than ordinary commercial platinum. The method is not available solely for the removal of iron, but retrieves crucibles that have become dark coloured and brittle from exposure to gas flame, as well as crucibles that have been attacked by silicates during fusion of these with carbonate of sodium. I cannot conclude this note without remarking on the extreme facility with which platinum becomes impure by heating in contact with matters containing only a very small proportion of substance capable of attacking the metal. Thus, a platinum crucible becomes sensibly impure after prolonged ignition at a high temperature, bedded in commercial magnesia. On the other hand, I have kept a platinum crucible at a constant weight, to the tenth of a milligramme, over a series of intense ignitions, when the precaution has been taken to bed it in chemically pure magnesia.

A conversation took place on the cattle plague, in the course of which the President and Mr. Spence stated that the use of carbolic acid as a disinfectant had been quite successful as a preventive in the limited number of cases tried, none of the cattle on the farms where it had been regularly and properly used having yet been attacked by the rinderpest. People rarely used enough.

An opinion was strongly expressed by some members that the means which had been adopted to arrest the progress of the disease had, in fact, served to propagate it and extend its ravages, as numerous instances could be cited in which the

infection had been carried by the official inspectors from unhealthy to healthy districts, and it was suggested that the inspectors ought to be compelled to cleanse themselves and undergo a disinfecting process before they were allowed to visit and inspect cattle not known for certain to be diseased. It was characterised as a most unwise proceeding to send forth an army of inspectors to invade every farmstead in the kingdom, and not to adopt and enforce stringent regulations to prevent the possibility of these men being the means of carrying the dreaded infection into healthy localities. It was remarked that official interference had virtually taken the management of the cattle plague out of the hands of those who, from their practical experience and the deep interest they had in the matter, might be held best qualified to deal with it, and there were grounds for thinking that if the farmers had been left to act for themselves without official interference, as they did some years ago when pleuro-pneumonia carried off large numbers of cattle, the rinderpest might never have assumed its present formidable aspect.

A paper was read "On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties," by Messrs. EDWARD HULL, F.G.S., and WILLIAM BROCKBANK.

In this paper the authors gave the results of their observations on the nature, geological position, and qualities of the iron ores which are now being worked at intervals from the banks of the Tees to that of the Evenlode in Oxfordshire, extending through the counties of York, Lincoln, Rutland, Leicester, Northampton, Warwick, Oxford, &c.; at the same time embodying the opinions of previous observers.

Remarking that just at the time when some of the older iron-producing districts were giving evidence of approaching exhaustion, the enormous stores of iron ore in the newer formations were discovered; the authors commenced by a description of the geological position of the strata from which the

ores are extracted, referring them to the middle lias or marlstone and the base of the great oolite; and it was shown that the ores of Cleveland in Yorkshire, Lincolnshire, and Oxfordshire, are derived principally from the lias, while those of Northamptonshire are extracted from the basement beds of the great oolite, called by the government geological surveyors "the Northampton sands."

The first district described was that of the East Riding of Yorkshire and the Cleveland hills, which, within the space of about sixteen years, has given birth to the iron trade of the Tees-side, of which Middlesborough may be considered the centre. The ore is here quarried in open and tunnel-works, and brought down by rail from the hills to the furnaces, which are erected along both sides of the river, and which are supplied with fuel from the Durham coal-field. The production of this district in 1865 was stated to be nearly one million of tons of pig-iron annually, drawn from 105 furnaces in blast. The ores in the valley of the Esk, near Whitby, and those of Guisborough were then described, and particularly the Rosedale ore, which is the richest in the district, and is magnetic. A branch railway is opened to the quarries, which in 1864 yielded nearly 300,000 tons of ore, having a per centage of 35·94 to 49·17 of metallic iron. The iron-stone was then traced to the banks of the Humber, near Hull, by Stokesley, Swainby, Northallerton, Easingwold, and Market Weighton.

The ores of North Lincolnshire were stated to be spread over a wide expanse of country—an area of not less than 100 square miles—having been proved to be quite close to the surface, and their local and geographical position in reference to the sea-ports on the one hand, and the South Yorkshire coal-field, together with the excellent quality of the iron they produced, satisfied the authors that this district was destined to rise in importance as a centre of iron manufacture. The pig-iron from North Lincolnshire is highly tenacious and

fusible, and commands a higher price in the market than the Cleveland brands. This superiority appears to arise from the spathic nature of the iron-stone, and from the presence of manganese in considerable quantities inter-stratified with the ores, and which is mixed in the furnaces. The ore, with its accompanying strata, is very finely shown along the line of railway at Frodingham; its thickness in different parts of the district varies from twelve to thirty feet. It is being smelted at three works in North Lincolnshire, and it is also carried largely by rail to the Park Gate Iron Company's furnaces near Rotherham, and other works in the Yorkshire and Derbyshire coal-fields.

From North Lincolnshire the ironstone may be traced southwards along an indented line running parallel to the western margin of the great oolite into Northamptonshire. At the base of the oolite a series of yellowish sands occur—and, in these there are at intervals, beds of iron ore—sometimes extending for miles with considerable regularity, at other times thinning away rapidly. From these beds, the Northamptonshire iron is smelted. The furnaces and quarries are situated at Gayton and Blisworth, Blakesley, Maidford, and Litchborough, at which place they are more than usually ferruginous. At Duston, where it is extensively worked and smelted, it is more consolidated and is coarsely oolitic. The metal produced is similar to that of the Cleveland district in quality. Large quantities of this ironstone are sent by rail and canal into South Staffordshire, and even South Wales, where it is valued for mixing with the argillaceous carbonates of the coal measures.

The Oxfordshire district is as yet almost unopened, but promises one day to be productive of iron on a large scale, similar in character to the Lincolnshire brand. In the neighbourhood of Banbury and Chipping Norton, the ore occupies considerable areas—sometimes in the form of tabulated hills, intersected by valleys of no great depth. It occurs here (as

already stated) in the middle lias, precisely the same formation as that to which the ores of Cleveland and Lincolnshire belong. The ironstone, however, is generally more calcareous than in the former district, and its richest portions are limited to a tract, of which the village of Bloxham may be considered the centre. As yet the ore has only been worked at Fawler, near Charlbury, and at Steeple Aston, to a very limited extent; but the railways, now in course of construction, will probably have the effect of bringing it within easier reach of the coalfields of South Wales and Staffordshire.

The authors remark, in conclusion, that every day's experience enlarges our acquaintance with the great mineral resources of our country, and that, in the case of coal and iron, it becomes a question which is more largely distributed and likely to outlast the other.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

February 26th, 1866.

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

The following objects were exhibited :—

Mounted specimens of twenty-four species of Ostracoda, from Dog's bay shore-sand, collected by Dr. Alcock, and named by Dr. G. S. Brady.

Mounted specimens of many forms of Foraminifera, from a deposit discovered while sinking a well at Boston, Lincolnshire.—Mr. Sidebotham.

The skull and skin of a male Otter shot in Rostherne Mere, Feb. 16.—Mr. Harrison.

Dr. Alcock read a paper on Foraminifera from mud washed out from a shell of Halia Priamus in Mr. Darbishire's collection. He said that part of the interest of these specimens depended on the information they may give as to the nature of the sea-bed where this rare mollusk is found, the exact locality from which it is obtained by the Cadiz fishermen being still doubtful. He exhibited mounted specimens of about forty forms of Foraminifera, twenty-five of them agreeing with British ones as described by Professor Williamson, the others, so far as he knew, not British, and at present unknown to him. He said that the great abundance of Globigerina with fragments of large Orbulinæ, appeared to indicate a deep-sea deposit, but the most remarkable feature was the extraordinary profusion of Textularia variabilis, a very considerable proportion of the whole mass consisting of these shells. Bulimina pupoides was perhaps next in abundance, and next to it, in about equal proportions, Cassidulina lævigata, C. obtusa, and Nonionina elegans. Rotalina Beccarii was plentiful but small, as were all the forms of Rotalina which occurred Lagenæ were scarce and were very small hyaline varieties. Nodosaria radicula was rather common, and three very distinct varieties of it were met with, one of them remarkable for its large size and the raised rings upon its neck. Polymorphina was entirely absent; and Polystomella, Spiroloculina, and Miliolina were represented by only a very few extremely small individuals.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 1st, 1866.

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.
E. W. BINNEY, F.R.S., F.G.S.

Vice-Presidents.
ROBERT WORTHINGTON, F.R.A.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.
Mr. THOMAS CARRICK.

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

A paper was read "On the Variable Star R Vulpeculæ. $\alpha = 20^h\ 58^m\ 22{\cdot}9^s$. $\delta = +23°\ 17{\cdot}2'$. Ep. 1865·0." By George Knott, F.R.A.S., communicated by Joseph Baxendell, F.R.A.S.

This star, which is No. 457, hour xx, in the Palermo catalogue, was first recognised as variable, so far as I am aware, at the observatory of Bonn. It appears to have been observed with some care by Dr. Winnecke at the Pulkowa observatory, and in a letter to the Rev. R. Main, printed in vol. xxii. of the Monthly Notices of the Royal Astronomical Society, p. 285, that able astronomer assigns the following

elements, "which represent seven maxima observed in the course of three years, with reference to Piazzi's estimations of magnitude in August, 1803," viz. —

Period = 138·6 days.
Epoch = 1860, Nov. 6.

Having observed this star with more or less regularity during the past four years, it occurred to me that it would not be uninteresting to compare the elements resulting from a discussion of my own observations with those which had been deduced by Dr. Winnecke. The results of this discussion I have now the honour of presenting to the Manchester Literary and Philosophical Society.

Projecting my observations in the usual way, I obtain the following dates of maxima and minima, with the corresponding magnitudes —

Maxima.		*Minima.*	
1861. Dec.	30·0—8·4 mag.	1861. Oct.	26·3—13·6 mag.
1862. Oct.	5·0—7·8 „	1863. Sep.	18·0—13·2 „
*1863. Nov.	19·4—7·6 „	1864. June	19·5—13·2 „
1864. Aug.	16·3—7·5 „	Nov.	4·0—13·1 „
1865. Jan.	7·3—7·7 „	1865. Aug.	6·3—12·8 „
May	25·5—7·8 „	Dec.	14·3—13·7 „
Oct.	5·5—7·5 „		

Treating the seven observed maxima according to Mr. Baxendell's method, we obtain the following elements: —

Period = 137·59 days.
Epoch = 1864, April 4·95.

Comparing the observed times of maximum with those calculated from these elements, and also from those of Dr. Winnecke, we obtain the following differences between calculation and observation: —

* The projection of a series of his own observations of this maximum obligingly communicated to me by Mr. Baxendell, yields the following results, in gratifying accordance with my own :—Date of maximum, 1863, Nov. 18·9, mag. 7·5.

Knott's Elements.	Winnecke's Elements.
Calc.—Obs.	Calc.—Obs.
Days.	Days.
+ 1·41	− 3·2
− 2·41	− 5·0
− 0·01	+ 0·4
+ 4·24	+ 7·1
− 2·23	+ 1·3
− 2·78	+ 1·7
+ 1·81	+ 7·3

the sums of the squares of these numbers being 43·75 and 143·78 respectively. But while it thus appears that my own observations accord moderately well with Dr. Winnecke's elements, it must be confessed that these latter represent more satisfactorily than my own (as indeed might be expected) the magnitude estimates of Piazzi in the years 1807 and 1810, as given by Dr. Winnecke in No. 1224 of the *Astronomische Nachrichten.* At the same time it must be remembered that my own elements were deduced solely from my own observations, without reference to any of earlier date.

Treating the six observed minima in the same manner, we obtain the following elements, the period presenting a striking accordance with that deduced from the observed maxima:—

Period = 137·55 days.
Epoch = 1864, June 17·50.

The differences between the calculated and observed times of minima being:—

Calc.—Obs.
Days.
+ 2·35
− 1·60
− 2·00
− 1·95
− 2·15
+ 5·40

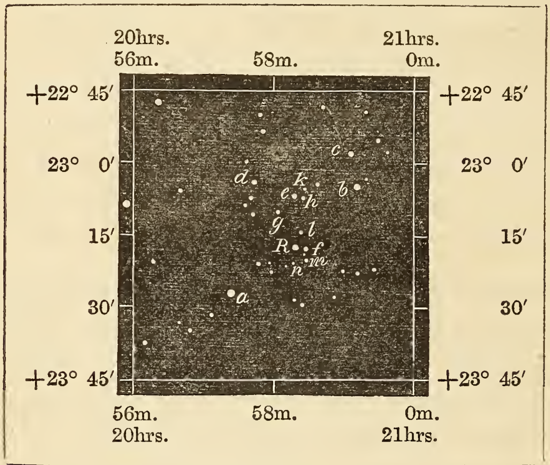
An examination of the mean light-curve (a copy of which accompanies this communication) which was laid down from the co-ordinates resulting from a discussion of all the observations I have obtained, yields the following results: —

Mean magnitude at maximum...............	7·77
Mean magnitude at minimum...............	13·14
Mean range of variation	5·37 magnitudes.
Mean magnitude	10·32
Interval from minimum to maximum......	66·0 days.
Interval from maximum to minimum......	71·6 days.
Interval from min. to mean mag............	25·8 days.
Interval from mean mag. to max.	40·2 days.
Interual from max. to mean mag.	37·3 days.
Interval from mean mag. to min............	34·3 days.
Interval from mean mag. before to mean mag. after maximum	77·5 days.
Interval from mean mag. before to mean mag. after minimum........................	59·1 days.

An examination of the various results of observation and calculation given in the former part of this paper suggests the following general remarks. Like many other variable stars R Vulpeculæ increases more rapidly than it decreases. The intervals between successive maxima and minima are subject to some little irregularity. And the observed magnitudes at maximum and minimum vary to the extent of some nine tenths of a magnitude. Still, as compared with some other stars, the movements of this variable must be regarded as tolerably regular. Although by no means so highly coloured as some variables, I have frequently noted the star in my observation book as "ruddy," or "decidedly ruddy." The maxima observable during the present year will fall, according to my own elements, on the following days: — July 17·0 and December 2·6. The observable minima will occur on May 6·3 and September 20·8.

The stars which I have used for comparison with R Vulpeculæ are shown in the small chart which accompanies this paper, and their magnitudes are as follow, the numbers in the cases of *a, b, c, d, g, l, m, n* being the means between my own values and those assigned by Mr. Baxendell:—

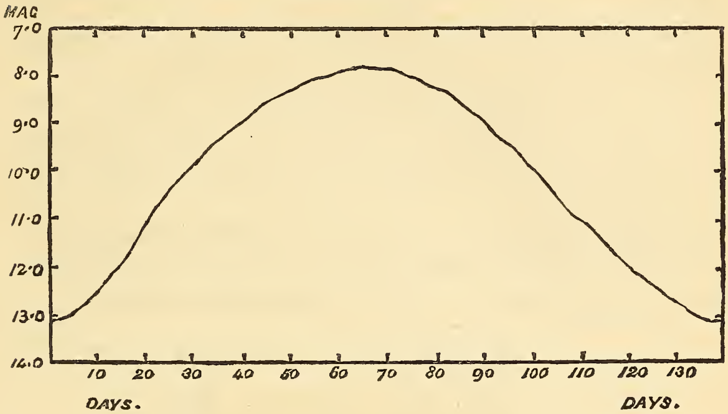
$a = 7{\cdot}1$	$g = 10{\cdot}0$
$b = 7{\cdot}3\pm$	$h = 10{\cdot}5$
$c = 8{\cdot}3$	$k = 10{\cdot}9$
$d = 9{\cdot}1$	$l = 11{\cdot}4$
$e = 9{\cdot}5$	$m = 11{\cdot}9$
$f = 9{\cdot}7$	$n = 12{\cdot}6$



The star *b* is variable to the extent of some few tenths of a magnitude, and may therefore with advantage be rejected as a comparison star; *g* is a double star, the magnitude assigned above being that of the two components seen as *one star.*

I cannot close this communication without gratefully acknowledging the courtesy and kindness of Mr. Baxendell in freely communicating to me his own methods, and in affording me all necessary explanations in cases of doubt or difficulty. It is to be hoped that the time is not far distant when the best methods of procedure in this branch of the science will find a place in our textbooks of practical astronomy.

MEAN LIGHT-CURVE OF R VULPECULÆ,
As derived from the Observations at the Woodcroft Observatory in the years 1861—1865.



A paper was also read, "On the Fall of Rain during the Different Hours of the Day, as deduced from a Series of Observations made by the Rev. J. C. BATES, M.A., F.R.A.S., at St. Martin's Parsonage, Castleton Moor," by JOSEPH BAXENDELL, F.R.A.S.

In the spring of last year, the Rev. J. C. Bates, M.A., F.R.A.S., of Castleton Moor, contrived a rain-gauge by which he is enabled to determine the amount of rain which falls during every quarter of an hour of the entire day. Observations with this instrument were commenced on the 24th of May last, but were interrupted for two days in October, owing to damage done by the violent gale of the 25th of that month, and again in December, while the driving clock was under repair. All the observations made up to the end of January have been kindly forwarded to me by Mr. Bates for examination and discussion, and although the series extends over a period of only a little more than eight months, and caution is, therefore, required in basing general conclusions upon it, yet the results I have obtained appear to me to be sufficiently remarkable and interesting to be worthy of being brought at once under the notice of meteorologists, in order that the law

of daily variation of rainfall which they indicate may be tested by the results of similar series of observations made in other localities.

The number of days in each month on which the rainfall was registered by the new instrument was—May, 5; June, 4; July, 19; August, 20; September, 3; October, 14; November, 9; December, 5; January, 21: total, 100 days. The total amount of rain registered during the 100 days was 21·854 inches.

Arranging the amounts of rainfall in 24 groups corresponding to the 24 hours of the day, and, taking the sums of the amounts in each group, we have the following results:—

Hour.	Number of Times Rain fell during the Hour.	Total Amount of Rain. Inches.
1	21	0·536
2	27	0·619
3	28	0·774
4	29	1·124
5	26	1·042
6	33	0·650
7	35	0·728
8	33	1·263
9	32	1·132
10	34	0·909
11	38	0·854
12	41	0·879
13	34	0·734
14	37	0·921
15	35	0·907
16	35	1·068
17	30	0·884
18	34	0·964
19	32	1·162
20	36	1·033
21	32	1·350
22	23	0·658
23	25	0·828
24	24	0·835
		21·854

Dividing the 24 hours into day and night periods of 12 hours each, we have—

		Inches.		Frequency.
Total rainfall from 6h. a.m. to 6h. p.m.	=	10·925	...	337
„ „ 6h. p.m. to 6h. a.m.	=	10·929	...	417

It appears, therefore, that the average fall of rain during the night was sensibly the same as that during the day; but the frequency of rain was much greater during the night than the day.

Dividing the day into four periods of 6 hours each, we have—

	Inches.		Frequency.
Noon to 6h. p.m.	4·930	......	155
6h. p.m. to midnight	5·536	......	205
Midnight to 6h. a.m.	5·393	......	212
6h. a.m. to noon	5·995	......	182

These results show that the greatest amount of rain fell in the fourth quarter of the day, reckoning from noon; and the least in the first quarter; and that the amount which fell in the second quarter was slightly in excess of that which fell in the third. The frequency of rain was least in the first quarter and greatest in the third.

The six consecutive hours in which the largest quantity of rain fell were from 4h. a.m. to 10h. a.m., the amount being 6·461 inches, and the amounts in the three following six-hourly periods were :—

10h. a.m. to 4h. p.m.	4·250	inches.
4h. p.m. to 10h. p.m.	5·939	„
10h. p.m. to 4h. a.m.	5·204	„

If now, the numbers in the last column of the above table are arranged in eight groups, and the sums of the numbers in each group taken, we have the following remarkable results :—

Hours.	Total Amount of Rainfall.	Frequency.
1, 2, 3,	1·929	76
4, 5, 6,	2·816	88
7, 8, 9,	3·123	100
10, 11, 12,	2·642	113
13, 14, 15,	2·562	106
16, 17, 18,	2·916	99
19, 20, 21,	3·545	100
22, 23, 24,	2·321	72

Projecting these numbers on ruled paper, and drawing a curved line through the points thus laid down, we obtain a well-marked curve, having two maxima and two minima, which occur at the following hours:—

Principal maximum at	8½h. a.m.
Secondary " "	8h. p.m.
Principal minimum "	2h. "
Secondary " "	1½h. a.m.

This curve has no similarity to that of daily temperature, but has a strong resemblance to the curve of diurnal variation of the barometer, with a difference of about two hours in the times of occurrence of the maxima and minima. The regular diurnal variations of the barometer in this latitude are, however, so small that it is hardly conceivable they can produce the great differences in the amounts of rain falling at different periods of the day as indicated by Mr. Bates's observations. The curve of daily variation of the intensity of atmospheric electricity has also strong points of resemblance to the rain-curve, but its night minimum is the principal one, and most meteorologists will probably regard variations of intensity of atmospheric electricity as consequences rather than causes of the changes in the amount of condensed vapour precipitated in the form of rain. No other meteorological phenomenon is at present known having diurnal variations similar to those of the rainfall; but if we turn to the phenomena of terrestrial magnetism,

we shall find that the curve representing the daily variations of declination of the magnetic needle has well marked points of similarity to that of the daily rainfall. Taking for data the mean results of ten years' observations of magnetic declination made at the Royal Observatory, Greenwich, (*Greenwich Observations for* 1859,) we have the following comparison of the times of maxima and minima of the two phenomena, the westerly deviations of the magnetic needle from the mean position, being taken with a negative sign.

	Daily Rainfall.		Daily Oscillations of Magnetic Needle.
Principal maximum	at $8\frac{1}{2}$h. a.m.	......	10h. a.m.
Secondary ,,	,, 8h. p.m.	......	$7\frac{1}{2}$h. p.m.
Principal minimum	,, 2h. ,,	......	1h. ,,
Secondary ,,	,, $1\frac{1}{2}$h. a.m.	......	3h. a.m.

Owing to the shortness of the period over which Mr. Bates' observations extend, it is probable that the results derived from them may be open to considerable correction, when a more extended series becomes available. I find, however, that when this short series is divided into *two* or even *three* groups, the main features of the daily variation are still preserved in the results of each group, and I do not, therefore, anticipate that any corrections which may hereafter be found to be necessary, will be of such a character as to affect materially the probability of a close connection between the daily variations of rainfall, and the diurnal oscillations of the magnetic needle, and I hope to be able in a future communication to bring forward other facts and results bearing upon this interesting and important question.

Whatever views may be taken as to the cause of the unequal distribution of the rainfall in the different hours of the day, it could, at all events, hardly have been expected that the greatest amount of rain would fall at that period of the day when the rate of increase of the difference between the temperature of the dew point and that of the air, is greatest; and

when, therefore, according to generally received meteorological principles, there would appear to be little probability of a condensation of vapour into the form of rain.

I cannot conclude this communication without gratefully acknowledging the kindness of Mr. Bates, in affording me an opportunity of examining his valuable observations which form, I believe, the only series of the kind ever yet made.

PHOTOGRAPHICAL SECTION.

February 8th, 1866.

Dr. J. P. Joule, F.R.S., Vice-President of the Section, in the Chair.

Mr. Sidebotham brought before the section a number of pictures, by Boulton and Watt, which had been supposed to be photographs. The examination he had made of them convinced him that they had been produced by a different process.

Mr. Dancer coincided in this opinion. He thought that the camera had been employed, but solely for the purpose of enabling the artist to trace the outline, and to enlarge or reduce the image to any required scale. The shading would be an after process, the crayon employed being made of some resinous or fatty substance mixed with the colour.

March 8th, 1866.

Dr. J. P. Joule, F.R.S., Vice-President of the Section, in the Chair.

Mr. Brothers, F.R.A.S., stated that since the last meeting he had tried the use of wax dissolved in ether, as recommended

by Mr. Rogerson, for the purpose of cleaning glass plates, and was quite satisfied it was an excellent method of cleaning glass.

A paper was read "On the Pantascopic Camera," by J. R. Johnson, Esq. Communicated by E. C. Buxton, Esq.

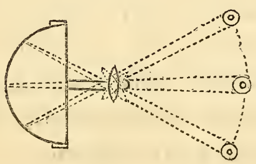
But a few years had elapsed since the introduction of the Daguerreotype, and photography had not yet passed beyond what may be termed "The Daguerreotypic Era," when the unsatisfactory nature of the "bits" of landscape produced by the lenses then in use became fully recognized. What could be more annoying to an artist desirous of producing pictures of some of the splendid architectural monuments of Paris than to find that from no possible point of view could the whole of many of the finest of these be embraced?

The desirability of not only effecting this, but of reproducing as pictures some of those striking "*coups d'œil*" which abound on the banks of the Seine, struck an eminent artist-engraver, Mr. Martens, so forcibly that he immediately set about finding a remedy, and he was not long before he had attained his object.

He found that if a lens be moved horizontally upon a pivot placed beneath its optical centre, the image is stationary notwithstanding the motion of the lens.

Now as all subsequent rotating cameras are based upon this principle it is desirable that we should clearly understand it, and the instrument I am about to describe, and which is intended to be used for viewing panoramically views produced by a rotating camera, will enable us to do so.

Fig. 1.



In fig. 1 we have a lens mounted in a socket so as to turn horizontally, and behind it we have a piece of cardboard bent to form part of a cylinder, of which the focal length of the lens is the radius.

Now, if in front of this we place a taper, an image of the taper is formed upon the cardboard screen. We may turn the lens backwards and forwards through a short arc, and yet you will see the image remains stationary. If we move the taper sideways keeping it at the same distance from the lens and turn the lens towards it, an image is produced on a different part of the screen. If a sufficient number of candles had been placed before the instrument and we had limited the action of the lens upon the screen to a narrow vertical band of light, the screen would have received consecutively the images of all the candles laid side by side, and perfectly distinct from each other.

Now if for the cardboard we substitute a bent Daguerreotype plate, and if for the ordinary reading glass we put a good photographic lens, and for the series of tapers any landscapes or series of lighted objects, we shall have an exact reproduction of Martens' camera. He took a number of beautiful views of Paris, from the bridges of the Seine, but the instrument failed in other hands than his. The difficulty of polishing and preparing large silver plates was of itself a serious obstacle, and a still greater want of success arose from the fact that Mr. Martens not being familiar with the practice of mechanics, did not attempt, or failed to produce the motion of the lens by self-acting means, and it requires the greatest skill and care to maintain a uniform motion for the length of time necessary for due exposure by means of the hand alone.

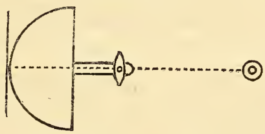
Even in the hands of Mr. Mayall, so successful in large Daguerreotype plates, and so careful and intelligent an operator, the instrument failed completely. He worked for a long time at Niagara, unsuccessfully, before obtaining the few views he brought away with him.

After the Daguerreotype give place to albumen and collodion on glass, Martens' camera was laid aside, but in 1854 he and a nephew made an effort to replace the bent plate at the back of the camera by a flat plate; and he contrived an apparatus for giving such a motion to the plate and lens respectively, that he succeeded in getting pictures upon such a plate more or less successfully.

To show what these motions are, we will go back to the instrument which has served to illustrate Martens' first camera. We will remove the bent cardboard, and making it straight, we will place it against the circular edge which previously held it bent.

If the taper be placed directly opposite the centre of the instrument, the flat paper must be at right angles to the ray of light, at a tangent to the curve on which the circular plate was bent, and the centre of the plate must be opposite the taper.

Fig. 2.



If the light be shifted to *b* fig. 3 the lens must be turned, and the plate must also be turned so that it is still at right-angles to the ray, and tangential to the curve as before, but the image must be received away from the centre. This will bring us to the position fig. 3 and if the one taper be moved in the opposite direction the lens and plate will be as shown in fig. 4.

Fig. 3.

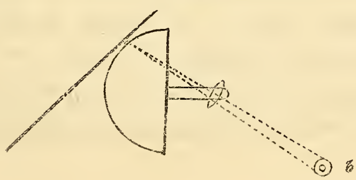
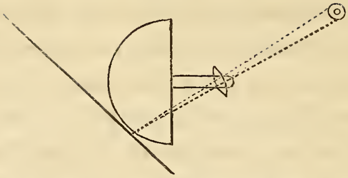


Fig. 4.



If the rotation of the lens and the change of position of the plate be made with due regularity perfect definition is obtained, but the greatest accuracy is necessary to obtain this.

To return to the history of this instrument. Mr. Martens by a note to the Academy registered the fact of having produced views in this manner, and his instrument was laid aside until it was disinterred for the first time we believe last summer. As far as we are informed, although Mr. Martens has yearly produced splendid views of Swiss scenery and has regularly published them, yet no product of the machine has been shown by him since 1854, the elaborately engraved panoramas of the Swiss alps having been produced by laying views taken by the ordinary camera side by side, and copying them juxtaposed.

Various other gentlemen subsequently undertook to solve the problem. Among others may be mentioned M. Garella, an engineer officer employed in Algeria; Mr. Sutton, so well known for his high attainments and inventive talent; Mr. Hosmer, &c.

The plans of these gentlemen have been published. Other plans have been heard of, as those of Mr. Rawlinson, of Keswick; Mr. Stuart, an Indian officer—but those plans have not been made known.

It would be an interesting subject to examine all these projects in detail, but to understand them would require numerous diagrams and perhaps models.

In general terms it may be stated that all of those known, including that of Martens, possessed two features which render

them quite distinct from the instrument we are about to describe, and which will, we believe, serve to account for the fact of these projects remaining as such, or never at least passing from the first to a more perfect stage of development.

These features are,

1. The instrument was placed upon a large table upon which the pivot was fixed and upon which the plateholders rolled, the motion of the supporting rollers describing complicated curves upon the table, which curves Mr. Sutton recognised as being evolutes of the circle of rotation.

2. No direct mechanical means were employed for effecting the relative motion of the plate and lens, guide curves formed by trial or less efficient means being alone used.

I believe I may state without fear of contradiction that up to the year 1862 no views produced by any of the inventions alluded to had been publicly shown, nor views by any similar instrument purporting to produce pictures on true panoramic projection by continuous motion. Several tolerably successful attempts had been made to place two or more views side by side upon the same plate by means of a shifting back. But these compound pictures are merely views in plane perspective placed side by side upon the sides of a polygonal prism, and have no pretensions to be considered views in panoramic or any other correct projection.

It was not until the year 1862 that I succeeded in taking panoramic views with any degree of success. I had been employed in devising plans for effecting this object since 1860, aided by a working mechanic, Mr. Harrison.

After passing over the same ground as some of the gentlemen above referred to without being aware of their labours, I succeeded in 1862 in perceiving that the complicated motion of the plate and lens might be really resolved into two simple movements, a circular motion of the whole apparatus, and a rectilinear motion of the plate; and that if the motion of the

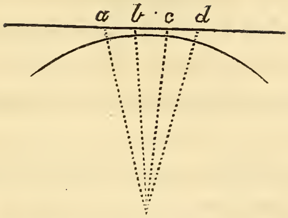
camera revolving be made to carry with it a strait rail at the back, the plateholder has only to traverse this in a right line to give the due motion required.

By forming a groove upon the edge of the circular base plate upon which the camera rotates, and turning this to the proper depth, the due rectilinear motion of the plate behind the lens may be given by means of two strings or wires, one end of each of which is attached to the disc or base plate and the other to the carriage containing the plateholder, so that if we remove the clockwork by releasing the spring which holds it up to the inner edge of the circular base, we can obtain all the necessary motions for working the camera without either wheel or pinion or any mechanism whatever unless the wires and disc and the five friction rollers upon which the apparatus moves can be called such. Views may be taken with the instrument in this state, but the accuracy necessary to produce an equal tint, particularly in the skies, is unattainable except by clockwork. The latter is extremely simple. It is a small clock movement moved by a spring, chain, and fusee, and is regulated by a fly or series of flys, the blades of which may be set at any angle from the horizontal to the vertical. It drives the instrument by means of a small steel pulley gearing into the milled edge of the circular base by a process which may be called a compromise between frictional and toothed gearing, and which has advantages that neither of these possesses singly.

In my rude attempts to illustrate the principle of the rotating camera I said that the ray of light passing through the lens must be limited. The reason of this is obvious. The true surface for receiving the image is a cylinder, but that substituted in the flat plate camera is a plane surface.

Now, the smaller the angular aperture of the narrow strip of light employed, the nearer the tangent approaches the curve.

Fig. 5.

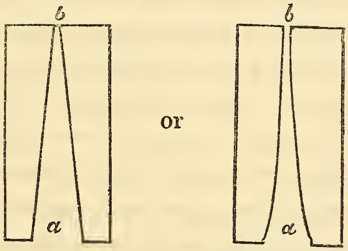


There is great discrepancy between the curve and straight line, when the width of the strip is equal to *a d*, fig. 5, but the difference is inappreciable when limited to *b c*.

The width of the active rays thus employed, is determined by a diaphragm, with shifting sides, placed at the back of the camera, near the surface of the plate, and advantage has been taken of this in order to produce clouds, and generally atmospheric effects not attainable in the ordinary camera.

By making the aperture taper thus

Fig. 6.



the narrow portion being opposite the sky, it is obvious that a totally different exposure will be given according to the width of the opening, and directly proportionate to the extent of that opening.

If the part (*a*) be fifty times the width of the part (*b*), then the time of the exposure of the first plane of the picture and that portion of the sky which approaches the zenith will be as 50 to 1.

Now we know that we can get the most delicate cloud by a sufficiently short exposure, and that we can also get parts

in deep shadow by an exposure sufficiently long, but hitherto we have not been able to obtain these simultaneously.

Several hundred views upon the table will show to what extent of perfection this principle of unequal exposure has been carried.

As all of these views were obtained during last season, many of them at elevations of from eight to ten thousand feet above the sea; and as the parts of the larger instruments had to be detached and carried separately and then mounted together on arrival at the elevated station, it may be safely alleged, we think, that the pantascopic camera is efficient in producing what it professes to do, and that it is a practical instrument bearing the necessary amount of rough usage inseparable from a campaign like that undertaken by the photographer who produced these views.

Having already occupied so large a portion of your time, I will only add a few words as to the nature of the perspective in which these views are produced.

It is a very common error to suppose that there is only one sort of perspective, and I was surprised to see that when the pantascopic camera was first explained to the Photographic Society of London, the vulgar view was entertained by at least two members of the council of that learned body.

On that occasion, two gentlemen entered their protest against the assumption that the views shown or taken by the camera were true.

The inventor very pertinently asked, What is truth—what is your standard? Do you mean that the views are not in plane perspective? But I have already said they are panoramic views. How can they be in plane perspective if they are panoramas? It was evident that to them there was but one perspective; but it has been said by competent authority that there are as many kinds of perspective, as there are imaginable surfaces upon which to receive the visual rays passing from objects to the observer. At least three such

systems of perspective are acknowledged and acted upon by artists. We may call these *spherical*, *cylindrical*, and *plane* perspective.

A painting upon the interior of a spherical dome, or a view taken by Sutton's beautiful spherical lens, is an illustration of the first system; Burford's or Selon's views on the walls of a circular apartment, or the pantascopic views, illustrate the second system; and an ordinary perspective drawing, or a view taken with a triplet or new doublet lens, furnishes us with an illustration of the third system, or that which is usually employed by artists.

Now all these are really true, but to be absolutely so, the pictures must be viewed upon surfaces similar to those upon which they have been projected, and from the same station point.

Mr. Sutton's pictures must be taken and viewed upon bowls of glass, the eye being in the position of the lens when the view was taken. With Burford's panorama we must place ourselves in the centre of the room and turn round so as to view the picture as we should view the landscape it represents.

This is equally true of a picture in plane perspective, so that if we really insist upon absolute truth in viewing a picture taken by a lens of four-inch focus, the picture must be brought within that distance from the eye, or the extreme lateral objects will not fall upon the retina at that angle which they would do in viewing the landscape itself.

As our object is usually to produce pleasing pictures, rather than an absolutely truthful representation of the scene, the discrepancy between the pantascopic pictures viewed flat is rarely objectionable, indeed, if we examine the numerous views before us, we shall see that in many of them the widening of the base line is advantageous, and often adds to the value of the picture, by giving better arrangement or composition. Perfect truth may at all times be obtained by

curving the picture, or by mounting it in an instrument I have devised for the purpose, and which I propose to call a Pantascope or Orthoscope. In that instrument the curved picture and lens have a definite relation to the lens and camera employed, so that the images are seen exactly in *situ*, and if for the lens we substituted a theodilite or similar instrument, the horizontal bearings of each object could be taken as in nature. As only a narrow strip of the field is employed, the view is free from the distortion which we should have had with the same lens covering a large field.

It will be seen, on examining pictures in this instrument, that from the fact of the lens being thus truly placed we have on looking through it such admirable light and shade, and such perfect modelling of the objects, that the effect is artistic relief in the highest degree, almost in fact competing with the solidity due to binocular vision. As the lens magnifies the objects considerably, beautiful detail is obtained, the geological character of the distant rocks and the peculiar character of the ice of the glaciers being represented with most striking effect.

These views have been shown to the Alpine Club and to Professor Forbes of Edinburgh, whose labours on the glaciers are so well known. All concur in considering them the most perfect representations of the subject which have been yet produced.

Mr. Buxton said that he could add his testimony as to the amount of rough usage which a Pantascopic Camera would bear without being rendered unfit for work. The camera he had used last summer was terribly knocked about on its way from London, and afterwards in Scotland, but the machinery always worked as smoothly as he could desire.

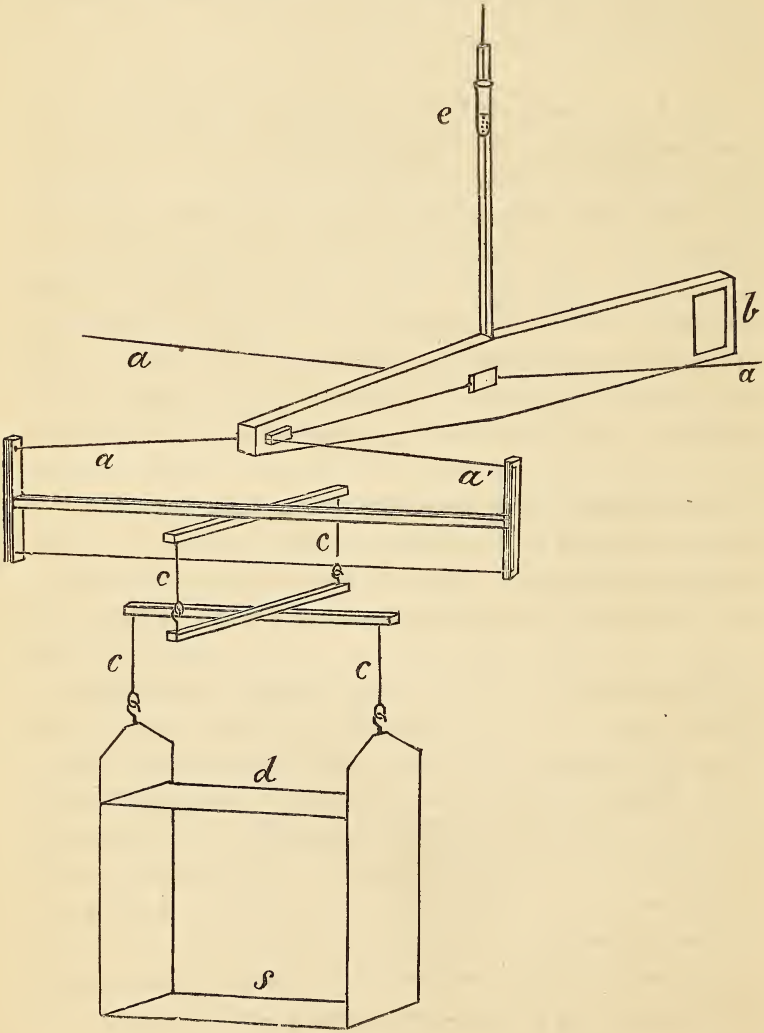
Ordinary Meeting, March 20th, 1866.

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

Mr. John Patterson was elected an Ordinary Member of the Society.

Dr. J. P. Joule, F.R.S., exhibited a balance which he had constructed on the principle which had been introduced by Professor Thomson, and employed by him in weighings for a long time. The adjoining figure will fully explain the instrument. The beam has a leaden weight let into its extremity *b*. It is supported by a wire *a a* stretched between the sides of the box containing the balance. This wire is led round so as to form the suspender *a′ a′* of the scale. Silk threads *c c*, *c c*, hanging from the cross pieces, form a gimbal system by which the scale is supported in such a manner that any variation in the position of the weights does not alter the torsion of the suspender. A counterpoise of known weight is placed on the stage *d*. When an article is to be weighed it is placed in the lower part of the scale *s*, and then, the counterpoise being removed, weights are placed on the stage to effect the counterpoise in the new condition. The difference between the first and second counterpoises of course gives the weight required. The upper edge of the beam is furnished with an index for showing minute effects; and attached to this is a small bottle *e* for holding shot or sand, by the addition of which the stability of the beam may be decreased to any required extent. The instrument exhibited was able to weigh articles of upwards of 3,000 grains to one hundredth of a grain. Dr. Joule stated that he had also employed Professor Thomson's principle in the construc-

tion of a galvanometer for the absolute measure of electrical currents. In this instrument a flat coil is suspended between two fixed flat coils, one of which attracts while the other repels the suspended coil, to which last the current is conducted by means of the suspending copper wires. This electrical balance is sensitive to one part in two millions.



Mr. Binney, F.R.S., exhibited a singular mineral which Mr. Ward of Longton had found in a nodule of clay ironstone from the North Staffordshire coalfield. At first sight it looked like a fossil coral of the genus *Cyathophyllum*, but on more careful examination it appears to be a mineral mass in a semicrystalline state. The form of the mineral appears to have been spheroidal with crystals radiating from the centre. By the kindness of Dr. Crace Calvert he had ascertained the specimen to consist chiefly of carbonate of lime, carbonate of iron, and phosphate of lime, with traces of magnesia, alumina, and organic matter, and ten per cent of silica.

He also exhibited a beautiful white specimen of carbonate of strontia obtained from a vein of carbonate of lime. It occurred among the lime in radiated masses similar to those of carbonate of barytes as sometimes found in veins of sulphate of barytes. This mineral has been found in considerable abundance, but up to this time it is believed that no use has been found for it on a large scale.

Messrs. Hull and Brockbank exhibited specimens of the iron ores referred to in their paper "On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties," read at the last meeting of the Society.

Professor Roscoe stated that he had just received a letter from Professor Bunsen, announcing the discovery of a most interesting and important fact, namely, that the well known black absorption lines of the Didymium spectrum, when examined with polarised light, vary according to the direction in which the light is allowed to pass through the crystal. This shows that the position of the black absorption lines is in some degree dependent upon the physical structure of the body through which the light passes, and is not merely determined by its chemical constitution.

A paper was read entitled "Notes on Cotton Spinning Machinery. Part II.—Roving Frames." By J. C. Dyer, Esq., Vice-President.

Having in Part I. of these Notes given an account of the inventions and improvements that had been applied to the mule jenny, and for converting it into the self-acting mule, the present paper in like manner traces the origin and progress of the machines known as roving frames, which come next in the order of scientific interest, as having engaged the labours of many eminent mechanics, through a long course of years, to bring them into their present accurate form of working. The rovings are equalised by doubling and drawing before coming to the roving frame, in which they are again drawn and slightly twisted to fit them for spinning; but the twist is not enough to give them strength to be taken up by the drag of the bobbin, as is the case in throstle spinning, so that the bobbins must be driven separately from the fliers and their speed must vary according to their different diameters, so as to make the surface motion of winding on to correspond with that of the delivering rollers, and when the latter motion is changed the former must be made to agree with such change, which is called the differential motion; and to make these agree with the frequent changes required was attended with much trouble and difficulty in working, which ultimately led to the introduction of the machine called the "tube roving frame," in which the said differential motion was wholly dispensed with. In this frame the bobbins are mounted on cylinders and turned by the friction of their surfaces. Being geared with the delivering rollers, their motions were thus made to agree at all speeds. But another property of importance in the tube frame was the twisting and untwisting of the rovings and the pressing of them hard upon the surface of the bobbins, so that the surface motion could be made accurate. This tube frame to a large extent was substituted for the bobbin and fly frame for

low numbers, but it could not be made applicable to rovings for fine spinning; but the pressing of the rovings hard upon the bobbins in the tube frame rendered the application of the presser to the fly frame of great importance. After many experiments this object was effected, and the application of the presser to the bobbin and fly frame was carried into effect by means of springs, or the centrifugal action of revolving weights attached to the arms of the fliers, and both of these methods were set forth in the patent for those improvements. Nevertheless several competing parties afterwards obtained patents for very trivial changes, called "inventions," for applying the said spring and centrifugal actions to press the rovings upon bobbins.

The differential motion of the bobbin and fly frame continued very imperfect until Mr. Henry Houldsworth's important invention of a self-adjusting apparatus for giving the differential motions required. This discovery of a very simple train of movements for securing the required rotations of the delivering rollers of the bobbins and of the fliers will at once be recognised as an invention of a high order in mechanical science. From the limits of this abstract the main features only of these delicate movements can be given, and for the details the paper must be consulted *in extenso*. I may here add that about sixty years ago Mr. John Kennedy employed rotating tin cans for giving twist to rovings; but as they could not be driven with sufficient speed, the old throstle spindle, enlarged to suit rovings, was adopted for the frames called the "slubber" and the "bobbin and fly," and these frames, with many improvements by the same gentleman and others, were in extensive use until partially superseded in the year 1828 by the tube frame as before mentioned; and the suggestive nature of new inventions and discoveries as leading to the production of others is strikingly shown in the successive application of those in the tube frame to the bobbin and fly, as before said.

Again, Mr. Houldsworth's beautiful differential motion, then called "Jack in the box," being governed by bands and pulleys, their chance of slippage rendered it desirable to substitute toothed wheels, on which occasion Mr. John Kennedy and Mr. Peter Ewart each discovered the three-wheel motions since adopted—those of Mr. Ewart were mitre wheels, Mr. Kennedy's, spur gearing—the latter being mostly preferred. And it is worthy of note that the three mitre wheels for giving the differential motion in the slide lathe, required for turning cones, had been long in use and publicly seen without any one having dreamed of the application of them to other objects; nor was Mr. Houldsworth at all aware of these slide lathe differentials until pointed out to him by Mr. Ewart.

The paper concludes by an earnest appeal to Mr. Houldsworth and other eminent mechanicians personally connected with the progress of modern inventions and improvements of the machinery employed in our manufacturing establishments, to record their own experience and observations upon the several branches with which they have been more especially conversant, as has been done by Dr. Fairbairn in his published works, and as the author of these Notes has aimed to do.

The following papers were read at the Photographical Section Meeting, February 8th, 1866.

"On the Supposed Photographs by Boulton and Watt." By Joseph Sidebotham, Esq.

About three years ago the scientific world was startled by the announcement of the discovery of sun pictures, on paper and on silver plates, said to have been produced at the close

of the last century, by Matthew Boulton and James Watt; shortly afterwards, Mr. Smith, of the Patent Museum, read a paper on the subject, and exhibited the pictures in question at a meeting of the London Photographic Society, and also produced copies of many documents connected with the subject. The whole was published in the *Journal of the Photographic Society*, together with the discussions, and a large amount of correspondence appeared in the journals. No conclusion, however, appeared to be arrived at, nor any suggestions made of the process by which the pictures on paper could have been produced.

During the last winter, in company with Mr. James Nasmyth, I paid a visit to the Patent Museum. Through the kindness of Mr. Smith, we had an opportunity of examining these pictures carefully, hearing what he had to say on the subject, and seeing some of the original letters and papers. Mr. Smith also gave me a small portion from one of the torn pictures, and has since sent me another, for the purpose of careful examination. Through his kindness, also, I am enabled to exhibit to you this evening a number of the perfect pictures, in the hope that, by your seeing and examining them, some light may be thrown on the secret of their production.

It will be, perhaps, well to give you a short historical sketch of the pictures. Those who wish to refer at length to the published accounts, will find them in vol. viii. of the *Journal of the Photographic Society*.

The pictures in question consist of a number on paper—some large (so large, indeed, that it requires two sheets to form one subject), others small. They vary in shade of colour from black to dull red—many being of a sepia tone. Some are plain, others coloured. These pictures came from Boulton's old house at Soho, and many of them are evidently experiments, being marked with large figures in pencil. The plates are two silvered copperplates, also found in the

old library at Soho. These we will, however, leave for the present, and proceed with the pictures on paper.

According to the evidence of letters and other documents produced by Mr. Smith, Matthew Boulton was in possession of a secret plan for producing what he called "mechanical pictures"—the inventor, or partial inventor, of the process being a person of the name of Eginton, who appeared to superintend this department of the Soho establishment. These pictures (all apparently copies of paintings) were produced rapidly, and at very low prices—from seven shillings and sixpence upwards, according to the size. They appear to have all the touches of a painting. Sometimes they were transferred to canvas and painted in oil colours, sometimes tinted on the paper itself, sometimes transferred to copper plates. Orders appear to have been given to artists for the sole purpose of having the paintings to copy or reproduce. They were sold in considerable numbers, and could, it appears, be produced of various sizes according to order. The pictures on paper were all reversed, the figures left-handed. When more than one sheet of paper was required for a subject, the picture was not joined in a straight line, but curved, so that the junction fell in the shadow, as in the leading of painted windows. Eginton was a glass painter, and perhaps took his idea from that source.

There is much interesting matter published concerning a proposed government pension to Eginton, and a letter to the Earl of Dartmouth on the subject from Matthew Boulton; also letters from Mr. M. P. W. Boulton and others; but, as they have no direct bearing on the mode of producing the pictures, I merely allude to them here.

Although it is well known that Watt, Boulton, Davy, Wedgwood, and other members of the Lunar Society experimented in photography, and tried to fix the images formed in the camera, we have the direct statement that, up to the year 1802, Wedgwood had been unsuccessful—that no

amount of exposure appeared to produce an image. It is not likely, therefore, that, for years before (the mechanical production of pictures was in full operation in 1790), his intimate friend Matthew Boulton should not only have been in possession of a secret mode of fixing images of pictures but actually producing and selling large numbers of them. For this and other reasons we must, I think, decide that these pictures could not in any way have been produced in the camera; besides the great size and the perfect definition would be beyond the power of any instruments that could then be made. There is another argument, too—that if the camera could have been used, paintings would have not been the only subjects reproduced—views from nature, or, at any rate, works of art, would have been experimented upon.

On some of the pictures are seen curious small spots, each casting a shadow in the same direction—a very familiar appearance to those who have copied oil paintings in a raking light, when each raised spot of colour does actually cast its shadow. This has been considered an evidence of the pictures having been produced by the camera; but I shall further on be able to account for this appearance in another way.

Although we cannot call these pictures photographs, seeing that they have been produced by some different process from any we are acquainted with, we have the distinct evidence of Dr. Lee and Mr. Hodgson, *clear and unmistakeable,* that pictures produced by this mechanical process were pointed out by Matthew Boulton as having been produced in some way by sunlight.

Perhaps we shall do well to narrow the field of inquiry, by considering some of the suggestions that have been made, and showing how they could *not* have been produced.

We may decide that they could not be copies by hand:— 1st. Because they are distinctly called "mechanical pictures," produced by a secret process. 2nd. Because in two copies of

the same subject such minute lines and marks are found to exist in both—lines not visible without the microscope, quite impossible to be copied by hand. Besides, we have the testimony of those whose lives have been spent in engraving and making *facsimile* productions that the thing is impossible. They could not, I think, be impressions from metal or blocks. The peculiar surface of the paper, and the colouring matters used, would be quite unsuited to printing purposes. The great cost of engraving these large surfaces for but a limited number of copies; the different sizes in which the pictures could be made; the non-necessity, in such case, for the reversing of the pictures—are all good arguments against any system of engraving being used.

It has been said that the plate mark on some of the pictures suggests the employment of plate printing; but, on examination, as you will see, the supposed plate mark is merely an embossed line on the paper, either intended as a finish or, more probably, to mislead as to the mode of production.

The specimens for your examination consist—

1st. Of two similar pictures—one plain, the other coloured. A careful examination of these will show that spots and lines only visible with a lens exist exactly the same in each, such as no artist could possibly copy; yet, strange to say, there are considerable differences—some of them striking; but these are of precisely the same nature as you would have in an under-exposed and an over-exposed print, both from the same negative.

2nd. We have a coloured mechanical picture—subject, *The Graces Awakening Cupid.* This may be compared with an engraving of the same subject, evidently both taken from the original painting. In this you will notice the mechanical picture is reversed.

3rd. We have a red mechanical picture—*Flora Bedecking Pan.* This may also be compared with an engraving of the

same subject, or rather one somewhat similar. In the engraving another figure is added; and there are other differences. In this case the original picture had probably been reproduced with additions, and the engraving taken from the later one.

4th. A large picture in two parts, from a painting by Benjamin West. You will notice, as peculiar, the mode in which the two portions of the picture are intended to be joined together; also that the two halves are not of the same tint, either in shade of colour or depth.

I will now give you the result of my examination of the pictures and the fragments given to me by Mr. Smith for the purpose of analysis. The surface of the paper appears, first, to have been prepared with gum and sugar. On that is the image impressed, consisting of finely divided particles, apparently laid on either in the form of vapour or very fine powder. Over the picture is a coating of albumen. This has been applied, most likely, by floating the picture on the surface of a vessel containing albumen. The picture has then, probably, been taken up carefully and allowed to drain for a short time, and then laid flat to dry. Small air bubbles, or particles of dust, on the surface would just produce the curious appearance of projections and shadows before-mentioned—the powdery surface being slightly carried away and deposited, just as we see it. Those who have made experiments in photography—such as in the old carbon process, &c.—will at once fully understand my remarks. The albumen, in drying, has run into the hollows of the paper, as we see in the specimens. It is easy now to see how the images could be transferred to canvass, or painted upon on the paper.

How the images were formed I cannot even venture a suggestion. The process, if re-discovered, would be still valuable, even with our other and various modes of reproduction. For effect and beauty the specimens now shown are

not to be despised; and, for permanency, have had the test of nearly eighty years.

Many of these pictures must be in existence in old houses and country inns; and, if more specimens were obtained, some clue to the secret might be found. I feel confident I have more than once seen specimens. Once, in particular, when on a photographic trip, in 1853, with our old member, Mr. Barton, either at Ludlow or Hereford, we saw several of them, and puzzled ourselves to make out what they were, with their striking photographic appearance. At length we decided that they must be sepia drawings.

The pictures on silver plates are two, and, on certain evidence, thought to be views of Soho House before the alteration. As this took place at the end of last century, these pictures, according to that idea, must have been taken at least sixty-six years ago. All we have to judge upon is this evidence, which is rather weak. The pictures are evidently taken in the camera, and are genuine photographs. From the evidence published in a pamphlet by Mr. M. P. W. Boulton, it appears highly probable that these pictures were taken about twenty-eight to thirty years ago by his aunt, Miss Wilkinson.

Independently, however, of this, I fear we must give them up as modern productions. Their appearance is that of daguerreotypes of the early period, made sensitive with iodine alone; and the image, as may be seen through the microscope, is composed of mercury vapour. But another piece of evidence appears to me more conclusive still, and that is the size of the plates. I have here a daguerreotype view of Rome, taken in the early days of the art—one of a number taken at that time in Rome and Paris. If we now compare the size of the plate with the Soho pictures, we shall find them identical. Such could scarcely be an accidental coincidence. These plates were not made or used for any other purpose; and it is not within the bounds of probability that

the standard size of pictures, taken by Daguerre in 1835, should have been precisely the same as those used by James Watt in 1799. However, from the stamp on the corner of my plate, the peculiar form of the figure four (4) points it out as a French production; whilst the Soho plates are apparently English, rendering the possibility of their exact coincidence in size still more improbable.

"Speculations on the Process employed by Messrs. Boulton and Watt in the Production of the Pictures called by them 'Mechanical Pictures.'" By J. B. Dancer, F.R.A.S.

My remarks at present are confined to the two pictures on the table, of human figures, numbered 7 and 8. They measure $17 \times 13\frac{3}{8}$ inches, and are identical in size and subject. By the kindness of Mr. Sidebotham I have had an opportunity of making a minute examination of them. The only apparent difference between them is, that No. 7 is in plain ink, and in No. 8 the garments of the figures are coloured—one red and the other blue.

I am informed that these pictures are similar to those which were supplied by the firm of Boulton and Watt, and copies from originals sent to them. They were issued, it is said, with tolerable rapidity, and at a very moderate price; but the process by which they were multiplied was kept a profound secret.

At the first glance the pictures look as if they were produced by hand; but, on comparing them carefully, the close resemblance in the drawing in each is found to be so remarkable that no artist, however clever, could produce such exact duplicates without great expenditure of time. A more minute examination by means of a lens shows scratches and lines (evidently accidental) which correspond accurately in each picture, affording a convincing proof that these copies could only be produced by some mechanical or chemical agency.

It has been stated on good authority, I believe, that a darkened room or tent was used, also that the presence of sunlight was required in the process. Now this at once suggests the use of the camera obscura — an instrument which was perfectly well known at that period. A common method of exhibiting this instrument was to darken a room and fix a lens in a hole made in the window-shutter, and view the images produced on a screen of paper placed opposite to the lens. By this method a reversed and inverted picture would be seen by the spectator.

Another form of camera in use at that time consisted of an upright box with a sliding tube through the top, in which was fixed a lens, and above this a mirror placed at the angle of 45°. This form is still in use for the purpose of tracing images thrown on to paper placed at the bottom of the box. These primitive forms of the camera must be well known to all present. I name them only as affording some explanation for the use of the darkened room or tent.

The next question which suggests itself is this:—Did they employ the camera in producing images on chemically prepared surfaces? In fact, did they practice what we now name photography?

It is stated that in 1802 Wedgwood and Davy experimented with salts of silver on leather, and produced impressions of various objects. Now if we may be allowed to imagine that Mr. Boulton and his colleagues were acquainted with this effect of light on such substances, they might have reasoned on the phenomena, and prepared paper with these constituents — tannic acid, gelatine, and some salt of silver. Had they done so they might have produced a picture by sufficient exposure in the camera. One thing is certain, the pictures 7 and 8 have a plentiful coating of gelatine, albumen, or some other substance on their surface.*

* My son William Dancer tested this substance and pronounced it to be gum.

At this stage of the inquiry I should like to ask a question of those who have inspected a large number of these pictures. Do any of them show unmistakeable marks of the originals, such as we are all familiar with in photographic copies? If this could be favourably answered, then photography of the present day would be benefited by the discovery of the means adopted in fixing these pictures. I much doubt if one of our best photographs on paper would contrast favourably with these pictures in the whites after a lapse of seventy years. But in my humble opinion photography has not been the agent employed in producing these pictures.

Mr. Sidebotham had a portion of one picture presented to him, and he kindly shared this with me. In my experiments I found that the colouring matter forming the picture could be washed off like ordinary ink, without leaving a trace of any chemical action on the paper. Now if this portion of the picture be a genuine sample, it does not exhibit the ordinary characteristics of a photograph.

On further consideration there are many reasons which would lead me to conclude that these pictures have been produced by some modification of mezzotint engraving, in which the camera had enabled the engraver to trace his outline from the original to any scale required, giving him the correct outline drawing, the shading being an after process; but the rapidity of production, and the low price charged, unfortunately excludes this process from the list of probabilities. Although mezzotint is said to be the most rapid method of engraving, I am compelled to abandon it or its modifications, and pass on to some other process.

Now if we do not believe in the photographic portion of the secret, and are not permitted to employ engraving on metal on account of the expense and labour, we find the field of speculation becoming somewhat limited.

Now comes a very important question:—Did Mr. Boulton anticipate Senefelder in some process similar to lithography, employing metallic plates or metallic alloys? I can almost

imagine that such was the case. Even prepared paper surfaces, such as are named in Senefelder's patent specification, dated 1801, might have been used at Soho. When we look at the rough hand-made paper on which these impressions are placed, and see the thick coating of gum or gelatine on the surface, the question then arises—Is this prepared surface necessary for taking up the ink or whatever was used? or is it merely to smooth over the coarse wire lines in the paper? We do know that the want of smooth paper interfered with the practice of lithography for a long period. Machine-made paper was a great improvement, but now paper is made especially for the purpose.

After glancing over these processes, I am led to imagine that the following mode may in some degree serve to produce pictures similar to those before us:—Admitting that the impressions were touched up by hand, I think the camera was employed only for tracing the outline and reducing or enlarging the image to any required scale. The pictures Nos. 7 and 8 give good evidence of tracing. The shading would be an after process, having the original for a copy, the crayon employed being made of some resinous or fatty matter mixed with the colour—this production then to be used as a transfer on the prepared surface.

In such pictures intended for painting, the ink would be light in colour—in others red or black, as the taste of the artist dictated. In this manner, as Senefelder states in his specification, the various styles of etching, stroke engraving, drawings in black and red chalk and aquatinta, &c., &c., could be imitated, and at small cost compared with any other known process.

I shall now leave this very interesting inquiry in your hands. As an excuse for the brief and imperfect handling of the subject, I may state that it is only two days since I first saw the pictures, and I have not had sufficient time to work up the subject in a proper manner; but if my speculations serve to provoke a discussion amongst the members of the Section, its purpose will have been answered.

Ordinary Meeting, April 3rd, 1866.

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

Messrs. Wm. Brockbank and G. C. Lowe were appointed Auditors of the Treasurer's accounts.

Mr. Binney, F.R.S., said that he had observed the humming bird hawkmoth (*Macroglossa Stellatarum*) during the past summer in far greater abundance than he ever remembered having seen it before. In the month of August, he saw upwards of a hundred of them in a garden near Grimsby, were they appeared to prefer the common lavender flower for food to any other in the place. Again in the first week of October, he observed upwards of twenty in a garden at Douglas, in the Isle of Man. Here they preferred to feed on heliotrope before other flowers. It was very interesting to watch these moths hovering over the flowers, and whilst on the wing extracting their food. They appeared very wary and shy after any attempt being made to capture them, but if you merely observed without making any attempt to molest them they would continue their feeding in confidence, and you could watch them at your leisure. So a great deal of the shyness and caution for which the little creature has got the credit of, is probably more due to the persevering efforts of its enemies to capture it than any natural fear of man.

A paper was read "On a Logical Abacus," by W. S. Jevons, Esq., M.A.

The author believed that this was the first attempt, or at all events, the first successful attempt, to reduce the processes of logical inference to a mechanical form. The purpose of this contrivance is to show tbe simple truth, and the perfect

generality of a new system of pure Qualitative Logic closely analagous to, and suggested by, the mathematical system of logic of the late Professor Boole, but strongly distinguished from the latter by the rejection of all considerations of quantity.

This logical abacus leads naturally to the construction of a simple machine which shall be capable of giving with absolute certainty all possible logical conclusions from any sets of propositions or premises *read off* upon the keys of the instrument. The possibility of such a contrivance is practically ascertained; when completed it will furnish a more signal proof of the truth of the system of logic embodied in it. Still the more rudimentary contrivance called the *abacus* will remain the most convenient for explaining the nature and working of formal inference, and may be usefully employed in the lecture room, for exhibiting the complete analysis of arguments and logical conditions, and the exposure of fallacies.

The abacus consists of—

1. An inclined black board, furnished with four ledges, 3ft. long, placed 9in. apart.

2. Series of flat slips of wood, the smallest set four in number, and other sets, 8, 16, and 32 in number, marked with combinations of letters, as follows:—

First Set.

A	A	*a*	*a*
B	*b*	B	*b*

Second Set.

A	A	A	A	*a*	*a*	*a*	*a*
B	B	*b*	*b*	B	B	*b*	*b*
C	*c*	C	*c*	C	*c*	C	*c*

The third and fourth sets exhibit the corresponding combinations of the letters A, B, C, D, *a*, *b*, *c*, *d*, and A, B, C, D, E, *a*, *b*, *c*, *d*, *e*.

The slips are furnished with little pins, so that when placed upon the ledges of the board, those marked by any given letter may be readily picked out by means of a straight-edged ruler, and removed to another ledge.

The use of the abacus will be best shown by an example. Take the syllogism in *Barbara* :—

Man is mortal.
Socrates is man.
Therefore Socrates is mortal.

Let

A = Socrates.
B = Man.
C = Mortal.

The corresponding small italic letters then indicate the negatives.

a = not-Socrates,
b = not-Man,
c = not-Mortal,

and the premises may be stated as

A is B,
B is C.

Now take the second set of slips containing all the possible combinations of A, B, C, *a*, *b*, *c*, and ascertain which of the combinations are possible under the conditions of the premises.

Select all the slips marked A, and as all these ought to be B's, select again those which are not B, or *b*, and reject them. Unite the remainder, and selecting the B's, reject those which are not C or *c*. There will now remain only four slips or combinations :

A	*a*	*a*	*a*
B	B	*b*	*b*
C	C	C	*c*

If we require the description of Socrates, or A, we take the only combination containing A, and observe that it is

joined with C, hence the Aristotelian conclusion *Socrates is mortal.* We may also get any other possible conclusion. For instance the class of things *not-Man* or *b* is seen from the two last combinations to be always *a* or *not-Socrates*, but either *mortal* or *not-mortal* as the case may be.

Precisely the same obvious system of analysis is applicable to arguments however complicated. As an example take the premises treated in Boole's Laws of Thought, p. 125.

(1.) Similar figures consist of all whose corresponding angles are equal, and whose corresponding sides are proportional.

(2.) Triangles whose corresponding angles are equal have their corresponding sides proportional, and *vice versa.*

Let

A = similar.
B = triangle.
C = having corresponding angles equal.
D = having corresponding sides proportional.

The premises may then be expressed in Qualitative Logic,* as follows :—

$$A = CD.$$
$$BC = BD.$$

Take the set of 16 slips; out of the A's reject those which are not CD; out of the CD's reject those which are not A; out of the BC's reject those which are not BD; and out of the BD's reject those which are not BC. There will remain only six slips, as follows :—

A	A	*a*	*a*	*a*	*a*
B	*b*	B	*b*	*b*	*b*
C	C	*c*	C	*c*	*c*
D	D	*d*	*d*	D	*d*

From these we may at once read off all the conclusions laboriously deduced by Boole in his obscure processes. We

* *See* Pure Logic, or the Quality of Logic, apart from Quantity, by W. Stanley Jevons, M.A., London (Stanford), 1864.

at once see, for instance, that the class *a*, or "dissimilar figures, consist of all triangles (B) which have not their corresponding angles equal (*c*) and sides proportional (*d*), and of all figures not being triangles (*b*) which have either their angles equal (C) and sides not proportional (*d*), or their corresponding sides proportional (D) and angles not equal, or neither their corresponding angles equal nor corresponding sides proportional." (Boole, p. 126.)

The selections as made upon the *abacus* are of course subject to mistake, but only one easy step is required to a logical machine, in which the selections shall be made mechanically and faultlessly by the mere reading down of the premises upon a set of keys, or handles, representing the several positive and negative terms, the copula, conjunctions, and stops of a proposition.

Mr. Jevons stated his opinion distinctly that these contrivances possessed a theoretical rather than a practical importance. Like the analogous Calculating Machine of Babbage or Scheutz, the logical machine would hardly find practical employment for the present at least. But its value consisted in showing the true nature of logic as a system of analysis of the possible combinations of things, in short as the highest and simplest form of the doctrine of combinations. Not only would the deductive, and especially the inductive processes of logic be thus presented in a new and clearer light, but the relation of logic, the qualitative doctrine of combinations, to mathematics the quantitative doctrine of combinations, would be defined, and the abstract sciences thus brought into harmony and due subordination.

In the description of his balance given in the last No. of the Proceedings, Dr. JOULE omitted to mention a fixed support against which the scale rests when the counterbalance is removed. By this means the wires are kept constantly in the same state of tension, and are thus preserved from the derangement which might otherwise ensue.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

March 26th, 1866.

A. G. Latham, Esq., President of the Sections, in the Chair.

The following objects were exhibited:—

Eight mounted specimens of hair of Australian animals for the cabinet; one of them, a species of Phascogale, very remarkable.—Mr. Latham.

A large collection of rare beetles from Ceylon, recently presented to the Natural History Society by —— Braybrooke, Esq.—Mr. Latham.

Many specimens of remarkable foraminifera from Dogs Bay.—Mr. Linton.

A sample of the Guano lately imported from Malden Island in the Pacific, for distribution among the members.—Mr. Latham.

Dr. Alcock showed mounted specimens of Embryonic shells of Mollusca, including fifty species collected by him from Dogs Bay sand, and named by J. Gwyn Jeffreys, Esq.—He said he had in a former communication described the peculiar characters of Anomia in the young state, and shells of this kind are abundant in the sand. Pectens are also common, and six different forms which he had sorted out are referred by Mr. Jeffreys to the following species:—P. varius, opercularis, tigrinus, testæ, similis, and maximus. Lima subauriculata and L. Loscombii are both rather scarce. Modiolaria discors in the young state is very common. Arca tetragona, abundant; Kellia suborbicularis, common; Cardium echinatum, rare; Cardium fasciatum, very common; several species of Venus, frequent; and Saxicava rugosa, very abundant. Patella vulgata, Helcion pellucidum, and Tectura virginea with spiral caps are all common, the last

being very abundant. Another limpet-like shell without spiral cap, referred to in a former paper, is identified by Mr. Jeffreys as Ancylus fluviatilis. Emarginula fissura is rare, and of Fissurella Græca, one specimen only has been found. Cæcum glabrum with spiral attached is common, and separate spirals are abundant; only one specimen of Aclis unica has been met with; Eulimella nitidissima is common; Homatogyra rota, rare; Cerithiopsis adversa, common. A few of the embryonic and partly grown shells of Aplysia depilans have been found, but they are very scarce. In addition to the named specimens, a considerable number of other species not yet identified were exhibited.

List of Embryonic Shells of Mollusca

Anomia ephippium.
Pecten varius.
„ opercularis.
„ tigrinus.
„ testæ.
„ similis.
„ maximus.
Lima subauriculata.
„ Loscombii.
Mytilus phaseolinus.
Modiolaria discors.
Arca tetragona.
Montacuta bidentata.
„ ferruginosa.
Kellia suborbicularis.
Sphærium corneum.
Cardium echinatum.
„ fasciatum.
Venus lincta.
Venus.
Tellina tenuis.
Scrobicularia alba.
Lyonsia Norvegica.
Thracia papyracea.
Saxicava rugosa.

Patella vulgata.
Helcion pellucidum.
Tectura virginea.
Emarginula fissura.
Fissurella Græca.
Homatogyra rota.
Cæcum glabrum.
Cerithium reticulatum.
Cerithiopsis adversa.
Scalaria communis.
Aclis unica.
Eulima distorta.
Chemnitzia elegantissima.
Odostomia unidentata.
Eulimella nitidissima.
„ acicula.
Velutina lævigata.
Lamellaria perspicua.
Purpura lapillus?
Mangelia linearis.
Cylichna cylindracea.
„ truncata.
Aplysia depilans.
Spirialis Flemingii.
Ancylus fluviatilis.

PHYSICAL AND MATHEMATICAL SECTION.

March 29th, 1866.

Robert Worthington, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. Baxendell, F.R.A.S., communicated the following:—

Results of Rain Gauge and Anemometer Observations made during the Year 1865,

At St. Martin's Parsonage, Castleton Moor, by the Rev. J. Chadwick Bates, M.A., F.R.A.S.

Table I.

	5-inch Gauges.			8-inch Gauges.			Total Movement of Wind.
	20 feet elevation.	5 feet elevation.	1 foot elevation.	20 feet elevation.	5 feet elevation.	1 foot elevation.	
January	3·177	3·492	3·874	3·292	3·631	4·050	7156
February	2·344	2·493	2·934	2·572	2·775	3·033	5875
March	1·055	1·161	1·359	1·111	1·246	1·396	7275
April	1·337	1·377	1·444	1·377	1·410	1·474	7074
May	2·297	2·622	2·863	2·278	2·647	2·784	6934
June	0·656	0·668	0·659	0·670	0·666	0·669	5213
July	3·224	3·407	3·546	3·260	3·347	3·412	5751
August	4·645	4·892	5·099	4·733	4·862	4·941	5591
September	0·548	0·614	0·657	0·580	0·628	0·652	4551
October	5·777	5·010	6·251	5·858	6·030	6·230	6621
November	3·303	3·508	3·829	3·384	3·608	3·774	6097
December	1·092	1·234	1·313	1·158	1·207	1·299	6121
	29·455	31·478	33·830	30·273	32·057	33·714	74259

TABLE II.

	No. of Days of Rain.	Total Movement of the Wind.	Mean Daily Movement of Wind on Days of Rain.	No. of Fair Days.	Total Movement of the Wind.	Mean Daily Movement of Wind on Fair Days.
January	13	4394	338	18	2762	153
February	15	3622	241	13	2253	173
March	11	2873	261	20	4402	220
April	8	2103	263	22	4971	226
May	18	4501	250	13	2433	187
June	4	1155	288	26	4058	156
July	14	2822	201	17	2929	172
August	16	3284	205	15	2307	153
September	2	530	265	28	4021	143
October	18	3847	213	13	2774	213
November	14	3958	282	16	2139	133
December	8	2709	338	23	3412	148
	141	35798	253·8	224	38461	171·7

From Table I. it will be seen that the total movement of the wind was greatest in the month of March, and least in September. In the year 1864 it was also greatest in March, but least in August. The total amounts in the quarterly periods of the two years were:—

	1865.	1864.
Winter	19152 miles.	19174 miles.
Spring	21283 ,,	20940 ,,
Summer	16555 ,,	18111 ,,
Autumn	17269 ,,	20088 ,,

Table II. shows that the mean daily movement of the wind on days of rain in 1865 was 253·8 miles; and on days without rain, 171·7. The numbers in 1864 were 251·6, and 179·4 respectively.

The mean results for the quarterly periods, and their ratios, were:—

	Mean Daily Movement of the Wind on Rainy Days.	Mean Daily Movement of the Wind on Fair Days.	Ratios.
Winter	298	156	0·52
Spring	256	214	0·83
Summer	213	160	0·75
Autumn	245	156	0·63

The ratios in 1864 were, Winter, 0·68; Spring, 0·82; Summer, 0·72; Autumn, 0·60. It appears therefore from

the observations of the two years that the ratio of the mean velocity of the wind on days when no rain falls, as compared with that on rainy days, varies least in the spring, and most in the winter quarter.

Mr. W. L. Dickinson read a Paper containing the results of calculations relative to the Eclipse of the Sun, and to two Occultations of the star Aldebaran by the Moon, visible here this year. The calculations have been made for the Observatory of Robert Worthington, Esq., F.R.A.S., Crumpsall, near Manchester, Lat. 53° 30′ 50″·0 N., Long. $0^h\,8^m\,56^s{\cdot}16$ W. The Elements used in the computations have been obtained from the Nautical Almanac.

The Partial Eclipse of the Sun, October 8, 1866, is partly visible at the Observatory, and

	h. m. s.	
Begins................	4 19 39	Mean Time at Greenwich.
Greatest Phase	5 21 35	

At Crumpsall the Sun will set at 5h. 27m.

Magnitude of the Eclipse (Sun's diameter = 1) 0·480.

Angle, from North Pole, of first contact, 43° .... } towards the West for *direct* image.
Angle, from Vertex, of first contact, 76° .... }

The Occultations of the star *α* Tauri (Aldebaran) by the Moon.

1866.	Disappearance. Sidereal Time at Observatory.	Mean Time at Observatory.	Mean Time at Greenwich.	Angle from North Point.	Angle from Vertex.
	h. m. s.	h. m. s.	h. m. s.	°	°
September 28th...	3 41 36	15 10 56	15 19 52	84	75
November 22nd...	1 53 44	9 47 6	9 56 2	73	44

1866.	Reappearance. Sidereal Time at Observatory.	Mean Time at Observatory.	Mean Time at Greenwich.	Angle from North Point.	Angle from Vertex.
	h. m. s.	h. m. s.	h. m. s.	°	°
September 28th...	4 51 41	16 20 49	16 29 45	295	303
November 22nd...	2 51 55	10 45 8	10 54 4	310	290

The Angles are reckoned towards the right hand round the circumference of the Moon's image as seen in an inverting telescope.

Mr. Brothers, F.R.A.S., stated that when a solution of nitrate of silver, which, from long use, has become contaminated with organic matter from collodion plates, is neutralized, or made alkaline with carbonate of soda, exposed to strong day-light in an evaporating dish, and heat applied, a scum is quickly formed on its surface. This scum, when broken by a puff of air exhibits, in a remarkable manner, all the appearances of solar spots as seen in good telescopes. So long as the current of air is continued the scum remains open at the spot, but immediately closes *partially* on the cessation of the cause, and streams of the film stretch across the opening, illustrating the *bridges* over the sun spots. At the same time a secondary scum commences to form at the edges of the opening, and may be called the *penumbra*, and in time closes the opening. The centre is occupied by the *umbra* formed by the carbonate of silver (white when first formed) which has in the course of the experiments turned *black*. It was found that when a current of the solution was forced upwards these effects could not be produced, and the film was not affected when a small body was dropped into it. When the solution has been exposed to the heat for a few hours the film becomes too thick to exhibit the experiment, and when cold the appearances described could not be produced. This experiment was referred to merely as illustrating the *appearances* of sun spots, and in no way as explaining their cause. At the same time it suggested the idea that if the luminous photosphere of the sun is formed of bodies, named by various observers, "willow leaves" (Nasmyth), "rice grains" (Stone), "bits of straw" (Dawes), and of the existence of distinct bodies of some kind on the visible surface of the sun there can now be very little doubt, they

may be floating in a fluid sufficiently dense to sustain them, but at the same time easily thrust aside by some disturbing cause below the surface. The existence of an external cause at the surface of the sun being improbable, may not the cause of the sun spots arise from a current or force, such as the "*red flames,*" which are supposed to be connected with the formation of spots? This force acting on the "willow leaves" would raise them from the level at which they may be supposed to float, they would slide under and over each other, and thus leave an opening; and, upon the gradual cessatiou of the disturbing cause, the tendency of the "willow leaves" would be to gradually assume their former positions and close up the spots in a way similar to the closing of the film in the simple experiment referred to.

Mr. Brothers also stated that, while observing the moon with his five-inch achromatic telescope at about eight o'clock on the evening of March 25th, he observed a small *dark* body cross the disc diagonally, from left to right, a little below the spot Copernicus. The motion was very rapid and similar to the passage of a luminous meteor across the field of view. He conceived it might be a meteoric body passing through space at a distance considerably beyond the limits of the earth's atmosphere.

Annual Meeting, April 17th, 1866.

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

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

The Council have again the satisfaction to report that although the balance of the Treasurer's account has been reduced from £360. 4s. 3d. on the 1st of April, 1865, to £269. 4s. 3d. on the 1st of April, 1866, the finances of the Society are nevertheless in a healthy state. Extra expenditure has been incurred during the past year for printing and publishing vol. 2, series 3, of the Society's Memoirs; binding books in the library; printing a catalogue of the Society's library; and purchasing new books, and also volumes to complete imperfect sets.

The number of ordinary members on the roll of the Society on the 1st of April, 1865, was 187. Five new members have been elected during the year; and the losses have been by deaths, six; resignations, one; and defaulters, two. The number of members on the roll on the 1st of April, 1866, was therefore 183. The deceased members are—Rev. Thos. Buckley, M.A., Mr. James Joseph Dean, Mr. John Parry, Sir Benjamin Heywood, Bart., F.R.S., Mr. John Whalley, and Mr. Richard Hampson.

Sir Benjamin Heywood, Bart., F.R.S., was one of the oldest members of the Society, having been elected on the 27th of January, 1815. He held the office of Treasurer for many years, and was one of the Trustees of the Society's

property. He took an active interest in local literary and scientific institutions, and was one of the founders of the Manchester Royal, and Mechanics', Institutions. He held the presidency of the latter institution from 1824 to 1841. In 1831–32 he represented the county in parliament, and whilst in London became an intimate associate of the prominent literary and scientific men of the time.

Mr. John Parry had been a member of the Society for 32 years, having been elected on the 26th of April, 1833. He was a constant attendant at the ordinary and sectional meetings of the Society, and took an active part in the formation of the Microscopical, Natural History, and Photographical Sections. He was the inventor of coloured signal lamps for railways, the first lamps of this kind having been made by the late Mr. Ford, of Cateaton-street, under his directions, and used on the Manchester and Leeds Railway. He had been forty-three years in the confidential employment of Messrs. Lockett and Co., the well known engravers to calico printers, and was one of the first to employ photography in connection with that business, in which he also introduced the electrotype process in 1839. His mechanical skill and ingenuity were displayed in the construction of a large solar microscope on a principle similar to that of the one exhibited at the Adelaide Gallery, London, about 1839 or 1840; in making a panoramic camera, camera bellows, apparatus for waxing paper, and for drying albumen plates; and in cutting and polishing sections of fossils for microscopic slides. He was an enthusiastic photographer, and had been very successful in producing enlarged photographs of microscopic objects. In the summer of last year he visited the coast of Galway, where he succeeded in finding a rich deposit of Foraminifera containing many new species, descriptions and lists of which have been given by Dr. Alcock in the Society's Proceedings.

The several Sections of the Society have continued to exhibit their usual activity, and have furnished many important

communications to the Proceedings, and to the Memoirs of the Society. The admission of Sectional Associates has again been found to work satisfactorily, and the Council have therefore resolved that the system shall remain in force during another year.

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

October 3rd, 1865.—"On the Internal Heat of the Earth as a Source of Motive Power," by Mr. George Greaves, M.R.C.S.

"Note on the Eclipse of the Sun, October 19th, 1865," by Mr. W. L. Dickinson.

October 5th, 1865.—"On Photographs of the Eclipse of the Moon, October 4th, 1865," by Mr. A. Brothers, F.R.A.S.

"On a Camera for outdoor work without a Tent," by Dr. J. P. Joule, F.R.S., &c.

October 12th, 1865.—"Rainfall at Eccles for 1864," by Mr. Thomas Mackereth, F.R.A.S.

October 16th, 1865.—"Notes on Atlantic Soundings," by Mr. Joseph Sidebotham.

"Notes on Acherontia atropos," by Mr. Joseph Sidebotham.

"Questions regarding the Life-History of the Foraminifera, suggested by Examinations of their Dead Shells," by Mr. Thomas Alcock, M.D.

October 17th, 1865.—"Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes, Part I.," by Mr. J. C. Dyer, V.P.

October 31st, 1865.—"On Cooling and Ventilating the Workings of deep Coal Mines," by Sir J. F. W. Herschel, Bart., M.A., D.C.L., F.R.S., &c.

"On Coresolvents," by the Hon. Chief Justice Cockle, M.A., &c., President of the Queensland Philosophical Society. Communicated by the Rev. Robert Harley, F.R.S., &c.

November 9th, 1865.—"Note on the Variable Star, S Delphini," by Mr. J. Baxendell, F.R.A.S.

November 14*th*, 1865.—"On a Predisposing Cause of the Cattle Disease," by Mr. Thomas Ainsworth.

"An Attempt to refer some Phenomena attending the Emission of Light to Mechanical Principles," by Mr. R. B. Clifton, M.A., Professor of Natural Philosophy in Owens College.

November 20*th*, 1865.—"On Collecting Foraminifera on the West Coast of Ireland," by Mr. John Parry.

"On the Illumination of Opaque Objects under the high powers of the Microscope," by Mr. J. B. Dancer, F.R.A.S.

November 28*th*, 1865.—"On the Amount of Carbonic Acid contained in the Air above the Irish Sea," by Mr. T. E. Thorpe, Assistant in the Private Laboratory, Owens College." Communicated by Professor H. E. Roscoe, F.R.S.

December 7*th*, 1865.—"On the November Meteors, as observed at Woodcroft, Cuckfield, Sussex, November 12–13, 1865," by Mr. George Knott, F.R.A.S. Communicated by Mr. Baxendell, F.R.A.S.

December 12*th*, 1865.—"Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes, Part II.," by Mr. J. C. Dyer, V.P.

December 14*th*, 1865.—"On the Application of Measuring Rods to Photographic Pictures," by Mr. Joseph Sidebotham.

"On Celestial Photography," by Mr. A. Brothers, F.R.A.S.

December 18*th*, 1865.—On late Improvements in Illuminating Opaque Objects under the higher Powers of the Microscope," by Mr. H. A. Hurst.

December 26*th*, 1865.—"On Fossil Wood found in Calcareous Nodules in the lower Coal Seams of Lancashire and Yorkshire," by Mr. E. W. Binney, F.R.S., &c.

January 4*th*, 1866.—Remarks on the Barometric Disturbances during the Months of October, November, and December, 1865," by Mr. G. V. Vernon, F.R.A.S.

"Rainfall for 1865 at Old Trafford," by Mr. G. V. Vernon, F.R.A.S.

"Note on the Variable Star, T Aquilæ," by Mr. J. Baxendell, F.R.A.S.

January 23rd, 1866.—"Notes on Section of Chat Moss, near Astley Station," by Mr. W. Brockbank.

January 29th, 1866.—"On the Humming-bird Hawk-moth," by Mr. Linton.

February 1st, 1866.—"Results of Rain-guage and Anemometer Observations made at Eccles, near Manchester, during the year 1865," by Mr. Thomas Mackareth, F.R.A.S.

"Note on the Variable Star, S Coronæ," by Mr. J. Baxendell, F.R.A.S.

"On the Determination of the Mean Form of the Light-curve of a Variable Star," by Mr. J. Baxendell, F.R.A.S.

February 6th, 1866.—"Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes, Part III.," by Mr. J. C. Dyer, V.P.

February 8th 1866.—"On the Supposed Photographs by Boulton and Watt," by Mr. Joseph Sidebotham.

"Speculations on the Process employed by Messrs. Boulton and Watt in the Production of the Pictures called by them Mechanical Pictures," by Mr. J. B. Dancer, F.R.A.S.

February 20th, 1866.—On a New Species of *Dadoxylon*, found in the Upper Foot Coal, near Oldham," by Mr. E. W. Binney, F.R.S., F.G.S.

"On Air from off the Atlantic, and from some London Law Courts," by Dr. R. Angus Smith, F.R.S., &c., President.

March 1st, 1866.—"On the Variable Star, R Vulpeculæ," by Mr. G. Knott, F.R.A.S. Communicated by Mr. J. Baxendell, F.R.A.S.

"On the Fall of Rain during the Different Hours of the Day, as deduced from a Series of Observations made by the Rev. J. C. Bates, M.A., F.R.A.S., at St. Martin's Parsonage, Castleton Moor," by Mr. J. Baxendell, F.R.A.S.

March 6th, 1866.—"Note on the Purification of Platinum," by Mr. E. Sonstadt.

"On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties," by Messrs. E. Hull, F.G.S., and William Brockbank.

March 8th, 1866.—"On the Pantascopic Camera." by Mr. J. R. Johnson. Communicated by Mr. E. C. Buxton.

March 20th, 1866.—"On a New Balance," by Dr. J. P. Joule, F.R.S., &c.

"Notes on Cotton Spinning Machinery, Part II., Roving Frames," by Mr. J. C. Dyer, V.P.

March 26th, 1866.—"On Embryonic Shells of Mollusca, collected from Dogs Bay Sand," by Dr. Thomas Alcock.

March 29th, 1866.—"Results of Rain-gauge and Anemometer Observations made during the year 1865, at St. Martin's Parsonage, Castleton Moor," by the Rev. J. C. Bates, M.A., F.R.A.S. Communicated by Mr. J. Baxendell, F.R.A.S.

"On the Eclipse of the Sun, October 8th, 1866; and on the Occultations of Aldebaran by the Moon, September 28th, and November 22nd, 1866," by Mr. W. L. Dickinson.

"On a Method of Imitating the Appearances of Sun Spots," by Mr. A. Brothers, F.R.A.S.

April 3rd, 1866.—"On the Humming-bird Hawk-moth," by Mr. E. W. Binney, F.R.S., &c.

"On a Logical Abacus," by Mr. W. S. Jevons, M.A.

The Council desire to express their sense of the value of the services rendered to the Society by the Honorary Librarian, Thomas Windsor, Esq., M.R.C.S., in arranging the Society's library, and making out and printing a catalogue of the books; and they regret that he is unable to continue his services. At his request Mr. Charles Bailey has, for some months past, ably discharged the duties of Librarian.

The Committee appointed to discuss the question of the medal have agreed upon certain resolutions concerning the mode of award of the Society's medal; but no award has yet been made.

The Librarian reports that since the last annual meeting a catalogue of the books in the library has been printed, and that arrangements are in progress for printing a supplementary catalogue of the books received since that time. Vol. 2,

series 3, of the Society's Memoirs, and vols. 3 and 4 of the Proceedings, have been bound, and are now being forwarded to the Societies and Institutions with which this Society exchanges publications. Many volumes have been bound during the year, and several purchases of books to complete sets have been made. The general condition of the books is satisfactory.

On the motion of Mr. TRAPP, seconded by Mr. B. ST. J. B. JOULE, the Annual Report was unanimously adopted.

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

President.

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

Vice-Presidents.

ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S., &c.
JAMES PRESCOTT JOULE, LL.D., F.R.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.

Secretaries.

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

Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

Librarian.

CHARLES BAILEY.

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GEORGE VENABLES VERNON, F.R.A.S., M.B.M.S.
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JOHN BENJAMIN DANCER, F.R.A.S.
MURRAY GLADSTONE, F.R.A.S.

ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

FROM 31st MARCH, 1865, TO 31st MARCH, 1866.

Dr.

Date	Particulars		£ s. d.	£ s. d.
1865. April 1.	To Balance in the Bank of Heywood Brothers and Co.		335 0 3	
" "	" Balance in Treasurer's hands		25 4 0	360 4 3
1866. Mar 31.	To *Members' Contributions*:			
	Arrears, 1864-5 ... 4 ..		8 8 0	
	" Half Subscription, 1865 ... 1 ..		1 1 0	
	" Admission Fee, 1865 ... 1 ..		2 2 0	
	Members on the Roll, April 1, 1865, 187, at 42s.	£392 14 0		
	Deduct Compounders ... 3			
	" Dead ... 3			
	" In Arrear, March 31, 1866 ... 22	58 16 0		
	28		333 18 0	
	Four Members elected in 1865, April—Dec.	£8 8 0		
	Deduct in Arrear ... 2 ..	4 4 0	4 4 0	
	One Member elected in 1866, half-subscription	1 1 0		
	Deduct in Arrear	1 1 0		
	Two Admission Fees received, 1865-6		4 4 0	353 17 0
	Ten Associates of Microscopical Section, at 10s. 0d.		5 0 0	
	Ten Associates of Photographical Section, at 10s. 6d.		5 5 0	
	One Associate of Physical and Mathematical Section, at 10s. 6d.		0 10 6	10 15 6
" "	To *Sale of Publications*:			
	Memoirs—Ser. 3, Vol. 1		1 1 0	
	" Ser. 3, Vol. 2		5 15 0	
	" Ser. 2, Vol. XI.		0 4 0	
	" Ser. 2, Vol. XII.		0 5 0	
	Proceedings		1 2 6	
	Memoir of Dalton		0 5 3	8 12 9
" "	To *Sundry Income*:			
	Sec. Contrib. Microscopical	£4 4 0		
	" " Photographical	2 2 0		
	" " Physical and Mathematical	2 2 0	8 8 0	
" "	Interest allowed by Bankers		11 12 0	20 0 0
				£753 9 6

Composition Fund	£72 10 0
General Balance	196 14 3
	£269 4 3

Cr.

Date	Particulars		£ s. d.	£ s. d.
March 31, 1866.	By *Charges on Property*:			
	Chief Rent		12 13 7	
	Fire Insurance		5 12 6	
	Property Tax		2 16 8	21 2 9
"	*House Expenditure*:			
	Water, Gas, Candles, and Coal		23 10 2	
	Cleaning and Petty Expenses		2 2 10	
	Repairs and Alterations		15 15 9	
	Tea and Coffee at meetings		17 6 0	58 14 9
"	*Administrative Charges*:			
	Wages—Keeper of Rooms	£42 12 0		
	" —Assistant ditto	1 10 0	44 2 0	
	Attendance on Sections		8 8 0	
	Postage and Parcels		16 18 9	
	Stationery, Printing Circulars, &c., and Receipt Stamps		9 10 0	78 18 9
"	*Publishing*:			
	Memoirs—Printing, Engraving, &c.		103 7 0	
	Printing Proceedings		43 14 6	
	Editor of Memoirs and Proceedings		50 0 0	197 1 6
"	*Library*:			
	Periodicals, Binding, &c.		71 4 11	
	Executor of E. F. Ekman, for Books, &c.		35 12 1	
	Printing Catalogue of Library		17 10 0	
	Subscription to Ray Society		1 1 0	
	" Palæontographical Society		1 1 0	126 9 0
"	*Sundry Expenditure*:			
	Solicitors for Preparing Bond from Keeper of Rooms			1 18 6
	Total Disbursements			484 5 3
	Balance in Bank of Heywood Brothers and Co.		267 2 3	
	" Treasurer's hands		2 2 0	269 4 3
				£753 9 6

2nd April, 1866.

ROBERT WORTHINGTON, Treasurer.

Examined and found correct, 10th April, 1866.

GEO. CLIFF LOWE, } Auditors.
WM. BROCKBANK, }

A paper was read "On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes, Part I," by James Nasmyth, C.E., Corresponding Member of the Society.

In this, the first part of his paper, the author describes in considerable detail the methods and processes by which he produces speculum metal of the best quality, and casts, anneals, and rough-grinds a speculum of ten inches in diameter, his descriptions being illustrated by drawings of the apparatus he employs. In the second part of the paper he will describe the processes of fine-grinding, polishing, and figuring; and will give directions for the general management and use of reflecting telescopes. His instructions are based upon the results of thirty years' experience in the art of working specula, and will, he believes, enable any zealous amateur to make for himself, at a moderate cost, a really good and useful reflector.

PHOTOGRAPHICAL SECTION.

April 12th, 1866.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

A paper was read entitled "Note on the First Use of Hyposulphite of Soda in Photography," by A. Brothers, F.R.A.S.

During an investigation into the early history of photography, I met with the statement that Daguerre used hyposulphite of soda in his process for fixing the pictures, and also that in Mr. Talbot's patent the use of that substance was included. I was under the impression that Sir John Herschel had pointed out that hyposulphite of soda would fix the photographic image, but was unable to ascertain where or when the discovery was first published. In order to determine this point I wrote to Sir John Herschel, requesting him to inform me whether the discovery was his, and the date when it was published. To these questions I received the following reply:—

Colingwood, October 29, 1864.

Sir,

I think I may very fairly claim the discovery of the hyposulphites as fixing agents, as I believe I was the first to call the attention of chemists to that class of salts and their peculiar habitudes, especially in relation to the insoluble salts of silver.

In my paper "On the Hyposulphurous Acid and its Compounds," which bears date Jan. 8, 1819, and which appeared in Brewster and Jamieson's *Edinb. Phil. Journal*, 1819, occur these words:—

"One of the most singular characters of the hyposulphites is the property their solutions possess of dissolving muriate of silver and retaining it in considerable quantities in permanent solution." (page 11.)

"*Hyposulphite of potash.*—It dissolves muriate of silver even when very dilute, with great readiness." (p. 19.)

"*Hyposulphite of soda.* . . . . . . Muriate of silver newly precipitated dissolves in this salt when in a somewhat concentrated solution in large quantity and almost as readily as sugar in water." (p. 19.)

"*Hyposulphite of strontia.* . . . . . Like the rest of the hyposulphites it readily dissolves muriate of silver, and alcohol precipitates it as a sweet syrup." (p. 21.)

'*Hyposulphite of silver.*—Muriate of silver newly precipitated is soluble in all liquid hyposulphites, and, as before observed, in that of soda with great ease and in large quantities. This solution is not accomplished without mutual decomposition, as its intense sweetness proves—a sweetness surpassing that of honey, and diffusing itself over the whole mouth and fauces, without any disagreeable or metallic flavour." (p. 27.)

In a second paper on the same subject, which appeared in the same jonrnal, vol. 1, p. 396 *et seq.*, it is shown (*inter alia*) that the affinity of this acid for silver is such that oxide of silver readily decomposes hyposulphite of soda and likewise the soda in a caustic state, "the only instance, I believe, yet known of the direct displacement of a fixed alkali *viâ humidâ* by a metallic oxide." (p. 397.)

"*Hyposulphite of ammonia and silver.*—Its sweetness is unmixed with any other flavour, and so intense as to cause pain in the throat . . . . . one grain communicates a perceptible sweetness to 30,000 grs. of water." (p. 399.)

In a third communication, dated November, 1819—"The habitudes of this acid with the oxide of mercury are not less singular than its relation to that of silver."—"The red oxide is readily dissolved by . . . . , hyposulphite of soda, while the alkali is set at liberty in a caustic state," &c. &c.

The very remarkable facts above described I have reason to believe attracted a good deal of attention at the time, and thenceforward the ready solubility of silver salts, usually regarded as insoluble, by the hyposulphites was familiar to every chemist. It would not therefore be surprising if Daguerre tried it to *fix* his plates (*i.e.* to wash off the iodide coating); but I have been informed, though I cannot cite a printed authority for it, that at first he fixed with ammonia, or with a strong solution of common salt.

For my own part the use of the hyposulphites was to myself the readiest and most obvious means of procedure, and presented itself at once. My earliest experiments were made in January, 1839, and in my notebook I find:—

"Exp. 1012.—1839, Jan. 29. Experiments tried within the

last few days, since hearing of Daguerre's *secret*, and also that Fox Talbot has got something of the same kind." . . . . . . [Here follow some trials of the relative sensitiveness of the nitrate, carbonate, acetate, and muriate of silver. I should observe that at that time I did not even know what kind of pictures Daguerre had produced. This process was not revealed till August, 1839.]

"Exp. 1013.—Daguerre's process—attempt to imitate. Requisites—1st very susceptible paper—2nd very perfect camera—3rd means of arresting further action. Tried hyposulphite of soda to arrest the action of light by washing away all the chloride of silver or other silvering salt—succeeds perfectly. Papers half acted on, half guarded from the light by covering with pasteboard, were withdrawn from sunshine, sponged over with hyposulphite, then washed in pure water, dried, and again exposed. The darkened half remained dark, the white half white, after any exposure, as if they had been painted with sepia."

"Jan. 30, 1839.—Formed image of telescope with the aplanatic lens . . . . and placed in focus paper with carbonate of silver. An image was formed in white on a sepia colored ground . . . . which bore washing with hyposulphite of soda, and was then no longer alterable by light. Thus Daguerre's problem is so far solved," &c. &c.

"Exp. 1014.—Jan. 30. Tried transfer of print and copper plate engraved letters," &c. &c.

The publication of Daguerre's process (according to Dr. Monckhoven, for I cannot refer at present to the original document) took place on the 19th August, 1839. My early experiments printed in the notices of the proceedings of the Royal Society of March 14, 1839, in which occurs this passage in the abstract of a paper read to the Society : —

"Confining his attention in the present notice to the employment of chloride of silver, the author enquires into the method by which the blackened traces can be preserved, which may be effected, he observes, by the application of any liquid capable of dissolving and washing off the unchanged chloride, but leaving the reduced oxide of silver untouched. These conditions are best fulfilled by the liquid hyposulphites."

"Twenty-three specimens of photographs made by Sir J. Herschel accompany his paper—one a sketch of his telescope at Slough, fixed from its image in a lens."

This is the image above mentioned as having been taken on Jan. 30, 1839—and was, I believe, the first picture ever fixed from an optical image ever taken in this country—at least I have heard of none earlier.

At the time of making these experiments, as already mentioned, I had no knowledge of M. Daguerre's process further than the mention of the existence of *a process* (a secret one) in a note from Admiral (then Captain) Beaufort some time about Jan. 23, 1839. Of course I used *paper*, not silver—and it was not a *suggestion*, but a regular and uniform *practice* to use the hyposulphite—I never used anything else.

I am, Sir, your obt. servt.,

J. F. W. HERSCHEL.

In reference to the subject of *fixing* the photographic image I find the following passage in a paper read before the Royal Society on January 31, 1839, by Mr. Talbot. After referring to the improvements of Wedgwood and Davy in 1802, and the difficulties they found in making the paper sufficiently sensible to receive the impression in a camera obscura, and their inability to *fix* the pictures, the author states that "his experiments were begun without his being aware of this prior attempt; and that in the course of them he discovered methods of overcoming the two difficulties above related. With respect to the latter he says that he has found it possible by a subsequent process so to fix the images or shadows formed by the solar rays that they become insensible to light . . . . . . and states that he has exposed some of his pictures to the sunshine for the space of an hour without injury."

In the abstract of the paper given in the Proceedings the method adopted for fixing the image is not stated; but in a paper read before the same Society on February 21 of the

same year, it is stated that the prints were fixed in a weak solution of *iodide of potassium*. Ammonia had been tried, but not very successfully, but the method preferred was a strong solution of common salt. It will be seen, therefore, that up to the date of the publication of Sir John Herschel's paper hyposulphite of soda had not been used in photography excepting by himself.

PHYSICAL AND MATHEMATICAL SECTION.

April 26th, 1866.

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

A paper entitled "Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of the Mensuræ Micrometricæ and the Bonner Sternverzeichniss," by GEORGE KNOTT, Esq., F.R.A.S., was communicated by Mr. BAXENDELL.

The question of the relation between the magnitude scales of different observers is one of considerable interest and importance, and has engaged the attention of several able astronomers. It is not my intention in this paper to go into the subject generally, but simply to submit the results of a comparison of the magnitudes of the late Admiral Smyth's *Bedford Catalogue* with those assigned to the same stars by the late Prof. Struve in the *Mensuræ Micrometricæ,* and by Dr. Argelander in the *Bonner Sternverzeichniss.* I may remark that I was induced to undertake the comparison by the perusal of an interesting paper by Mr. Pogson, the present Director of the Royal Observatory at Madras, in vol. xvii. of the Monthly Notices of the Royal Astronomical Society, in which he has given a short table of corresponding magnitudes of Struve, Bond, Herschel, and Smyth, as referred to

what he terms the *standard scale*, or Argelander's extended to the lower magnitudes by the adopted light-ratio of 2·512. The comparison was facilitated by the simplicity of the notation in the Bedford Catalogue (whole and half magnitudes only being employed), and in the annexed table will be found the corresponding magnitudes of the three observers, together with the number of comparisons on which each result depends.

TABLE I.

Mag. Sm.	Mag. Σ.	No. of Obs.	Mag. A.	No. of Obs.
1	1·3	5	1·3	7
1·5	1·5	1	1·4	3
2	2·0	3	2·1	13
2·5	2·6	2	2·3	12
3	2·9	12	3·0	36
3·5	3·3	14	3·4	22
4	3·8	27	3·9	28
4·5	4·3	19	4·4	16
5	4·8	40	4·9	35
5·5	5·1	41	5·3	33
6	5·8	87	5·9	59
6·5	6·2	45	6·3	27
7	6·5	81	6·9	37
7·5	6·9	79	7·3	23
8	7·4	101	8·1	47
8·5	7·8	85	8·5	20
9	8·4	75	9·0	34
9·5	8·8	21	9·2	6
10	9·4	25	9·4	6
10·5	9·7	5		
11	10·0	24		
12	10·2	13		
13	10·7	20		
14	10·3	6		
15	11·0	8		
16	10·9	3		

Treating the numbers in the preceding table in accordance with Sir John Herschel's formula (Mem. R.A.S., vol. iii., p. 179),

$$M = \left\{ \frac{m_1 n_1}{S_1} + \frac{m\,n}{S} + \frac{m^1 n^1}{S^1} \right\} \cdot \frac{S}{n_1 + n + n^1}$$

where S_1 S S^1 represent any three consecutive numbers in column 1, m_1 m m^1 the corresponding numbers in columns 2 or 4, and n_1 n n^1 the corresponding number of observations in each case, we obtain the following table of equalised results.

TABLE II.

Mag. Sm.	Mag. Σ.	No. of Obs.	Mag. A.	No. of Obs.
1	1·30	5	1·30	7
1·5	1·76	9	1·67	23
2	2·03	6	1·96	28
2·5	2·45	17	2·49	61
3	2·89	28	2·94	70
3·5	3·33	53	3·44	86
4	3·79	60	3·98	66
4·5	4·29	86	4·40	79
5	4·73	100	4·87	84
5·5	5·25	168	5·38	127
6	5·72	173	5·85	119
6·5	6·27	213	6·37	123
7	6·52	205	6·84	87
7·5	6·94	261	7·48	107
8	7·39	265	8·00	90
8·5	7·86	261	8·55	101
9	8·33	181	8·97	60
9·5	8·86	121	9·39	46
10	9·33	51	9·40	6
10·5	9·74	54		
11	10·00	42		
12	10·39	57		
13	10·66	39		
14	10·85	34		
15	10·88	17		
16	10·90	3		

An examination of the tables suggests the following remarks. The scale of the *Bedford Catalogue*—(it should be borne in mind that the mag. of the principal star in each

case was adopted from Piazzi)—and that of the *Bonner Sternverzeichniss* may be regarded as practically identical down to the 9th magnitude.* Assuming with Mr. Pogson that the 13th mag. of Argelander's scale corresponds with the 16th mag. of that of Admiral Smyth, we may tabulate the mags. below the 9th thus:—

Mag. Sm.	Mag. A.
10	9·6
11	10·1
12	10·7
13	11·3
14	11·9
15	12·4
16	13·0

I have not been able fully to verify these results; but the examination of eight stars marked by Sm. as of the 13th mag. on three different occasions gave 11·2 mag. as the corresponding mean mag. on Argelander's scale, presenting a satisfactory accordance with the tabulated magnitude.

The coincidence between the scale of the *Bedford Catalogue* and that of the *Mensuræ Micrometricæ* ceases practically with the naked eye magnitudes. At the 6th mag. the scales diverge, and the 11th mag. of the one corresponds with the 10th mag. of the other. It will be observed that the five magnitudes of Admiral Smyth's scale below the 11th are represented by only one magnitude in the scale of Professor Struve.

In conclusion I may just notice that by a slight oversight Mr. Pogson has, in the table to which I have already referred, assumed the 12th mag. of Struve's scale as the limit of vision for the Poulkowa refractor of 14·93 inches aperture; that mag. was however assigned by Prof. Struve as the limit for

* Mr. Pogson has assumed the 8th mag. as the point of divergence of the two scales.

the Dorpat telescope of 9·6 inches aperture, the calculated limit for the larger telescope, as given in the Poulkowa Catalogue of 1843, being 12·89 mag.

Mr. Baxendell drew attention to Sir John Herschel's "scarlet, almost blood-coloured star," immediately following *α* Crateris, and stated that, from observations made during the spring of last year, and again during the present month, he had found that this remarkable object belongs to the list of variable stars. Its present brightness is fully three-fourths of a magnitude less than that observed last spring; but this probably does not represent the extreme range of variation to which it is subject. In conformity with Prof. Argelander's system of nomenclature this new variable will be designated R Crateris. Its mean place for 1865·0 is R.A. $10^{h.}$ $53^{m.}$ $55{\cdot}4^{s.}$; Dec. 17° 36′ 3″ S.

PHOTOGRAPHICAL SECTION.

Annual Meeting, May 15th, 1866.

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

A note, by Mr. J. W. Dancer, on the use of Lager Beer in Photography, was read by the Secretary.

The Annual Report and Treasurer's Account were submitted to the meeting and unanimously passed; and the following Officers were elected for the ensuing year:—

President.
THE RIGHT REVEREND THE LORD BISHOP OF MANCHESTER.

Vice-Presidents.
J. P. JOULE, LL.D., F.R.S., &c.
H. E. ROSCOE, Ph.D., F.R.S., &c.
JOSEPH BAXENDELL, F.R.A.S.

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

Treasurer.
THOMAS H. NEVILL.

Of the Council.
JOHN B. DANCER, F.R.A.S.
E. C. BUXTON.
JOHN ROGERSON.
JOSEPH SIDEBOTHAM.
SAMUEL COTTAM.
LESLIE J. MONTEFIORE.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

Annual Meeting, June 4th, 1866.

A. G. Latham, Esq., President of the Section, in the Chair.

The Annual Report of the Council was read and unanimously adopted.

The following Gentlemen were elected Officers of the Section for the ensuing year:—

President.
ARTHUR G. LATHAM.

Vice-Presidents.
JOSEPH SIDEBOTHAM.
JOHN B. DANCER, F.R.A.S.
WILLIAM HENRY HEYS.

Treasurer.
THOMAS H. NEVILL.

Secretaries.
HENRY ALEXANDER HURST.
THOMAS ALCOCK, M.D.

Of the Council.
JOSEPH BAXENDELL, F.R.A.S.
PROF. W. C. WILLIAMSON, F.R.S., &c.
JOHN WATSON.
THOMAS COWARD.
R. D. DARBISHIRE, F.G.S.
ROBERT WORTHINGTON, F.R.A.S.
JOHN LEIGH.
SAMUEL COTTAM.

The President exhibited two beetles, male and female, nearly allied to Rhynchophorus Palmarum; also specimens

of the very curious case formed by the larva from vegetable fibres, in which the change to the perfect insect takes place. He said that "both the larva and the insect itself are considered great delicacies by the Indians of South America, where it is found. The gizzard of the beetle, shown with the microscope and illustrated by a drawing, is divided into eight oval sections each surrounded by a strong rib strengthened by a central longitudinal rib, which is connected with the margin by powerful muscles, and from the marginal rib projects a fringe of scimitar-shaped cilia, the whole forming a most beautiful and powerful apparatus for the preparation of the food for digestion. A large number of parasites exist on the insect, and these are found congregated in groups of four or five with their tails entwined." Specimens and a drawing were shown.